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THE

BRITISH AND FOREIGN

MEDICAL REVIEW,

FOR JANUARY, 1846.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

1. *The Sixth Annual Report of the Registrar-General of Births, Deaths, and Marriages, in England,* 1845.
2. *Contributions to Vital Statistics: being a Development of the Rate of Mortality and the Laws of Sickness; from original and extensive Data, procured from Friendly Societies, showing the Instability of Friendly Societies, "Odd Fellows," "Rechabites," &c., with an Inquiry into the Influence of Locality on Health.* By F. G. P. Neison, F.L.S., &c., Actuary to the Medical Invalid and General Life Office. *Read before the Statistical Society, March* 17, 1845.
3. *Facts connected with the Social and Sanitary Condition of the Working Classes in the City of Dublin, with Tables of Sickness, Medical Attendance, Deaths, Expectation of Life, &c. &c.; together with some Gleanings from the Census Returns of* 1841. By Thomas Willis, F.S.S. *Dublin,* 1845.
4. *On the Duration of Life among the Families of the Peerage and Baronetage of the United Kingdom.* By William A. Guy, M.B. Cantab., &c., &c. (*Extracted from the Journal of the Statistical Society for March* 1845.)

It is always an agreeable task to pass under review a periodical publication which bears on the face of it evidence of a continual striving after progress and improvement. This pleasure is provided for us year by year in the Reports of the Registrar-General. The one which is now before us is the largest, and, in many respects, the most interesting and important of the six which have issued from Somerset House. It contains, in addition to the usual Report and detailed abstracts of births, deaths, and marriages in England and Wales, a series of foreign official returns, of which the greater part was procured for the Registrar-General by the Earl of

Aberdeen (at the instance of Sir James Graham,) through the Foreign or British Ministers, and the remainder from Dr. Elliott of Stockholm, M. Hoffman of Berlin, and M. Moreau de Jonnes of Paris. Returns have been obtained from Sweden, Norway, Denmark, Russia, Austria, Prussia, Saxony, Hanover, Frankfort, Bavaria, Wurtemburg, Netherlands, Belgium, France, Portugal, and the United States of America. We shall presently have occasion to notice some of the results obtained from these sources.

After the abstracts for England and the several continental returns, which together occupy 500 closely-printed octavo pages, we are favored with a series of very valuable dissertations by Mr. Farr, headed respectively "Public Health," "Summary of results deduced from the English Life Table," "On the properties and applications of the New Life Table, and an account of some improvements in its form," "The *annual mortality* and the *mean age at death*," "English Joint-Life Table," "Life Insurance," &c. &c. These dissertations occupy upwards of 150 pages, and abound in tables, which must be of the first importance to the actuary.

These tables are not of a nature to admit of analysis in this place; we must, therefore, content ourselves with this reference to them, reserving only certain parts of the essays, that entitled "the annual mortality, and the mean age at death," among the number, for criticism in connexion with Mr. Neison's valuable "Contributions to Vital Statistics." We cannot, however, refrain from pointing, in passing, to the valuable table at p. 655 of the Report, which, for the first time, presents us with the mean age at death of the inhabitants of England living at every age, and contains other useful columns. Some interesting applications of this table will be found at p. 661 of the 8vo edition of the Report.

There were registered, in England and Wales, in the year 1842, 118,825 marriages, 517,739 births, and 349,519 deaths, being an excess of births over deaths of 168,220. Taking the average of four years, 1839-1842, 1 person in 130 was married, 1 in 45 died, and 1 child was born to 31 persons living. The marriages have decreased during that period from 1 in 126 to 1 in 136, the births have increased up to 1841, and the deaths were highest in 1840, and lowest in 1841. The years 1841 and 1842 have exhibited little difference in the relative number either of births or deaths: 1 in 64 of the male population, and 1 in 66 of the female population married annually; there was 1 birth to 15 males and to 16 females living; and the mortality of males was 1 in 44, and of females 1 in 47, or, to be more accurate, 2·294 and 2·124 per cent.

An interesting comparison is instituted between the marriages, births, and deaths in England and the four principal European states: *France, Prussia, Austria,* and *Russia*. It appears that during the last few years to which the returns refer, there have been fewer marriages in England than in any of the larger European states. While in England there is only 1 marriage in 131 persons living, there is 1 in 124 in Austria, 1 in 121 in France, 1 in 113 in Prussia, and 1 in 99 in Russia. The births, as might be anticipated, are also in defect in England. The relative numbers being as follows: England 1 birth in 35 persons living, Prussia 1 in 27, Austria 1 in 26, Russia 1 in 23, France alone presenting the equal ratio of 1 in 35, a conclusive proof that in France there are fewer children to a marriage than in England.

The number of illegitimate children registered in England and Wales in 1842 was 34,796; of these a somewhat larger proportion were males than in the case of legitimate births, for it appears that while among legitimate children there were 20 boys to 19 girls born alive, among illegitimate children there were 21 to 20. In all England there were 6·7 illegitimate births in 100 births, and it is not a little remarkable that nearly all the large towns stand below this average. The highest proportion anywhere observed is 18 per cent., the lowest somewhat above 1 per cent. The proportion of illegitimate children varies not less remarkably in different nations, for while Sardinia has so low a proportion as 2 in a 100, Bavaria has no less than 20 in the 100. Of 13 European states England takes the fourth place in regard to continence. The returns for the continental states confirm the well-known fact of the high mortality of illegitimate children. Thus, in Saxony, while 464 in 10,000 legitimate children were still-born, 616 in the same number of illegitimate children were still-births. The deaths during the first year in Saxony, Sweden, and Stockholm respectively, were 26, 16, and 26 per cent. of legitimate to 34, 27, and 40 illegitimate.

It now only remains to speak of the mortality of England, as compared with the continental states. The average annual mortality of the English population in the five years, 1838-42 was 2·209 per cent., or nearly 1 in 45; but in 1842 it was 2·167, or nearly 1 in 46. This is a more favorable rate of mortality than that which prevails in the chief European states. In France it is 1 in 42, in Prussia 1 in 38, in Austria 1 in 33, and in Russia 1 in 28.

The result of the registration for 1842, then, as compared with former years, and the movement of the English population as compared with that of the principal states of Europe, appears on the whole to be satisfactory. It is clear that we possess advantages which it is our duty and our interest to improve to the utmost. It will not do to compare ourselves only with our neighbours; we must take a much higher standard than that which any continental state affords, and be satisfied for the whole country with nothing short of the rate of mortality of its most favoured divisions. In the South-western and South-eastern divisions the mortality is stated at 1 in 52, and in no less than four counties (Sussex, Dorsetshire, Devonshire, and North Wales,) it is as low as 1 in 54. The time will, we trust, arrive when the entire country shall boast as low a mortality. In the mean while let it be our standard of excellence.

We have still to notice a valuable return of violent deaths and suicides registered in 1840. They amount to no less than 10,881, of which 900 were cases of suicide, and 65 were murders. If we take the instrument or means of death employed by suicides, the following will be the order of their frequency: hanging, strangling, and suffocation, 381; poisons 161; wounds 129; drowning 107; gun-shot wounds 45; leaps from heights 18; unascertained 60. Of the cases of suicides by poison 26 were by arsenic, 19 by opium, 3 by oxalic acid, and 113 by other poisons. Among the accidental deaths the following are deserving of notice: overdose of opium or laudanum, given or taken by mistake, 39; oil of vitriol, given or taken by mistake, 15; drinking boiling water (children,) 23; making in all 77 accidental deaths from these three causes.

Having now given our usual abstract of the Registrar-General's Report, as far as it refers to the number of births, deaths, and marriages, we proceed to present our readers with some account of the other works which stand at the head of the present article, especially of the very valuable and highly interesting publication of Mr. Neison.

We have already introduced Mr. Neison to our readers in an article on the "Vital Statistics of England and Wales;"* and we then took occasion to express our dissent from the extreme views which he had adopted. We regret that he has imposed the same duty upon us on the present occasion; but ere we enter upon the discharge of it, we must express the strong sense we entertain of the value of Mr. Neison's labours. His facts and calculations may be implicitly relied on, though his conclusions are sometimes open to objection; and his liberality in stimulating and rewarding his contributors by prizes from his own purse must not pass without notice. The "contributions" too, consisting, to so great an extent, of tables, must have been published at great cost, and with little hope of a pecuniary return; so that on the whole, Mr. Neison has placed all who may profit by his labours under many and great obligations. The members of friendly societies, "Odd Fellows," "Rechabites," &c. must own themselves, in a peculiar manner, his debtors; and they will do well to listen to Mr. Neison's warning, ere it be too late.

The "contributions" consist of two parts, of which the first treats of the duration of life among different classes, and the second of the prevalence of sickness among the members of benefit societies.

The first part comprises several life-tables, which Mr. Neison uses as the foundations on which to build his deductions. By throwing his facts into this form he has obtained by far the most satisfactory data, and has avoided the error which he himself pointed out in an essay formerly referred to, of instituting comparisons from which an essential element (the ages of the living,) was omitted. On the superiority of life-tables to all other data for determining the sanitary state of nations, or classes of persons, Mr. Farr makes some very just observations. In the chapter of the 'Report,' headed "the annual mortality" and "the mean age at death," he proves satisfactorily that the mean age of the living has always been esteemed by the highest authorities as a necessary element in every just comparison of class with class and country with country; and he demonstrates the necessity of taking this circumstance into account, by comparing the expectation of life, as calculated from the ages both of the living and the dying, with the average age at death, and the number of living out of which one death takes place annually. From this comparison it results that there is a much nearer correspondence between the expectation of life and the mortality, than between the expectation of life and the average age at death. Thus, if we compare England, France, and Sweden, we find that they hold the same relation to each other in regard both to the expectation of life and the mortality, the order being England, France, Sweden; but in respect of the average age at death the three countries occupy a different position: France having the highest average age at death, Sweden holding the second rank, while England stands at the bottom of the scale. So also if we compare England, Surrey, the Metro-

* British and Foreign Medical Review, No. XXXV, p. 198.

polis, and Liverpool, there is the same correspondence between the expectation of life, as shown by the life-tables, and the mortality, while the mean age at death in England and the Metropolis is precisely the same, viz., 29 years, though the expectation of life for the former is 41 years, and for the latter only 37 years.

The necessity of employing life-tables as our tests of the sanitary condition of different classes, and of the inhabitants of different localities, is further illustrated by the anomalous results obtained by employing only the mean age at death. According to this latter method, Greenwich is the healthiest district of the Metropolis, for the obvious reason, that there is an accumulation of old men at Greenwich Hospital, who, dying at advanced ages, make the mean age at death high. By using the same one-sided test, Rotherhithe is made to appear more healthy than Islington, Marylebone, and Pancras; and Whitechapel takes a more favorable sanitary position than St. James's district and the city of London.

In adopting the life-table, therefore, as his test of the sanitary condition of the classes which he makes the subjects of comparison, Mr. Neison has made choice of the only perfectly unobjectionable standard; and we shall willingly admit that his facts and tests are unexceptionable, though, as we have already hinted, we shall not be quite so easily satisfied with his arguments.

Mr. Neison first provides himself with a standard of comparison in the shape of a life-table of England and Wales, founded on the census for 1841, compared with the deaths as given in the 2d, 3d, 4th, and 5th Reports of the Registrar-General. Our readers may recollect that Mr. Farr's life-table was calculated on the mortality of the year 1841 alone; it is evident, therefore, that the average employed by Mr. Neison must form "a broader and more satisfactory basis on which to found a measure of the duration of life in this country." On comparing the two life-tables with each other, Mr. Neison's table will be found to give a higher expectation, both for males and females, throughout the whole of life. At 60 years of age the difference amounts for the male to one year, and for the female to somewhat more than a year. It is somewhat singular that the difference between Mr. Neison's table and the Carlisle table, is, taking one period of life with another, still less than that which exists between the two tables just compared.

A standard of comparison thus obtained, our author proceeds to develope his views, and to reveal his scepticism with regard to the "supposed influence of locality on the duration of life;" and he justly observes that hitherto no public means have been employed to solve this question.

"For the progress of vital statistics it unfortunately happens, that the public records of this country are kept with very little regard to method or unity of plan. The Report of the Census may certainly in itself be regarded as a very complete document; and perhaps no other country possesses such excellent mortuary registers; yet for almost every purpose of exact calculation, both documents are nearly useless. No two things should have been more intimately related in design and classification, than the census of the people and the registration of deaths. Still they seem to have been compiled without any regard to each other. For example, if it were required to compare any two counties in England—a manufacturing with an agricultural county—an inland with a coasting county—in order to determine the relative value of life in the respective populations, it cannot at the

present time be done. The Report of the Census Commissioners gives the population for those counties; but on reference to the Reports of the Registrar-General, it is found that the deaths are given for quite a different arrangement of districts. Again, if it be required to compare one district of the Registrar-General with another, the same kind of difficulty arises; for, on turning to the Census Report, those districts are in no way recognized. Precisely the same want in unity of plan is to be regretted in respect to the town districts of England, the districts of Census Commissioners constantly differing from those adopted by the Registrar-General."

Were these difficulties overcome—and no expense ought to be spared by Government to bring these two orders of reports into strict correspondence—another inquiry would still remain to be made before we could determine the true influence of locality on health and the duration of life, namely, an inquiry into the influence of employment upon health.

Not that this subject has been altogether overlooked, as the works of Thackray, Dr. Holland of Sheffield, and the recent essays of Dr. Guy, in the Statistical Journal, to say nothing of the older writers, sufficiently testify; but in some, at least, of these inquiries the want of the important element of the ages of the living, following the several occupations, has been felt and acknowledged, and till that want is supplied, we are not in a position to speak very decidedly as to the influence of employment upon health. We are not, therefore, disposed to gainsay our author's position, that "at present it is right to assume, that either employment or occupation, condition in life, or rank in society, poverty or riches, has as direct an influence on the duration of life, as peculiarity of locality or habitation; for the effect of neither one nor the other of the presumed influencing causes has yet been correctly defined;" nor shall we withhold our assent to the proposition that "in order to determine the simple influence of locality, like classes in the respective districts must be compared." This is true philosophy, and the only proceeding whereby observation can attain to the dignity and certainty of experiment. Let us now see how Mr. Neison, applies these principles to practice. He first provides himself with materials for his inquiry from two sources, namely, the quinquennial returns for 1836-1840, made under the Friendly Societies Act, and returns from Scotch friendly societies obtained by offering prizes for the best reports. From these two sources the two essential elements of all correct reasoning and calculation as to the duration of life, the ages of the living, and the ages of the dying, were obtained. There can be no doubt, therefore, of the correctness and completeness of our author's data.

A simple and natural arrangement of these facts in three groups, the first comprising benefit societies belonging to the rural districts, the second societies belonging to town districts, and the third societies established in large towns or cities, was then made, and the results for the several employments were calculated separately. By throwing the results for any single employment in rural, town, and city districts together, the influence of that employment upon health and life would be readily ascertained by comparing it with other employments, or with some common standard. On the other hand, by placing the results of the several employments under the distinct heads of rural districts, town districts, and city districts, the influence of locality is as readily determined. It is obvious that we are here provided with accurate and sufficient data for answering the

question, what is the influence of employment and locality respectively upon health?

The first series of tables which we shall notice, presents in distinct columns the ages, the number living at each age, the number dying at the same age, the mortality per cent. at that age, and the specific intensity of life, (the number living divided by the mortality per cent.,) for rural, town, and city districts, separately, and for the three combined. Now as these tables present the expectation of life of that prudent part of the labouring population which possesses the means of joining benefit societies, we may expect some interesting and instructive results from conparing this important class of the community with the whole of England and Wales.

The relative value of life in this class under different circumstances of locality will be made to appear by the following statement. If we take 100,000 persons at 10 years of age, in England and Wales, and from among the members of benefit societies in rural, town, and city districts respectively, we find that while in England and Wales, half that number have died between 62 and 63 years of age, the equation takes place in rural districts, between 68 and 69, in town districts, between 64 and 65, and in city districts between 61 and 62. The rural districts, therefore, when compared with the kingdom at large, enjoy an advantage of 6 years, the town districts an advantage of 2 years, while the city districts lose one year of life. The difference in favour of the rural when compared with city districts, is no less than 7 years. Again, if we compare the specific intensity of the four classes, the vitality, so to speak, of all England, at 30 years of age, is equal to the vitality of the rural members of benefit societies at 47, of the members of town societies at 41, and of those of the city district at 33. After 50 years of age, however, the kingdom at large has a slight advantage over the towns, though it still yields to the rural districts. So also, with the expectation of life; it is uniformly better for the rural districts than for England and Wales, uniformly worse for city districts, and worse for town districts at 40 years and upwards. When the three districts are thrown together, the total results are also found to be in favour of the members of benefit societies, and they fully justify Mr. Neison's observation, that though "the circumstances in which the humble and working population of the country is placed, have generally been thought adverse to a prolonged duration of life, the healthiest life tables hitherto formed, have not shown anything so favorable as the present results, even among what are generally considered the select classes of society."

In commenting upon this remarkable fact, Mr. Neison first betrays the tendency to exaggeration which we have alluded to as one of the defects attaching to his valuable labours. The persons composing friendly societies, he observes, "are almost exclusively the hard-working members of the community, chiefly occupied in the drudgeries and toils of the mechanic arts, and consequently exposed to the inclemencies of seasons, excesses of temperature, impure atmospheres, constrained postures, and other conditions usually thought objectionable. Their incomes are very limited, affording but the scantiest and simplest means of support. Their habitations are of an inferior order, being of the cheapest kind, and consequently in the worst streets." But they are nevertheless raised above the very

lowest class, for the very fact of their belonging to friendly societies, is an indication of superior industry and frugality, qualities by which they are very widely separated from the reckless and improvident, the victims, *par excellence*, of "poverty, distress, destitution, and disease." Mr. Neison would seem to be here speaking solely, or at least chiefly, of the inhabitants of large towns, and to forget that the classes who alone enjoy this longer duration of life, are the inhabitants of the country and the smaller towns, but chiefly the former, while the inhabitants of the larger towns, members of benefit societies though they be, fall far short in this respect of the average of all England. Here, then, we think that Mr. Neison's strong views have led him to exaggerate the bearing of his facts on the sanitary question.

We nevertheless fully agree with Mr. Neison, in thinking the superior value of life among the members of friendly societies, a very remarkable and important feature in the inquiry, in which we willingly follow him, "From what source or class does the excess of mortality, which makes up the general average of the community, arise?" We learn, in the first place, that the upper classes of society are among the contributors to this excessive mortality. This statement, though at variance with the prevailing opinion, is fully borne out by facts and arguments adduced by Mr. Neison, and by tables which will be found at pages 36 and 37 of his work. The first table places side by side, the expectation of life among the families of the peerage and baronetage, as calculated by Mr. Neison from facts collected by Dr. Guy, and the expectation of life among the members of benefit societies in the most unhealthy city in England—Liverpool. The result is very remarkable. The expectation of life among the aristocracy exceeds that of the numbers of friendly societies in the city of Liverpool, by somewhat less than a year, throughout the whole term of life, and falls short by two years or more of the expectation for the members of friendly societies in city districts. *A fortiori*, therefore, it is lower among the aristocracy than for the male population of England and Wales. In further confirmation of the statement that the higher classes have a shorter expectation of life than the average of the entire community, a second table is given contrasting the expectation of the members of friendly societies, belonging to 16 trades, in the rural districts with the aggregate observations of assurance companies, and the tables calculated by Mr. Finlaison, on the lives of the nominees of the Government tontines and annuities. Now, here we have selected lives from among the higher and middle classes, contrasted with selected lives among the labouring population, and yet a result favorable to the latter class. To prove this it will suffice to compare the three classes at the same age. If we take, for instance, 20 years, the vitality of the three classes will be represented by the following numbers.—Friendly societies 44, assured lives 40, government annuitants $37\frac{1}{2}$. At the age of 30, the numbers are $37\frac{1}{4}$, $33\frac{1}{4}$, and $32\frac{1}{2}$; at 40—30, 26, 26; at 50—23, $19\frac{1}{2}$, $19\frac{1}{2}$, and at 60—$16\frac{1}{4}$, $13\frac{1}{2}$, $13\frac{1}{2}$.

We cannot, therefore, withhold our assent from the proposition that the labouring classes have a better expectation of life than the higher orders, and that the lower duration of life among the latter is one of the sources of the excess of mortality which makes up the general low average of the community as compared with the members of benefit societies.

Nor are we disposed to differ from the sentiments conveyed in the following passage: "The blessing thus bestowed on the frugal and industrious workmen of the country composing friendly societies, in having granted them, as appears by the present inquiry, a prolonged duration of life, must, therefore, be recorded as a really true and distinctive feature of that class of persons, and is, no doubt, the result of their simple and uniform habits of life, and the more regular and natural physical exercises to which they are habituated." We think it highly probable, moreover, that "it could be clearly shown, by tracing the various classes of society, in which there exists sufficient means of subsistence, beginning with the most humble, and passing into the middle and upper classes, that a general deterioration in the duration of life takes place; and that just as life, with all its wealth, pomp, and magnificence, would seem to become more valuable and tempting, so are its opportunities and chances of enjoyment lessened. As far as the result of figures admit of judging, this condition would seem to flow directly from the luxuriant and pampered style of living among the wealthier classes, whose artificial habits interfere with the nature and degree of those physical exercises which, in a simpler class of society are accompanied with a long life."

This view is strengthened by a comparison instituted in Dr. Guy's essay already referred to, between the members of the families of the peerage and baronetage, and the successors to titles among the peerage. At 25 years of age the expectation among the former class exceeds that among the latter by upwards of 5 years, and by upwards of 2 years at 35 and 50 years of age. "This difference," as Dr. Guy observes, "can scarcely be due to the mode of calculation, and therefore gives rise to a question of some interest. The families of the peerage and baronetage comprise a large proportion of persons urged by ordinary motives to wholesome exertion of body and mind, while the expectants of title may be fairly presumed to have a greater command of the means of self-indulgence, and less motive to those efforts, whether mental or bodily, by which men may be said to earn health and long life. It is not a little remarkable that the expectation of life among the male members of the families of the peerage and baronetage should exceed by from two to four (five?) years the expectation of life for the same age among the successors to titles; and that the expectation of life in the former class falls short in a similar manner, and to a similar extent, of that of the entire kingdom and of the lives insured in the principal insurance offices. There is here a coincidence which cannot be overlooked, and a fair ground for answering the question already proposed in the affirmative. In the unlimited command of the means of dangerous self-indulgence, and in the absence of the common motives to wholesome exertion, the expectants of titles differ as much from the other members of noble families as these differ from the mass of mankind; and the effect in each case displays itself in broken health and a shorter average duration of life. When the duration of life shall be accurately ascertained for all the several classes of society, it will probably be found that the labouring man, placed above want, but always dependent upon his own exertions, attains a higher average age, as he undoubtedly reaches a higher extreme, than his richer and more luxurious superior."

From all that we have stated it must be quite obvious that the classes

which by their high rate of mortality lower the duration of life of the whole community are the higher and middle classes and the inhabitants of large towns, and that the laborious inhabitants of the rural districts and smaller towns tend to increase that duration. That the labouring classes of large towns generally have a low average duration of life may be inferred from the fact that even members of benefit societies inhabiting these towns enjoy a less favorable term of life than the average of all England. Mr. Neison seems to have overlooked this fact so clearly established by his own tables, when he represents the mortality of England as being constituted by that of the members of friendly societies added to that of the middle and upper classes, leaving as the chief cause of the low average duration of the entire community the high rate of mortality prevailing among "the improvident and reckless, the poor and the destitute, who are exposed to the inclemencies of the seasons, the fluctuations of trade, and fall victims to epidemical and other diseases."

It may be held to be perfectly certain, and it is a fact more and more confirmed by every fresh inquiry, that there is an excessive mortality and low duration of life in the inhabitants of large cities, and that not during infancy alone, but at every period of life. With the mortality during infancy we have here no concern, further than to signalize it as a proof that towns are in themselves injurious to health and life; but if we confine our attention to the case of adults it is clear that a high mortality may be due to two causes—the inherent unhealthiness of a town life, or the concentration in towns of employments in themselves unwholesome. This is the next question to which Mr. Neison addresses himself, and with a very marked leaning towards the latter alternative. We will follow him step by step in his inquiry.

The first result at which he arrives is that the agricultural labourers are longer-lived than the total of the inhabitants of rural districts; from which it may be reasonably inferred that the luxury and self-indulgence of the better classes, and the more confined and sedentary life of the tradesmen and artisans inhabiting those districts is, in a very marked degree, unfavorable to health and longevity. The difference in favour of the labourer, at the earlier periods of life, is about two years. The position of the labourer appears still more favorable, when we contrast his expectation of life, not with the total including his own class, but with the residue. Thus, while half the population of labourers dies off in the 72d year, and half the general average in the 69th year, half the residue have perished in the 65th year. The labourer, therefore, enjoys an advantage over the residue of about seven years; a difference which Mr. Neison attributes solely to difference of occupation, to the exclusion of the "*supposed* contaminating influences of ill-ventilated houses, narrow streets, bad sewerage, poisoned air, epidemic town fevers, and factory restraints." We have italicised the first word of this quotation to attract attention to this fresh proof of our author's scepticism, which, as we think, is scarcely justified by the facts of the case. Though it is quite true that our rural villages in which the residue, with the exception of country gentlemen and farmers, reside are not cursed with narrow streets or factory restraints, they are not quite so free as Mr. Neison seems to assume from ill-ventilated houses, bad sewerage, poisoned air, and epi-

demic fevers. On the contrary, ventilation is often quite as much neglected in villages as in large towns; sewerage is not bad simply because it does not exist at all; the air is not entirely free from unwholesome exhalations, nor is epidemic fever quite so rare a phenomenon as the above quotation would seem to assume. To all these influences, in a degree of intensity not much inferior to that which prevails in large towns, the inhabitants of rural districts are exposed, and those most who are most within doors, and most debarred from exercise. The error into which, as we believe, Mr. Neison has fallen is the not uncommon one of regarding occupations only in one point of view, namely, as entailing more or less exercise, or demanding a more or less constrained and irksome posture of body, without considering all the circumstances by which they are distinguished, and keeping out of sight the vitiated atmosphere in which all in-door employments are carried on. The agricultural labourers combine in their greatest possible degree, exercise and pure air, while those who follow other employments have too little of the one or the other, or of both: the former are less exposed to the causes of disease, and better prepared by healthful exercise to resist them, the latter are less favorably circumstanced in both these respects. This, then, is the secret of the higher expectation of life enjoyed by the agricultural labourer.

It will be seen that we differ from Mr. Neison, not in denying that employment has a very marked effect on the duration of life, but in asserting that one employment is more wholesome than another, because it exposes those who follow it in a less degree to the causes of disease which lurk in and about all the habitations of men, whether in villages, towns, or cities.

As to the necessity of taking into account the nature of the employments followed by the inhabitants of civic and rural districts, and of comparing men following the same occupation in the two localities, in order that we may ascertain the real influence on adult life of town and country respectively, we are in perfect agreement with our author. The necessity of such a mode of proceeding is too obvious to be denied, and until such a comparison is instituted we are bound to suspend our judgment as to the precise influence of locality on the health of our adult population. Mr. Neison has the materials for this exact comparison in his own hands, and we trust that he will not lose the opportunity of solving so interesting and important a problem. In the meantime, we are willing to hazard the conjecture, that the comparison will confirm the views of those who attribute the unhealthiness of our large towns to defective structural arrangements and overcrowding of dwellings and places of work, while it will somewhat disappoint those who have shown a tendency to an exaggeration of the opposite kind to that which Mr. Neison seems to us to have fallen into. But whatever may be the result of such a comparison, the high mortality of children in large towns is a fact which no argument drawn from the prevalence of unwholesome employments upon adults can shake,—a fact which proves beyond doubt or cavil, that cities are in themselves injurious to health and fatal to life.

But though we differ from Mr. Neison in the interpretation of facts we willingly own the great obligations under which he has laid us for the facts themselves, and we proceed to select from his ample stores some of the more striking and important of them.

Having shown the difference existing in the duration of life of the agricultural labourer and of the remnant of the inhabitants of rural districts, and thus proved the marked influence of employment upon health, where that influence would least have been sought for, he proceeds to compare the rural, town, and city districts in the aggregate with the special employments of the miner, the baker, the painter, and the clerk. Taking the age of 30 years, it appears that while the aggregate at that age enjoys an average expectation of nearly 37 years, the miner has about 33, the baker 32, the painter 30½, and the clerk 27½.

"It will no doubt cause some uneasiness in the minds of inquirers to find, that so highly important and industrious a class of men as clerks should stand lowest in the scale of the above employments, and that from 20 to 60 their expectation of life should be only 75 per cent. of the general average. The expectation of life among plumbers, painters, and glaziers in the same period is equal to 81 per cent., miners 85 per cent., and bakers 88 per cent. of the general average." It is doubtless a source of uneasiness, and one which must increase with civilization, but it cannot surprise us for a moment when we consider the sedentary life, the long hours, and the sort of air in which these unfortunate persons have to pursue their occupations. The fate of the tailor, the compositor, and the drapers' assistant is as much, if not more, deplorable, and we are sorry that Mr. Neison has not extended his comparison to these unhappy victims of overwork in a close, heated, and impure atmosphere.

We are next presented with tables contrasting the expectation of life of the aggregate of city districts with Liverpool, and the members of friendly societies with the entire population; a contrast which confirms the results of recent inquiries, at the same time that it proves the better sanitary state of the members of benefit societies. Our author shall explain in his own way the excessive unhealthiness of Liverpool. He says:

"A careful consideration of all the preceding observations, it is believed, will be sufficient to show that the excessive mortality of the general population of Liverpool must be due to some other cause than simply that of locality. The persons over whom the observations in the first column extend, being members of friendly societies, and almost exclusively workmen and mechanics, of necessity inhabit the inferior class of houses, in the worst conditioned streets; and it is therefore impossible that they can escape the contagious effects of the pestilential diseases supposed to be the scourge of unhealthy neighbourhoods; *and, admitting this*, the results given for the friendly societies must evidence all the legitimate effects due to locality; and therefore the excessive mortality of the general population is due to some other cause—such as the poverty and distress, which, unhappily, are allowed to remain too much neglected in the large manufacturing and commercial towns of the kingdom"

The italics in this passage are our own, and by adopting them we wish to intimate that we do not admit the position, at least to the extent to which it is pushed. We think it highly improbable that men who have the means or the prudence to belong to friendly societies form any considerable proportion of the dwellers in the cellars or narrow courts of Liverpool. And, be it recollected, the unhealthiness of Liverpool is not attributed by those who know anything at all of the matter to its locality, but to the peculiarly bad structural arrangements with which it abounds. Liverpool is pre-eminently a city of cellars and courts; and it is these

cellars and courts, and not the locality of the city, nor the peculiar destitution of its inhabitants, which must bear the blame of its excessive mortality. Like all other large cities it abounds in unhealthy trades, and has its share of the poor and destitute, but there is no ground for supposing that in the aggregate its trades are more unhealthy, or its poor more numerous than those of other large cities which present a far more favorable figure of mortality. Are the employments of its inhabitants, taken one with another, more unhealthy than those of Birmingham or Sheffield? We think not; nor do we imagine that its poor are more numerous. Our author insists strongly upon the necessity of taking into account the occupations of the inhabitants of the cities which we make the subject of comparison. We cordially agree with him; but we submit that we ought to be equally careful in contrasting their places of abode. If it would lead to serious error to compare the rural labourer with the clerk, it would be to the full as fatal to accuracy to institute a comparison between the dweller in an open street and the miserable occupant of a cellar. If the comparison between Liverpool and Birmingham would be inexact because the one abounds in dock labourers and the other in mechanics, surely it would be equally inconclusive, as far as the influence of mere locality is concerned, seeing that in the one there are several thousand of cellar-dwellings, while in the other such disgraceful habitations are altogether unknown. The precise influence of locality upon health, therefore, is a problem of very difficult solution, for it is not likely that we shall be able to find a group of conditions—employment, place of residence, wages, habits of life, &c.—precisely, or even very nearly, the same in any two cities under the sun. We must be content, therefore, with approximations and probabilities, receiving with due acknowledgment such valuable suggestions as those which Mr. Neison has thrown out, but not allowing ourselves to be driven from a position which we have taken up and fortified by fact and argument, till we are either fairly beaten, or see that our adversary is prepared to overwhelm us with weightier metal than our own. At present we are determined to hold out, and fight the battle of locality, or to speak more correctly, of habitation *versus* employment.

We are obliged to dismiss an interesting chapter on the duration of life in Scotland, with the brief remark that in that country there is one city which presents a worse sanitary state than unhealthy Liverpool itself, we mean Glasgow. At 30 years of age, the expectation of life for Liverpool is 27 years, for Glasgow less than 25; and a similar difference exists at other ages. Surely this is a strong argument in favour of the extension of the provisions of the sanitary bill recently laid on the table of the House of Commons to Scotland.

But what of unhappy Ireland? Mr. Neison's inquiries do not extend to her; we must, therefore, turn for a moment to the work of Mr. Willis, where we find (at p. 18,) a life-table founded on facts carefully collected by inquiries among the labouring population of Dublin. Though this table does not admit of exact comparison with those of Mr. Neison, inasmuch as the life-tables for Liverpool and Glasgow, are formed for the whole population of those cities, yet it may be interesting to our readers to know how the results for this one class stand as compared with those obtained for the unhealthiest cities of England and Scotland. If we take as before

the age of 30, the expectation of the town of Liverpool being 27 years, and for Glasgow 25 years, that for the labouring class of Dublin is 24 years. For more advanced periods of life, however, the Dublin table presents more favorable results. Thus at 50 years, the expectation for Liverpool and Dublin being 16, that for Glasgow is 14½ years.

The greater part of the inquiries on which the life-table is founded, were made in the crowded parish of St. Michan, each house in which parish, contains on an average 16¼ persons, (St. Giles' in London, can only boast of 11 to a house.) Of this district, Mr. Willis gives a description which we cannot forbear from quoting, so strongly does it remind us of what we have recently read of the state of some parts of the unhappy country of which this is the capital.

"There are no gentry within the district, and the few professional men or mercantile traders, whom interest may still compel to keep their offices here, have their residences in some more favoured localities. This parish, that within the last thirty years might boast of as large a proportion of professional and mercantile wealth as any in the metropolis, is now the refuge of reduced persons from other districts; and very many of the houses then occupied by respectable traders, are now in the possession of a class of men called 'house-jobbers,' who re-let them to poor tenants. These jobbers have no interest in the houses save their weekly rent; the houses, therefore, undergo no repair; the staircases, passages, &c., are all in a state of filth; the yards in the rear are so many depots of putrid animal and vegetable matter; and if a necessary be in any of them, it frequently is a source of further nuisance. The courts and back places are, if possible, still worse, and are quite unfit for the residence of human beings. They are almost all closed up at each end, and communicating with the street by a long narrow passage, usually the hall of the first house, and not more than three or three and a half feet wide. Pipe-water, lime-washing, dust-bin, privy—these are things almost unknown. The stench and disgusting filth of these places are inconceivable, unless to those whose harrowing duty obliges them to witness such scenes of wretchedness. In some rooms in these situations it is not an infrequent occurrence to see above a dozen human beings crowded into a space not fifteen feet square. Within this space the food of these wretched beings, such as it is, must be prepared; within this space they must eat and drink; men, women, and children must strip, dress, sleep. In cases of illness the calls of nature must be relieved; and when death releases one of the inmates, the corpse must of necessity remain for days within the room. Let it not be supposed that I have selected some solitary spot for this description: no, I am speaking of an entire district, and state facts incontrovertible. I indulge in no theories as to the causes which produce this state of things, but I may state the results. They are—that every cause that can contribute to generate contagion exists here in full vigour, and that disease, in every aggravated form, with all its train of desolating misery, is rarely absent."

After this description we shall scarcely be surprised to hear that "eighty cases of fever, including relapses, were said to have occurred in one house in the course of twelve months." "Fifty persons have been admitted to hospital from another within a year." "Thirty patients from another within eight months." "Nineteen from a fourth in six weeks." "The inmates of a house which was thrice lime-washed in the space of a few weeks, were as often readmitted to hospital, in consequence of sleeping in their infected bedding."

Poor Ireland! Turn where we may, you present us with the same sad picture of destitution and misery. The records of the land-commission,

the letters of the "Times commissioner," the census, and the writings of those who treat of sanitary matters, all tell one consistent tale. Poverty, and his haggard offspring, misery, disease, and crime, possess the land, and all the efforts of all the governments in the world cannot remove them. We must be satisfied with knowing that it is so, and go on learning till knowledge bears its certain fruit. To those who seek for knowledge of this sort, we recommend Mr. Willis's work, and resume our notice of Mr. Neison's labours.

The first part of his work treated of the influence of locality, and incidentally of employment, on the duration of life; the last two chapters treat of the influence of locality on sickness, with an application of the results to the purposes of friendly societies. Our readers will soon perceive that this is a most important portion of the work before us. We shall treat this part of the work with all the respect which we have already paid to the first part, and shall analyse the results with the same minuteness.

We are first presented with a table showing the average sickness per annum, in weeks and fractions of a week, in the rural, town, and city districts, from which it appears that sickness, speaking generally, is at a minimum in rural districts, more rife in town districts, and still more prevalent in city districts, thus showing a general agreement with the mortality. The correspondence, however, is only general; for tables subsequently adduced, show that the relation of cause and effect generally supposed to exist between sickness and mortality does not always find place, but that, on the contrary, "the highest ratio of sickness is sometimes found associated with a favorable rate of mortality." "For example, labourers, although influenced by the most favorable rate of mortality, are found to be subject to as high an amount of sickness, as the general average." "Bakers, also, at the early and middle periods of life, are less subject to sickness than the general average, and among them there is likewise a higher mortality. The class butchers seem to experience a very high rate of mortality, although not subject to above the average amount of sickness." Tailors and clerks, again, are subject to a very high rate of mortality, without being subject to so much as the average of sickness.

It must be recollected that the sickness here spoken of is benefit society sickness, or, in other words, disabling sickness, and "that the same trivial circumstances which would be sufficient to disable sawyers, colliers, and miners, would have little effect on the tailor or the clerk." Sawyers, colliers, and miners are also subject to accidents and various injuries, which cannot be considered constitutional disease or sickness; yet it entitles them to relief from benefit societies, and they will of course be returned on the sick list. Tailors and clerks are less subject to these accidents, and accordingly their sickness is also less." When these circumstances are taken into the account, it must be evident that the true relation of sickness and mortality is not set forth in the returns of benefit societies. At the same time it must be admitted to be extremely probable that in some employments there is a great liability to slight attacks of disease, in others, a liability to one or two dangerous and fatal maladies. But we must pass on from this speculative point to one of the highest practical importance; one in which the well-being of thousands of our most deserving fellow-

citizens is involved; the observed amount of sickness, as the basis of the calculations and practical operations of benefit societies. And here we must present our readers with a short table contrasting the results obtained by the Highland Society and those of Mr. Ansell with the more recent calculations of Mr. Neison.

Annual Sickness to each person in weeks:

Age.	Highland Society.	Ansell.	Neison
20	·575	·776	840
30	·621	·861	·911
40	·758	1·111	1·559
50	1·361	1·701	1·960
60	2·346	3·292	4·166
70	10·701	11·793	14·039

On this table the author has the following remarks.

"The remarkable increase in the amount of sickness, as shown by the present results, beyond the two other tables, will no doubt appear very startling to those not intimately familiar with the condition of friendly societies throughout the country. The rate of sickness as given in the table of the Highland Society, has been long and generally acknowledged to be much below the actual average, and even so far back as 1825 it was thought unfavorably of by a Committee of the House of Commons. It is unnecessary to enter intc the objections against the nature and source from which the data for the Highland Society's tables were obtained, as that subject has been amply discussed elsewhere. For some time after Mr. Ansell's work appeared, it was thought that contributions calculated according to the increased amount of sickness shown in his tables would render friendly societies perfectly safe; but instances occur almost daily of societies breaking down whose contributions approximate to these tables; and recently the increased amount of sickness has become so apparent to the members of some of the best regulated societies, that meetings have been held, and reports of a very clear and opposite kind published, pointing to the increased amount of sickness as the cause of their falling condition. A knowledge of circumstances of this kind first led to the present inquiry, the original object of which was simply to answer the question —whether friendly societies were subject to a higher rate of sickness."

We shall not follow Mr. Neison into his discussion of the causes of the difference existing between results so recent as those of Mr. Ansell, and his own; but content ourselves with expressing our entire confidence in his own tables, and the strong sense we entertain of the benefit which he must be the means of conferring on the societies in whose welfare he has taken so deep an interest. We can scarcely conceive a more important subject than that to which he has directed our attention. If it be important to encourage to the utmost habits of forethought and frugality it is little short of a crime to disappoint the cherished hopes of the deserving persons who display these virtues by negligently leaving the solution of the question on which a future provision must rest to the chance efforts of individuals. We, therefore think that there is much force and justice in the following observations:

"To those interested in the progress of friendly societies, the results are highly important, as they will demonstrate the impossibility of permanence in those institutions on their present foundation. Considering the immense number of those societies which have broken down, it is lamentable to think that so little should have been done to ascertain the real nature and extent of the risks to which they

are subject. It is still more remarkable that so many legislative enactments should have occupied the attention of the government of the country from time to time, and that committees also of the House of Commons should have had the condition of those societies for several years under consideration, without any practical measure being carried out for collecting and arranging data in a proper shape to point out the true character of the liabilities to which they are subject. In fact, the stimulus given to the formation of those societies by some recent acts of Parliament should be regarded as an evil rather than as a benefit to the working classes. An immense number of societies were formed in a very short period, and their contributions regulated by the most delusive and inadequate data, so that at the present time very few are to be found calculated to survive many years. Under a scientific and amply-developed system, those societies would be calculated, in a few years, to completely remove the cause of nearly all that poverty, distress, and misery, which haunt our manufacturing towns, and fill our workhouses with the working classes of the country; but owing to the imperfect and unstable foundation on which they are at present built, instead of being a help and support to a poor man, they involve him in those difficulties for which he might otherwise have provided. On becoming a member of such a society, he reasonably looks forward to it as a support for his declining years, and a protection during periods of sickness and disease; but, ultimately, at the very time when assistance is required, he discovers that the society has been formed on a ruinous plan, that the increasing years and infirmities of its members have absorbed all its funds, and that those surviving must be thrown destitute on the parish or a public charity. It is thus, by the most ill-conceived of all proceedings, the legislation of the government has hitherto tended. Every facility and encouragement are given to the formation of societies, without any help or information for their management or guidance. The ship is cast upon the waves without a rudder or a compass, and the safety of the vessel left to accident or chance."

There is a glimpse here of what will assuredly some day come to pass, when the government shall be alive to the value of the principle of insurance carried out to its full extent, and substituted for the clumsy, cumbrous, and costly machinery of poor-laws and union-houses, and all the wasteful extravagance of our present system of mending, and soldering, and patching; when in all things the great practical motto, "*Prevention better than Cure,*" shall become the motto of the state, and the system of *Laissez faire*, with all its fearful offspring of misery, disease, and crime, shall cease and determine. Can any one doubt for an instant what the effect would be of a comprehensive system of benefit-societies, coextensive with savings'-banks, guaranteed by government against the accident of limited observation or incorrect calculations, and in which the labouring classes might place implicit reliance? We may rest assured that, ere many years had passed over our heads, some of our unions would be found too large, and our prisons perchance to offer too much accommodation. The principle of assurance, like our civilization, is but in its infancy, and the art of government in its cradle. But the nineteenth century has something better than railroads in store for us, or we greatly mistake the signs of the times.

We are warned by the length to which the foregoing remarks on this truly valuable work have extended, that we must hasten to a close; but we cannot conclude without quoting a somewhat lengthened passage, which embodies our author's views on one of the greatest practical questions of the day—the sanitary question:

"If in any public inquiry it should be attempted to ascribe the increased amount of sickness in the town districts to the less healthy nature of the districts,

or their peculiar local influence on health, the conclusion would certainly be fallacious. Precisely similar arguments to those made use of in reference to the mortality of those districts, will explain the differences in the ratio of sickness in the same places; and it is therefore to be inferred, that whatever sanitary regulations may be carried out for promoting the health of towns, the wide distinction between the rates of sickness and mortality in particular districts will still not disappear. The cause of that difference is beyond the reach of any sanitary measure; and unless a change were to take place in the character and machinery of the manufactures of a town, by which the workmen would be habituated to less restrained but more active and complete physical exercises, no improvement in the state of health is to be looked for. The evils, so far as relates to health, represented to exist by some writers to so frightful an extent, and to connect themselves with inferior sewerage, filthy streets, and ill-planned houses, are certainly overstated by them. The data brought forward have generally been of the most indefinite and insufficient nature; and when, in connexion with this, the erroneous methods employed, and the promiscuous manner in which their figures are generally combined, are kept in view, it must seem surprising that the thinking and intelligent portion of the community should have given their opinions any credence, or believed their conclusions entitled to so much weight. Perhaps no statistical facts are better established than the duration of life among the middle and upper classes of this country; and if the data brought forward in this paper be received as of sufficient merit to represent the duration of life among the working classes, it will then appear clear that any important change to be hoped for in the value of life in the town districts, must be effected through other means than sanitary regulations. Those persons purchasing government annuities, and having dealings with assurance companies, are certainly beyond the reach of any improvements to be introduced by local regulations; and if cleanliness of habit, comfort of dwellings, and fresh air be of themselves powerful elements in raising the standard of life, their influence should be felt among that class of persons. But what are the actual results? The poor workmen inhabiting the miserable streets of our large towns, and inhaling their supposed noxious vapours, are actually longer lived than the affluent and upper classes, whose easy circumstances enable them to inhabit comparatively the palaces of the kingdom. It is evident, from the disparity in the value of life among different classes of workmen, whose conditions as to whatever is within the scope of public sanitary measures are the same, that other elements must exist having a powerful influence on the duration of life. It would further appear, by viewing the various classes of society more in connexion with the physical exercises to which they are habituated, than in connexion with their moral position and rank in society, and consequently with their sanitary condition, that a better clue will be found to the differences in the duration of life. It is not to be expected that any arrangements whatever as to the drainage and planning of streets, are likely to add to the longevity of a tailor; but if it were possible to give his frame the physical exercises of a ploughman, twenty per cent. would be added to the duration of his life. Neither is it to be thought that the plumber, painter, and glazier is to be relieved from the poison of the metallic emanations to which he is subject; nor that the clerk can inhale the fresh air and indulge in those exercises necessary to develop his physical constitution, while he follows the drudgeries of the counting-house. It is an aggregation of these, and other employments similarly conditioned, which make up the excessive mortality of our large towns; and since it has been shown in the preceding pages, that this class of lives is also less healthy even in the country districts, and that the town populations are chiefly made up of persons following such occupations, the legitimate result to be expected is a shorter duration of life in towns, independent of any local influence on health. If improvements and changes are to be effected in the sanitary regulations of our large towns and cities, let them at once be carried out—not upon the necessity of such municipal innovations to avert a pestilential havoc in human life—but on the true merits of the question—the comforts, conveniences, and elevation of taste and moral purity thence arising."

These opinions of Mr. Neison, though, as we think, hastily formed and greatly exaggerated, are entitled to be treated with the respect due even to the errors of one who spares no pains or expense in collecting facts, and who exhibits unusual assiduity in using them. But we cannot withhold the expression of our surprise and regret that Mr. Neison should have treated with so little respect the conclusions of the many men of science, who have concurred in tracing the high mortality of our large towns to the causes to which he attaches so little importance. It is at least within the bounds of possibility that similar fallacies to those which Mr. Neison assumes to have misled Mr. Chadwick, Dr. Southwood Smith, the members and witnesses of the different health-commissions, and the government itself, may have crept in among his own conclusions.

The higher classes may be short-lived in spite of their broader streets, and larger houses, and better food, and warmer clothing, and greater personal cleanliness; just as the lower classes may be short-lived from the want of all these advantages. Luxury and self-indulgence may be to the full as fatal as hardship and privation. A king may not be one whit healthier or longer-lived than a beggar, a peer than a tailor, a member of the House of Commons than a delegate of a trade's union, an author than a compositor; but *non constat* that the unhealthiness of the extremes of the social scale may not spring from such different causes as to render any comparison between them ridiculous. The one may take as much poison into his stomach as the other into his lungs, the chief difference between them being, that the rich man has more control over his cook than the poor man has over his employer. It will be much easier for the one to order a plain dinner, than for the other to procure a single breath of fresh air. When Mr. Neison shall have succeeded in explaining away the eighty cases of fever, including relapses, occurring in one house in Dublin in the course of twelve months; the fifty cases occurring in another in the same period; the thirty patients in eight months from a third; and the nineteen in six weeks from a fourth, and scores of similar facts occurring in all the large towns of the empire, without the aid of defective drainage and sewerage,—we will discard this item of our sanitary creed. When he shall have instituted an exact comparison between the tailor, compositor, or needlewoman, working in a crowded, heated, and unventilated apartment, and the same class following the same avocations for the same number of hours a day, in a cool and pure atmosphere, and shall succeed in proving that the one class is to the full as healthy as the other, we will give up all faith in ventilation, and embrace his own theory of deficient exercise. All this, and something more, must come to pass before our friends of the Health-Commission and their allies can be forced from the strong position which they have taken up. They may have fortified themselves with some worthless arguments, and entrenched themselves behind some showy fallacies, but in the main we are convinced that they will stand their ground, till they find Mr. Neison himself of their mode of thinking. Not content with advocating "municipal innovations," as conducing to "the comforts, conveniences, and elevation of taste and moral purity, thence arising," they will continue to urge them with all their power and influence as essential to the enjoyment of such health as is compatible with the disadvantages to which the social occupations of the

labouring class expose them. In the mean time, they cannot be insensible to the great value of Mr. Neison's labours, and they hail him as a fellow-worker with themselves in that stubborn and tangled field of mingled facts and fallacies, where the wheat and the tares still grow together, and the weeds cling even to the sheaves.

ART. II.

Dr. J. L. Schönlein's, Professor in Zürich, Allgemeine und Specielle Pathologie und Therapie, &c. &c. In Vier Theilen. *St. Gallen*, 1839.

The General and Special Pathology and Therapeutics of Dr. J. L. Schönlein, Professor at Zürich. *Edited by several of his Class, from Notes taken at his Lectures.* In Four Parts. The Fourth Edition, much-improved. *St. Gall*, 1839. 8vo. pp. 327, 276, 281, 208.

Schönlein's Klinische Vorträge in dem Charité Krankenhause zu Berlin. Redigirt und herausgegeben von Dr. L. GÜTERBOCK. Erster und Zweiter Heft, 1842; Dritter Heft, 1844. *Berlin.*

Dr. Schönlein's Clinical Discourses in the Charité Hospital at Berlin. Taken and edited by Dr. L. GÜTERBOCK. In Three Parts. 8vo, pp. 480. *Berlin*, 1842. 1844.

Dr. Schönlein als Artz und klinischer Lehrer aus der Schilderung der Dr. Güterbock; einer unabweisbaren Kritik unterworfen von Dr. LEHRS und Dr. SCHARLAU. *Berlin*, 1842.

An unimpeachable Critique on Dr. Schönlein, as Physician and Clinical Lecturer, &c. &c. By Dr. LEHRS and Dr. SCHARLAU. 8vo, pp. 201.

WE feel that an apology is due to our readers for our tardy notice of the pathology and therapeutics of Schönlein. His fame is European, and whether justly or unjustly awarded, that extended reputation was reason sufficient for his trial in our critical crucible. If, however, we were to print a whole page of excuses our readers would be little the wiser; therefore, we content ourselves with observing that the delay has been advantageous, inasmuch as it enables us to judge of the clinical performances of Schönlein, as well as of his labours at the desk. We begin with the latter.

Medicine concerns itself with life generally, and with the life of man specially. Man is a part of the general system of the universe, and in common with all other organisms, seeks to separate himself from the great whole, and to be manifested as an independent entity. The great whole resists, antagonizes this effort;—a contest arises between the egoistic and planetary principles; and so long as the latter is equipoised or overcome by the former, there is health to the individual. The ascendancy of the planetary principle is death. Diseases is the struggle of the egoistic with the planetary—the resistance of the individual organism against injurious agents (schädlichen potenz) which seek to destroy it. These struggles appear under diverse forms which mark the forms of disease. The form of disease may be determined by three circumstances: 1. The nature of

the hurtful agents. 2. The organs on which the hurtful agents act. 3. The individuality of the subjects in which diseases are developed. Hurtful agents are divided into two classes, accordingly as they are of internal and external origin. The external may be, *a*, Alimentary, and act on the chylopoietic viscera. *b*, Atmospheric, and act on the lungs and skin. *c*, Cosmic, operating on the nervous system. *d*, Chemical, which appear sometimes as injurious agents, sometimes as curative agents: poisons comprised in the last division may be arranged in two classes, the one being represented by arsenic, the other by prussic acid. *e*, Mechanical agents: these differ according to their mode of operation and the organ on which they act. Stone in the bladder or gall-ducts, and monstrosities belong to this head. Hurtful agencies of internal origin may consist, *a*, of increased or diminished power, physical or psychical, as paralysis of the muscles, loss of memory, &c.: *b*, in suppressed or increased secretion, as of the saliva, urine, &c.: *c*, in a peculiar derangement of the soul, with which insensibility to other impressions is connected. The emotions belong to this class, which may be divided into the depressing and exciting.

The second circumstance determining the forms of disease is the organ on which the hurtful agent acts. All organs have a receptivity for hurtful agents,—a predisposition to disease, which is greater or less as the organ is, or is not, in a state of evolution. The evolution may be either permanent or periodic; the former may be subdivided into the true period of evolution, or youth; middle age; and the period of involution, or old age. The physiological characters of these successive stages of development and decline are known, as well as their pathological relations, or (in Schönlein phrase) the receptivity of the organs during each stage.

The third circumstance determining the forms of disease, is the individuality of the subject on which the hurtful agencies operate. The points to be noticed specially under this head, are, the temperament, sex, and idiosyncracy.

Contagion. Schönlein breaks off abruptly from the true sequence of his ideas to the subjects of contagion and spontaneous generation. We, however, follow the master mediciner in his own way. There are hurtful agencies which originate in the organism, and excite disease, and which are communicable to another organism, and excite similar disease. They are developed from spontaneous diseases, either in solitary or many individuals. The contagions developed in solitary individuals have their origin in the so-called acrid humours (Schärfe, acrimonia.) Acrimony of the humours is understood to mean a peculiar derangement of the quality of the animal chemistry: as, for example, a peculiar condition of the blood, saliva, &c. Two particulars are to be noted regarding contagion: 1, the basis, or materies; 2, the ætherial or active principle. The materies may be in the form of a fluid, a vapour, or a gas. The nature of the active principle is unknown, but Schönlein thinks it allied to free electricity; because firstly, it is perceptible to the taste and smell, as is also galvanic action (!); secondly, because certain electrical bodies are the best media of contagion, as glass, resin, silk (!).

Symptoms of disease. Every hurtful agent acts only on its own proper organs: as, for example, the atmosphere on the lungs: consequently it excites only a local disease, and not a general affection in the widest sense. The egoistic principle, however, reacts against the injurious agent, and a

series of phenomena are developed, which were not manifest in the healthy organism. These phenomena are the symptoms of disease. There is, however, no true signum pathognomicum: dread of water, for example, is present in the more intense forms of typhus as well as of hydrophobia; but there are symptomata symptomatum. Symptoms are subject-objective, when they are expressed by the patient; object-subjective, when noted by the observer. They also may be symptoms of the cause of disease arising directly from the operation of the injurious agent; or symptoms of the disease, and originate in the reaction of the egoistic principle. If the hurtful agent is not intense, or if the organ be unimportant, the symptoms are local; if the contrary, the whole organism is implicated. According to Schönlein, this sympathy of the organism with the local affection constitutes fever.

Fever. Fever is rather the shadow of the disease than the disease itself. The mode of reaction of the egoistic principle is threefold. 1. It is sufficiently powerful to remove at once the injurious agent, and maintain the integrity of the system: this is erethismus. 2. The reaction is greater, and the struggle to remove the injurious agent more prolonged: this is synocha. 3. The reaction is too weak: this is torpor. If, for example, food acts injuriously on the alimentary canal, reaction of the egoistic principle takes place, vomiting follows, and the injurious agent is removed. Should the phenomena not cease here, but vomiting continue, and sensibility and irritability be developed, the reaction is too strong, and takes on the character of synocha; but if vomiting do not supervene, the reaction is too weak, and it assumes the torpid form. Just then as reaction is threefold, every topical affection and every form of fever is threefold. Erethism is the centre, synocha and torpor the two poles. The stage of convalescence from either of the poles must be through the erethistic. If it go beyond this, paralysis (lähmung) takes place: the organs affected cease to perform their functions. This paralysis or lameness may affect the nervous system, when it induces paralysis properly so called; or it may affect the vascular system, and gangrene be developed. In every disease, whether local or general, the nervous and vascular systems are active. The affection of the former is shown by the subjective symptom pain: of the latter, by the objective symptoms redness, heat, and swelling.

Inflammation has a threefold form: firstly, there is erethistic inflammation. In this form neither the venous nor the arterial system takes part, but the capillary system only. In extremely vascular structures, the capillary network is enlarged, the capacity increased, the organ reddened, the warmth exalted. In less vascular structures, new capillaries are formed. Secondly, there is synochal, or sthenic inflammation, in which all the preceding phenomena are more marked. The arteries themselves, as well as the capillaries, are implicated. The capillaries become arteries, and the arteries are dilated and their tunics thickened. Thirdly, there is the torpid or asthenic inflammation. In this form, venosity, or venousness, is the distinguishing characteristic. The veins are dilated and become varicose, and the capillaries change into veins, as in chronic ophthalmia, and the chronic inflammation of the mucous membrane in which polypi form. Inflammations may, however, be divided into arterial and venous; the former corresponding to the sthenic form, the latter to the asthenic.

Course of disease. Under this head Schönlein states the doctrines usually propounded by systematic writers as to the exacerbations and remissions of disease, tracing them, however, to telluric and lunar influences, albeit without clear and definite views as to the modus operandi of these influences. If we understand Schönlein aright, they are in fact more typical than real. Man is a microcosm, and just as the barometer and magnetic needle have their maxima and minima, so the pulse has a maximum and minimum, &c.

Doctrine of critical terminations. Schönlein adopts this to its full extent. The crisis may be general, and operate through the kidneys. When this crisis happens, a burning sensation is felt in the genital organs, and a feeling of dragging in the lumbar region and along the urethra experienced. With these there are joined a frequent desire to pass urine, a dry, rough skin, thirst, and sometimes an intermitting pulse. When the urine is critical, it must be secreted in due quantity; in the beginning, it exhibits a cloudy appearance near the surface (nubecula), and afterwards in the middle (suspensum), and lastly, a precipitate, or sediment, which is easily commingled, is reddish, and is somewhat raised in the middle. At the same time, the skin is moist and perspirable, or a copious sweat actually breaks out. When the crisis takes place through the skin, it is reddened, warm, and soft; the pulse becomes soft and small, the urine scanty; the perspiration must be general and clammy (kleberig); the skin must be warm afterwards, and the patient feel sensibly relieved. Sometimes there are other marks of a cutaneous crisis, as exanthems, and which must be considered as local crises. Of this kind are the vesicles which appear on the abdomen in typhus, and on the thorax and about the mouth and nostrils in peripneumonia.

Critical hemorrhages appear only in synochal diseases. They break out in various parts, but with reference to the organ affected and the peculiarities of the patient. Critical hemorrhages from the nose are most frequent in the young, and when the affected organ is situate above the diaphragm, particularly if the disease be synochal. The premonitory signs are redness and tension of the face, red watery eyes, spectral flashes, singing noises in the ears, itching of the nose, weight in the temples and headache, particularly of the back of the head. A discharge of a serous fluid often precedes the hemorrhage; the carotids beat violently, and the pulse is dicrotic. When the organ affected is below the diaphragm, the bleeding takes place from the nostril of that side in which the organ is situate: for example, in splenitis it is from the left nostril, in hepatitis from the right (?). Critical hemorrhages from the uterus are most usual in synochal diseases of organs situate above the diaphragm; those of the rectum occur only in adults in whom the seat of the disease is below the diaphragm.

All crises, except those operating through the skin and kidneys, are local, according to Schönlein's views; a proposition not only opposed to sound physiology, but to experience. A diarrhœa is often the indicant and outlet of a general critical effort, and so also is a bronchorrhœa.

The terminations of disease may be: 1. In complete restoration to health. 2, In imperfect restoration. The terminations of inflammation are: *a.* The formation of pus, which Schönlein considers to be the secretion from a newly-formed mucous membrane, constituted by nature for the

relief of the organism. *b*. In hydropical effusion. This is an electric process (!) modified by natural laws. It takes place where serous membranes are in contact, and are in polaric tension with reference to each other. Their natural function to excrete an aqueous vapour is exalted in disease, and water itself is poured out, the action being analogous to that between the earth and the atmosphere, in which the vapours or clouds are precipitated in the form of rain or dew. *c*. Effusion of plastic lymph. *d*. Gangrene. *e*. Arthritis; the serous tissues becoming the seat of earthy deposits.

Metastasis. A disease on disappearing may be immediately replaced by another. This process takes place according to three principal laws. 1. In tissues which having a fundamental similarity of structures are related to each other, as the muscles and the muscular fibres of the heart, bronchi, intestines, or the peritoneum and the synovial membranes. 2. In organs constituting a system, as the stomach, liver, and spleen in the chylopoietic system. 3. In organs which are in direct polarity, as the liver, which being rather the positive or oxygen pole is opposed to the spleen, the hydrogen, or negative pole.

General Therapeutics. We do not pretend, be it understood by our readers, to give anything like a detailed view of the system of Schönlein. Eleven hundred pages of closely-printed German are not so easily reduced in our crucible. The foregoing paragraphs must therefore be considered as exhibiting the general nature of his pathological views. In the following, we intend in like manner to give a general view of his therapeutics.

The first step in therapeutics is the examination of the patient, which comprises two points of notice, the object and the method. The object includes the form and character of the disease. The form depends on the three circumstances already stated; the character depends on the mode in which the egoistic principle reacts against the hurtful agents, and may be erethistic, synochal, or torpid. If the disease be considered as a struggle between the egoistic and planetary principles, it may be, 1, vital—2, morbid.

The examination of a patient may be either genetic or analytical. The genetic traces the history of the patient, notes the sex, age, temperament, and other personal characteristics; the mode of origin of the disease, the cause as assigned by the patient, the previous state of health, &c. These are followed by a detailed inquiry into the condition as to structure and function of every organ, and system of organs.

The indications of treatment are founded on the anamnesis, the diagnosis, the prognosis, and the idea of the disease. From these arise two classes of indications, namely, indications of life, and indications of disease; the object of the former being to maintain the integrity of the organs, of the latter to remove the disease from the organism. The latter is subdivided into the indicatio causalis; indicatio curatoria, and indicatio urgens or palliativa.

The indications of treatment vary as the character of the disease varies. If it be erethistic, the reaction against the hurtful agent is usually not greater than is necessary to remove it from the system; here the expectant method is alone necessary. When the disease has the synochal type, the reaction is greater than necessary, and must be checked. This may be done by two methods: the one the antiphlogistic, applicable to the vas-

cular system; the other the sedative, applicable to the nervous system. Bloodletting is the principal of the indications in the former method, narcotics are the chief of the sedative remedies. It is interesting to observe the transition from the pure bitters to the pure narcotics; simply by the former containing less carbon. Quassia in large doses will act as a narcotic. The first class of narcotics, represented by opium and the papaveraceæ, acts on the cerebral system; the second acts primarily on the ganglionic system, and only secondarily on the cerebral. Belladonna, hyoscyamus, lactuca, &c., belong to it, and are best given in powder or extract. The third class acts on the spinal system, the communicating link between the cerebral and ganglionic. Prussic acid is at the head of this class, which comprises all vegetables containing the acid, as the amygdala amara, prunus lauro-cerasus, &c. A fourth class comprises those vegetables which contain an active principle, as cicuta, nicotiana, &c., and which act on the nervous system in general. To the soothing class of remedies belong also all those in which carbon predominates over hydrogen, comprising also the mucilaginous and fatty compounds, as aloes.

The diseases characterized by torpor, require to be treated by the roborant and excitant method. The excitants are: 1. Ammonia, and its preparations, which act directly on the vascular system: containing an excess of hydrogen, it is the counterpart of the narcotics, in which carbon predominates. 2. Those which act on the cerebral system are compounds of hydrogen and include animal secretions, as musk, ambergris, the poison of serpents, alcohol, &c. Among the excitants of the spinal cord are nux vomica, rhus toxicodendron, but especially arsenic and its compounds. The excitants of the ganglionic system are the fetid gums, anemone pulsatilla, certain metals as copper and silver; and "animal magnetism, a neutral product of two indifferent things, of which the one acts as the brain, the other as the heart, while here the soul is requisite in action on the one hand, and the will on the other." A long list of excitants of the chylopoietic, cutaneous, serous, muscular, and sexual systems, with minor subdivisions, is given, all of which, with a more detailed exposition of the doctrine of crises we shall pass over—to notice,

The natural system of Schönlein. We know disease by its phenomena or symptoms only; these therefore must constitute the basis of a natural system of classification. They indicate either derangement of function or change of structure. In disease of the heart, for example, the pulse may be smaller, or intermittent, the heart changed as regards firmness, thickness, colour, &c. In inflammation of the eye, there is redness, a structural change; impaired vision, a functional change. In forming a natural system, as well as either diagnosis, or prognosis, it is essential that the true value of symptoms be noted, and the accidental distinguished from the essential. Schönlein thinks that great errors have been made by combining distinctive and non-essential characters. Changes in the form and appearance of organs are more important than a change of colour. The chemical composition of the urinary secretion is more important than its quantity in a given time. Every phenomenon or symptom of disease has an absolute and a relative value. Delirium is a symptom of absolute value; but there may be delirium from cerebral disease, and the delirium of intoxication, in which cases its value is relative. A knowledge of the

circumstances which enable the physician to acquire a knowledge of morbid phenomena is also important to right classification. A disease may be noted and known at a glance; but it may also be distinguished only after putting together fragments of observation elaborated from the circumstances which prevent a clear perception of the disease as a whole.

The rules laid down by Schönlein for estimating the value of characters in disease. Functional derangement of the more important organs or structures takes precedence of that of the less important. Symptoms of vascular disturbance are of higher value than symptoms of disturbed renal function. A functional symptom is the more valuable in proportion to the number and importance of other phenomena with which it is connected, as the dread of water in hydrophobia. The more dignified the organ affected, the more important the symptoms referred to. Example; the heart takes precedence of the skin. Lastly, objective symptoms are to be preferred to subjective.

Among the circumstances which prevent a due estimate of the characters of disease are, firstly, the phenomena are never permanent, or in other words, the disease is continually undergoing a metamorphosis. The symptoms are changed, modify each other, or totally disappear, to be succeeded by new symptoms. Secondly, one disease runs into, or arises out of another. Thirdly, two or more diseases may be coincident, and then the phenomena complicate each other. Thus syphilis and gout may be combined in the same individual, and a peculiar affection of the bones may result, having the characteristics of both diseases. The correct combination of phenomena so as to characterize a disease truly, is a peculiar talent. The mineralogist, or botanist, has his subject before him as a whole; not so the physician, who has to make up a whole from a number of disjointed and disconnected parts. It is in doing this that the tact and skill of the physician is most displayed; and its successful practice (constituting skill in diagnosis,) depends often as much upon the innate powers as the experience of the practitioner. Experience and observation are, however, always requisite. The forms of disease, as described in systematic works, are often ideal, and not seen in practice. They are also the less available from a want of a critical arrangement and a technical terminology. A nosography should be correct, simple, and complete. Those phenomena should be grouped together, which have a physiological relation, and the functional changes should correspond to the structural.

Two modes of classification may be adopted, the analytical and the genetic. The genetic rises from the special to the general, the analytical, on the contrary, descends from the general to the special. He who would discover a system, must adopt the genetic method; he who would propound one, must adopt the analytic.

The genetic method of classification. The fundamental principle to start from is this; that there is no individual disease, but only individuals diseased. There is no actually existing pneumonia, but only individuals affected with pneumonia. Now all these diseased individuals, who under all varying circumstances of age, sex, temperament, present the same phenomena constitute a species of disease, and those essential phenomena characterize that species. For example, all persons suffering from traumatic pneumonia invariably exhibit the same phenomena of cough, diffi-

cult breathing, dull tone on percussion, and crepitating râle, whatever may be their individual condition or circumstances. Consequently, to determine any species, it is necessary to distinguish the essential from the accidental phenomena. The genus is determined after the same method by collocating species having characters in common. In inflammation of the lungs, for example, we have a series of phenomena which, although differing as regards the specific characters, have the common or genetic marks of cough, shortness of breath, &c. These general principles are also applicable to the determination of orders and classes, and are in fact the principles of classification adopted by taxonomists generally. We will now analytically follow our author through his nosology.

The nosology of Schönlein. The classes are three, and their characteristics are structural. There are three fundamental tissues : zoogen or animal tissue ; blood (the vascular system) and marrow (the nervous system) ; The first class is the morphæ, (μορφη), consisting in changes of the zoogen ; the second hematoses; the third neuroses.

The class of morphæ comprises all abnormal developments, and all morbid changes in the form (μορφη), or nutrition of organs, and all surgical diseases. The families of this class are seven : namely,

Dysmorphæ,	Arrests of development.
Theromorphæ,	Malformations.
Hypertrophia,	Hypertrophy.
Atrophia,	Atrophy.
Stenoses,	Contractions.
Ectopiæ,	Dislocations.
Vulnera,	Wounds.

These families are subdivided into genera. Dysmorphæ includes, *a.* Hydrotrachia or spina bifida. *b.* Hydrocephalus chronicus. *c.* Cryptorchidismus. Theromorphæ has one family only, namely, atresia ani. If we understand Schönlein aright, these forms of abnormal development are mentioned only because others of this class are surgical; to us a reason altogether futile. We can see no characteristic difference between closure of the anus and congenital hernia of the bladder; or between certain forms of hermaphrodism or split penis, and orchidismus, or non-descent of the testicle. Mention ought surely to have been made here of the precocious development of the sexual organs occasionally observed in both sexes, but particularly in females, in whom menstruation has taken place at various ages from eight months to seven or eight years, (referred by Schönlein for this reason to the class Hæmatoses,) with development of the mammæ and other cutaneous structures belonging to the sexual system.

The class of hypertrophied structures is divided into four groups, comprising the nervous, muscular, glandular, and horny structures. We subjoin a *precis* of this portion of Schönlein's nosology.

Class I.—Hypertrophy.

1st Group. Hypertrophy of the nervous system.

Genus 1. Hypertrophy cerebri. G. 2. Hypertrophy of the peripheral nervous system.

2d Group. Hypertrophy of the muscular system.

Genus 1. Hypertrophia cordis. G. 2. H. sphincteris ani.

3d Group. Hypertrophy of the glands and glandular structures.
Genus 1. H. mammæ. G. 2. H. uteri. G. 3. H. ovarii. G. 4. H. prostatæ. G. 5. H. glandulæ thyroideæ. Struma.

The Atrophiæ are arranged as follows: Genus 1. Atrophy of the stomach and bowels. Marasmus. Sp. 1. Marasmus infantum. Sp. 2. M. juvenilis. Sp. 3. M. senilis.
Genus 2. Atrophy of the nervous system.
Sp. 1. Atrophy of the spinal chord. Tabes dorsalis. Sp. 2. Atrophy of the brain. Cretinismus. *a.* C. alpinus. *b.* C. campestris. *c.* C. senilis.
Genus 3. Atrophy of the genitals.
Sp. 1. Atrophia virilis. *a.* A. leprosa. *b.* A. acquisita. Sp. 2. Atrophia genitalis fœminea.

The class of Stenoses is divided into groups thus: 1. Stenosis of the organs of sense. (Some of these belong to the Dysmorphæ.) 2. Stenosis of the alimentary canal. 3. Of the respiratory organs. 4. Of the heart. 5. Of the urinary organs. 6. Of the vascular system. 7. Of the female genital organs. Some of these being surgical diseases are only noticed cursorily: the following are the genera in each group:

2d Group. Genus 1. Contraction of the œsophagus. *a.* Dysphagia sclerosa. *b.* Dysphagia cardiaca. *c.* Dysphagia lusoria.
Genus 2. Enterostenosis. Ileus, or passio iliaca.
Genus 3. Rectostenosis. Stricture of the rectum.

3d Group. Genus 1. Laryngostenosis.

4th Group. Genus 1. Cardiostenosis.

The Ectopiæ is entirely a surgical class, and comprises all forms of herniæ, dislocations, and prolapsus of organs. Schönlein places varices and ecchymoses in this class, as ectopiæ of the blood.

The Hæmatoses constitute the largest and most important class of diseases. Schönlein arranges them into eighteen families, some of which are made up of a most extraordinary medley of affections: hæmoptöe, apoplexy, and menorrhagia, for example, are in the same genus. Indeed the class as a whole exhibits an utter failure of the inventor to carry out his own adopted principles of classification.

The characters of hæmatoses are: 1. Chemical and physical changes in the constituents of the blood. 2. A change in the temperature of the body. 3. If the organ affected be a secreting organ, the secretion is changed, and as a product of disease is characteristic of the hæmatoses. In phlogoses, there is a change in the renal secretion, in hepatitis in the hepatic, &c. 4. There may be changes in the quantity as well as the quality of the blood. As a coup d'œil of the hæmatoses will be worth pages of verbal criticism we present the class in its families, tribes, and species.

CLASS II.—HÆMATOSIS.

FAMILY 1. Erythroses—plethora.
a. E. Vera (spissitudo sanguinis.) *b.* E. Neonatorum. *c.* Menstruatio precox.

FAMILY 2. Phlogoses—inflammations.

1st Group. Inflammations of the vascular system.

Genus 1. Arteritis. *a.* Acute general arteritis. *b.* Chronic general arteritis. *c.* Inflammation of the aorta. *d.* Inflammation of particular arteries.

Genus 2. Phlebitis. *a.* Phlebitis of veins of the upper extremities. *b.* Phlebitis of veins of the lower extremities. *c.* Phlegmatia alba dolens. *d.* Inflammation of the external jugular vein. *e.* Inflammation of the umbilical vein. *f.* Inflammation of the descending vena cava.

Genus 3. Carditis. Cardiac inflammation. *a.* Pericarditis. *b.* Carditis serosa. *c.* C. rheumatica. *d.* C. polyposa. *e.* C. arthritica. *f.* C. scorbutica.

Doubtful forms. *a.* C. syphilitica. *b.* Hydrargyria.

2d Group. Inflammation of the nervous system.

Genus 1. Encephalitis. *a.* E. traumatica. *b.* Meningitis. *c.* Arachnitis (acute and chronic.) *d.* Encephalitis vera, or phrenitis. *e.* E. insolationis. *f.* E. potatorum. Delirium tremens. *g.* Delirium traumaticum. *h.* Encephalomalacia—softening of the brain.

Genus 2. Inflammation of the spinal chord. *a.* Spinitis. *b.* Meningitis spinosa. *c.* Myelitis vera.

Genus 3. Neuritis. Inflammation of nerves.

a. Ischias postica.

3d Group. Inflammation of the organs of respiration.

1st Division. Inflammation of the mucous membrane.

Genus 1. Laryngitis. 2. Tracheitis. 3. Bronchitis.

2d Division. Inflammation of the cellular tissues.

a. Pneumonia traumatic, acute, chronic. *b.* Pleuro-pneumonia; bilious-rheumatic. *c.* Pneumonia venosa.

3d Division. Inflammation of the serous structures.

Genus 1. Pleuritis, acute and chronic.

4th Division. Inflammation of the glandular structures.

Genus 1. Inflammation of the thyroid body. 2. Of the thymus. 3. Of the bronchial glands.

4th Group. Inflammation of the chylopoietic viscera.

1st Division. Inflammation of the lining membranes. A. Above the diaphragm. B. Below the diaphragm.

Genus 1. Odonitis vera. 2. Glossitis. 3. Angina (five species.) 4. Œsophagitis. 5. Gastritis mucosa, serosa, venenata. 6. Enteritis serosa, mucosa (acute and chronic.) 7. Colonitis—simplex—venenata. 8. Proctitis (inflammation of the rectum.) 9. Dysenteria (four species.)

2d Division. Inflammation of the glandular structures of the chylopoietic system.

A. The salivary organs.

Genus 1. Parotitis (five species.) 2. Pancreatitis.

B. The biliary organs.

Genus 1. Hepatitis. *a.* Acute. *b.* Chronic (four varieties.) 2. Lienitis.

C. The mesenteric glands and peritoneum.

1. Peritonitis. *a.* Muscularis. *b.* Membranacea, acuta and chronica. *c.* Miasmatic; puerperal, four species, namely, erythritic, inflammatory, erysipelatous or gastro-bilious, and typhoid.

5th Group. Inflammation of the urinary apparatus.

Genus 1. Nephritis. *a.* Acuta. *b.* Calculosa. *c.* Chronica.

2. Cystitis. *a.* Acuta. *b.* Erysipelacea. *c.* Chronica. *d.* Muscularis.

6th Group. Inflammation of the sexual organs.

A. The female genitals.

Genus 1. Ovaritis. 2. Metritis. *a.* Acuta. *b.* Chronica. *c.* Deformans. *d.* Mucosa.

B. The male genitals.

Genus 1. Orchitis, four species.

7th Group. Inflammation of the locomotive organs.

Genus 1. Myitis. 2. Ostitis. 3. Arthritis. 4. Dermatitis.

FAMILY 3. Neurophlogoses.

1st Group. Neurophlogoses of the nervous system.

Genus 1. Hydrocephalus acutus. 2. Trismus neonatorum.

2d Group. Neurophlogoses of the chylopoietic system.

Genus 1. Stomacace. Gangrene of the mouth. 2. Angina gangrenosa. 3. Gastromalacia, softening or perforation of the stomach.

3d Group. Neurophlogoses of the respiratory organs.

Genus 1. Angina membranacea, croup. 2. Bronchitis maligna. *a.* Acuta. *b.* Catarrhus suffocativus. *c.* Consecutiva. 3. Gangrena pulmonum.

4th Group. Neurophlogoses of the sexual system.

Genus. Metritis septica.

5th Group. Neurophlogoses of the skin.

Genus. Anthrax. *a.* Carbuncle. *b.* A. contagiosa. Allied forms. 1. Pustula maligna. 2. Tinea infernalis.

FAMILY 4. The Typhi. The plague and yellow fever probably are intended to be introduced in this family, instead of the next, to which, by what appears to be a typographical error, they are referred.

Genus 1. T. cerebralis. 2. T. ganglionalis. 3. T. petechialis or exantheticus (four species.)

FAMILY 5. The Cyanoses.

Genus 1. Peliolis, or purpura. *a.* Hemorrhagica. *b.* Rheumatica. *c.* Senilis. 2. Scorbutus. 3. Cyanosis. *a.* Cardiaca. *b.* Pulmonalis. 4. Sclerosis. Induration of the cellular tissue. 5. Hemorrhaphilia Hereditary hemorrhagy. 6. Chlorosis, (allied to the latter is an abnormal formation of fat.)

FAMILY 6. The hemorrhages are divided into groups, including the cerebro-spinal axis, respiratory organs, &c.

Genus 1. Apoplexia cerebralis. 2. A. spinalis (two species.) 3. Epistaxis. 4. Hæmoptöe. 5. Pneumorrhagia. 6. Hæmatemesis. 6. Melena. 8. Proctorrhœa. 9. Mictus cruentum. 10. Metrorrhagia.

FAMILY 7.—*1st Group.* Catarrh of the respiratory organs.

Genus 1. C. simplex. 2. C. contagiosus, or influenza. 3. C. pulmonalis, or blennorrhea. 4. C. senilis, or moist asthma. 5. Emphysema. 6. Morbilli (four species.)

2d Group. Catarrh of the alimentary organs.

Genus 1. Gastro-ataxia. *a.* G. saburralis or indigestion. *b.* G. pituitosa. 2. Febris gastrica. Febris remittens. 3. Febris mucosa. 4. Hepatalgia. Hepatitis nervosa. 5. Gastrodynia biliosa. 6. Febris biliosa. Febris ardens, (three species.) 7. Cholera morbus. 8. Diarrhea, (seven species.) 9. Helminthiasis. 10. Aphthæ, (three species.)

3d Group. Catarrh of the urinary organs.

4th Group. Catarrh of the genital organs.

Family 8. The Rheumatic Affections.

1st Group. R. of the voluntary muscles; containing 11 genera.

2d Group. R. of the involuntary muscles; 3 genera.

3d Group.

Genus 1. Febris miliaria. 2. Sudor anglicanus.

Family 9. Erysipelaceæ, or erysipelatous diseases.

1st Group.

Genus 1. Febris erysipelacea. 2. Angina erysip. *a.* Simplex. *b.* Aphthosa. 2. Rose of the intestinal mucous membrane. 3. Rose of the genital organs.

2d Group. Rose of the skin.

A. Flat roses. Genus 1. Erysipelas (with 11 species and varieties.)
Genus 2. Febris scarlatina, (four species.)

B. Vesicular or elevated roses.

Genus 1. Urticaria, (five species.) 2. Zoster. 3. Variola, (five species.) 4. Rubeola. 5. Pemphigus.

Family 10. Skin Diseases.

We shall only indicate the groups of this and the remaining families.

1st Group. Crypto-impetigines. 2. Acne. 3. Herpes. 4. Porrigines. 5. Psoræ.

Family 11. The Scrofulous Diseases.

1st Group. Scrofula of the lymphatics. 2. S. of the bones. 3. S. of the mucous membranes.

Family 12. Tuberculous Affections.

Family 13. The Phthises.

1st Group. Phthisis of the respiratory organs. 2. P. of the digestive organs. 3. Of the urinary system. 4. The genital system. 5. The nervous system.

Family 14. The colliquative diseases.

Family 15. The dropsical diseases.

Family 16. The Dyschymoses, comprising icterus, urodyalisis, and dysmenorrhea.

Family 17. The Arthritic Diseases.

1st Group. Hemorrhoidal Diseases. *a.* Regular. *b.* Irregular. 2. Podagraform Diseases. *a.* Normal. *b.* Abnormal.

Family 18. The Carcinomatous Diseases.

The class of neuroses comprises the numerous and varied forms of neuralgia, hysteria, and spasmodic and convulsive diseases. Venereal affections are comprised in one family, forming also a distinct class, and with these the volume closes.

On looking over this nosological arrangement, we find as many anomalies as in any other. As propounding a natural system, it seems to us altogether artificial. The leading characters of the hæmatoses, namely, a morbid condition of the vascular system and of the blood, with increase or diminution of temperature, are by no means exclusively characteristic of the diseases grouped together under that designation. In numerous forms of neuralgia, in hysteria, in various forms of asthma, as, for example, the asthma urinosum, these characters are most prominent.

The family of the phlogoses alone are a natural group. But what pathological relation is there between insolation and delirium tremens, in group 2 of the phlogoses, that there is not between insolation and puerperal convulsions in group 2 of the neuroses? or between ischias postica (sciatica) and meningitis in group 2 of the phlogoses, that there is not between sciatica and the neuralgiæ properly so called, placed in the somatic neuroses?

In our opinion, the exanthematic typhus placed among the typhoid fevers, is, in all its pathological characters, one of the exanthematous fevers, and much more nearly allied to scarlatina, variola, or morbilli, than to typhus cerebralis. While measles are placed amongst the catarrhal affections, with influenza, catarrhus senilis, blennorrhea, &c., scarlatina and variola are grouped amongst the erysipelatous, with erysipelatous angina, and rose of the intestinal mucous membrane, in the same genus as urticaria! This breaking up of the class of epidemic exanthemata, so well defined, and so closely allied to each other, is alone a fatal objection to the adoption of Schönlein's nosology in Britain. This is one of the many anomalies we might notice, but these we leave to be detected by our readers, having already provided them the means of forming a judgment.

Special Pathology of Schönlein. Such being the nosology, what is the pathology of Professor Schönlein? We had marked several passages for criticism and animadversion, in which our author states not only erroneous views, but erroneous facts. Take, for example, the following description of cryptorchidismus, or non-descent of the testicle:

"The patient, although at the age of puberty, is imperfectly developed. The body is elongated, the patient being tall, but he is weak, and in appearance childish. The muscles are weak, and not consolidated: the external genital organs diminutive; the penis does not enlarge, the scrotum is contracted, and contains often one testicle only. There is no beard; the voice is unchanged at puberty, is unmanly, or the 'break' is peculiar, mixed up with bass tones. The patient cannot learn to pronounce certain letters, as particularly the letter R. The mind is also affected; it is childish. The testicles remain altogether in the abdomen, or in the inguinal canal, where they form a perceptible tumour, which may be mistaken for hernia. The absence of the testes from the scrotum, the want of manly development, and the absence of all symptoms indicating incarceration of a portion of intestine, are sufficient for distinguishing the two." (Part I, p. 61.)

The symptoms of *atrophy*, or non-development of the testicles, are here detailed. The pathology of cryptorchidismus has been recently discussed

in this Review, (vol. XIV, p. 61,) and all that need be repeated here is, that in the above description, Schönlein betrays not only a want of practical or clinical skill, but also of literary and physiological knowledge. A testicle, if fully developed, is just as efficient within as without the abdomen, and the patient will have a bass voice, a developed penis, and other marks of virility, and be quite able to pronounce the letter R, although they *both* be stuck fast in the groins, if so be that they are so large as to simulate a hernial tumour.

Again: In describing phlebitis, no mention is made whatever of a symptom which, in Great Britain, is considered almost pathognomonic; namely, the formation of abcesses in different parts of the body. Schönlein states that coagulated fibrin is often found in the inflamed vein, and beneath this a pus-like fluid; and this is all. We observe that the *causos* of Aretæus, or *febris ardens*, is described amongst the venous inflammations, as inflammation of the ascending vena cava. We might note other views entertained by Schönlein, as, for example, that scrofulous and tubercular disease are not identical; that tubercles are the result of suppressed secretions or excretions. Thus, he describes menstrual tubercles from suppressed menses, puerperal tubercles from suppressed lochiæ, tubercles from drinking cold water, from repulsion of exanthemata and impetiginous diseases, and from suppressed gout,—the lungs being affected in all cases. Schönlein also objects to the modern doctrine that phthisis is but the continuation and higher development of tubercular disease. It often is so, he argues, when the lungs are the seat of the disease, but hepatic, or gastric, or muscular phthisis, occurs without tubercles having preceded it. In phthisis, according to Schönlein, a morbidly-secreting surface is formed within the organ affected, which secretes a peculiar fluid, commonly termed pus, but which is by no means identical with that fluid. This morbid surface has a great resemblance to mucous membrane. Yet entero-phthisis is described as dependent on *ulcers* of the intestines, which not unfrequently perforate the intestines. Further, phthisis meseraica is simply the mesenteric disease of infants, and phthisis hepatica is neither more nor less than abscess of the liver. Then we have phthisis of the brain, of the ovaries, of the bladder, &c. Here is another instance in which Schönlein sets aside not only the nomenclature generally received by medical writers, but the first principles of taxonomy. Phthisis, as the term is now used, is applied to a disease of the lungs only, accompanied with hectic fever and atrophy. No systematic writer has applied the term to *tabes* generally, to hectic fever, to hepatic abscess, or to mesenteric disease. Schönlein makes phthisis synonymous with hectic fever and marasmus, and then contradicts, as if he had made a great discovery, the general opinion, that phthisis, in its ordinary form, is a higher development of tubercular disease. We certainly have read the arbitrary definitions and reckless contradictions displayed by Schönlein, with very great astonishment.

The Clinical Medicine of Schönlein. Dr. Güterbock having been a clinical student with Schönlein, and so having had an opportunity of hearing and appreciating his clinical discourses, felt certain that good service would be done to the medical commonwealth by the publication of them. That the reader of these clinical discourses may not mistake their

object and spirit, Dr. Güterbock premises a few observations. He remarks, in the first place, that they are not intended for beginners; that they do not profess to afford detailed descriptions of disease, or medicinal formulæ, or rules for prescribing. Schönlein takes it for granted that his hearers are already acquainted with the theory of medicine. He wishes rather to form the ripe student into the scientific practical physician; to teach him how to observe; how to apply his five senses to the detection and comprehension of the phenomena of disease, and his understanding to the elaboration of a true pathology. Schönlein therefore rather sketches the outlines for his hearer to fill up, than draws a finished picture.

Having said so much by way of preface, and as an explanation due to Schönlein, we take for our criticism the first case in the volume, which is one marked as *Typhus abdominalis.*

"2d Nov. 1840. Christian Kämpfer, aged 19, a weaver's apprentice. He states that he has felt poorly and languid for the last four weeks, his feet being too weak to carry his body. He has often experienced vertigo. His sleep was light, and often broken by dreams, and he had an oppressive pain in the forehead. Eight or nine days ago, as nearly as he can remember, he experienced frequent rigors, followed by a continued heat. These data are important, as, previous to this period, was the stage of opportunity, and with it commenced the second seven-day period. We now observe three series of symptoms in the case:

"1 Nervous symptoms, as weariness, dulness of the head, dizziness, tottering gait, sleeplessness.

"2. Symptoms limited to the intestinal mucous membrane. The abdomen is soft, not painful on pressure, even if the cæcal region be strongly pressed. Three more watery stools have been passed during the last 24 hours. The tongue with a white fur was dry at its point yesterday evening.

"3. Symptoms of reaction. The fever is distinctly remittent, the remission occurring in the morning, the exacerbation in the evening. The pulse was 104 yesterday evening, to-day it is 84. The skin is harsh, dry, and in the evening hot. The urine is turbid, and deposits a slimy sediment, not pathognomonic.

"After a comparison of these three series of phenomena, there can be no doubt that the case is one of abdominal typhus in the second seven-day period.

"In modern times, we have heard of attempts to cut short this disease, to render it abortive from the first, or at least to alleviate the symptoms. This method is directly opposed to that which is founded on the principle that, after rigors have once set in, and all the symptoms of the disease are manifested, it must be allowed to run its course, through all its stages. The elder physicians (as Hildebrandt, Stoll, Richter,) have attempted to cut short the disease by emetics, and maintain that these have been the most effectual when gastric symptoms are manifest. I must distinctly express my disapproval of this means, which I have never found beneficial, even when ipecacuan only has been given. So far from cutting short or ameliorating the disease, I believe emetics have had an injurious effect, especially when tartar emetic has been combined with ipecacuan. At least, I found it so, in the case of some strong healthy nurses in the Julius hospital at Wurzburg, who were attacked with typhus fever after nausea, taking cold, &c. An emetic was administered to them, and the tongue became still more coated. The stools were rendered also more frequent, and death in some instances took place on the fourth day. The similarity between this disease and the acute exanthemata ought not to be overlooked. As in the latter the eruptions are the most marked where the skin is irritated, (as, for example, after bleeding in smallpox, the pocks are most numerous around the wound in the vein,) so after the irritation of an emetic on the intestinal mucous membrane, in the former, the eruption is likely to be more vivid. It is for similar reasons that simple saline aperients are so hurtful in this disease. Partly for these theoretical reasons, and partly

from experience, I have come to the conclusion, that the use of emetics for cutting short the affection ought to be abandoned. A more valuable remedy for this purpose is that recently adopted, namely, calomel. To Autenrieth is due the merit of having first used it for this purpose; he administered it so early as 1806 and 1807, continuing its use until the peculiar green evacuations were induced. An objection may be thus raised at once to its use: 'How, after warning us against the administration of the innoxious neutral salts, can you recommend calomel?' It is true that calomel excites alvine evacuations, but they are not such as follow on stimulation of the intestinal mucous membrane. Further: it is allowed that calomel is useful when it excites its peculiar evacuations, which were formerly thought to contain the constituents of the bile, but, according to more modern researches, is altered colouring matter of the blood. The difference between the operation of calomel and of the neutral salts, is further shown by this, that, while the diarrhea increases with the latter, and the stools become more watery, they are more and more consistent with the continued use of calomel; so that at last, aperients (clysters) must be had recourse to. In what stage of the disease ought calomel to be administered? All observations agree that its administration must be limited to the first seven-day period, and the beginning of the second, and that the earlier it is administered, the more notable are its good effects. Its use is contra-indicated when phenomena referrible to the intestinal mucous membrane, and nervous system, as tenderness of the abdomen, dry tongue, and frequent pulse intervene. Given at a late period, it is injurious; it is most useful on or before the fourth day.

"The mildness of the symptoms in the present case leads to the conclusion, that this method of cutting short the disease should be adopted. Opinions vary as to the proper dose of calomel. Some recommend from three to four grains every two or three hours, until the characteristic evacuations are produced: another method (originating with the Tubingen school,) is to give the full dose of a scruple; then to omit it the next day, then on the third day repeat the dose, until the evacuations are less frequent. We have on former occasions followed the former method, let us now adopt the latter; and since calomel is apt to excite acidity in the stomach, we will combine it with eight grains of carbonate of magnesia.

"3d Nov. The patient took a scruple of calomel yesterday at two o'clock, and up to the present time only three characteristic motions have resulted, nevertheless watery motions continue. At first there was vomiting; no tormina. The exacerbation was very slight; the pulse was 96 per minute, and the patient slept for some hours during the night.

"This morning the pulse is only 80: the skin is still tense and drier. The urine deposits a slimy precipitate. Since the kidneys appear inclined to act, while the skin remains inert, to excite the latter, we will give the acetate of ammonia, and early to morrow the scruple dose of calomel shall be repeated.

"4th Nov. Yesterday there was no stool; the exacerbation was still more slight than on the day before (pulse 84,) and this slighter exacerbation was followed by a quieter sleep. To-day the patient feels much stronger, and his head is better, although there is still some intolerance of light.

At half-past five this morning, he took another dose of calomel; but up to this moment (and six hours later,) there have been neither evacuations nor molimina. ..... The abdomen is soft and without pain, the tongue moist, and its yellow coat coming off, the fever moderate as yesterday, the pulse yet strong. (p. 6.)

On the 5th Nov. it is reported that an alvine evacuation took place 14 hours after the calomel was taken, and shortly afterwards three more. The stools were of a brownish or green colour. The pulse was 75, as on the previous evening. Schönlein conceiving the critical day to be near, and the crisis in this disease occurring through the skin, warm bathing and diaphoretic drinks were ordered. On the 6th of Nov. the patient complained of weakness and sleepiness, and seemed more soporose. The

warm bath was followed by a copious sweat, the skin becoming softer. The patient was quiet during the night, but it was rather a state of stupor than regular sleep. The head not better than usual. On the 6th Nov. the same soporose state continued; the tongue coated, but moist; little or no thirst; the abdomen soft, and "the characteristic noise in the cæcal region." The fever and pulse as before. To encourage diaphoresis the dose of liquor of acetate of ammonia was increased from ʒvj to ℥j, with ʒj of the tincture of valerian. In the evening the warm bath, and if the sopor continued, sinapisms to the calves of the legs. From the 9th to the 14th, all the cephalic symptoms were aggravated. On the 9th the patient lay on his back in a state of stupor, from which he could be readily roused, to relapse again immediately. Urine passed involuntarily; head hot; pulse accelerated; abdomen tender on pressure; hemorrhage from the nose and delirium during the night. Leeches and lotions to the head, and a clyster of liquor of acetate of lead and starch. The acetate of ammonia and valerian continued. On the 10th the patient was in the same condition, the pulse small, the speech stammering, the abdomen tender on pressure. Schönlein inquires, "is the congestion of the head consensual, proceeding from the ulceration of the intestinal mucous membrane? or is it the consequence of the fever? or is it idiopathic? The questions are not easily answered." Blisters to the legs were ordered; the saturnine clyster repeated, and infusion of cinchona with gum and oil. On the 11th, paralysis, dilated pupil, and profound apoplexy are reported, and death on the same day.

"*Post-mortem examination.* On the muscular and peritoneal covering of the small intestines there were found small bodies, varying in size from a millet to a pea, and filled with a cheesy substance. In the lower portion of the small intestine, not far from the cæcum, were a few small ulcers, the largest no bigger than a lentil; these were in the act of healing. In the large intestine, about four or five inches from ilio-cæcal valve there were a few similar ulcerations, but a little larger. The affection of the intestinal mucous membrane was much less than is usually noticed in this disease. There were a few miliary tubercles on the pleura pulmonalis. On removing the skull, the convolutions of the brain were found flattened, and the sulci of the pia mater contained a yellowish gelatinous lymph. This was in greatest quantity at the base of the skull, around the chiasma and infundibulum. The vessels were congested; the lateral ventricles, and particularly the left, filled and dilated with fluid, so that the fornix was stretched and appeared softened. Tubercles, the size of beans, were found in both hemispheres of the cerebellum."

Schönlein remarks on the the paucity of ulcers in the intestines. He thinks that no one could justly attribute the cerebral changes to the large doses of calomel, but on the contrary, the post-mortem appearances corroborate the views as to the utility of that remedy in abdominal typhus!

We have given this case at some length, that our readers may have a clear idea of Schönlein's clinical practice and instructions, and that we need not multiply quotations. For ourselves, we can aver that we have written it with surprise and astonishment. The elaborate "Pathology and Therapeutics" of the author had prepared us for a logical, rapid, yet effective examination of the patient; a clear and discriminating view of the symptoms; a separation of what was essential from what was accidental; an exposition of the causal relations of the symptoms: a well-reasoned and decided plan of treatment. Nothing, we need scarcely say, of this kind is in the case before us. The information as to the early history of the patient, his temperament, constitution, expression of countenance,

colour of skin, condition of the pupils, of the respiration, and other points of importance to be observed, are all thought sufficiently described, as Dr. Scharlau observes in the seven words: "Christian Kämpfer, nineteen years old, weaver's apprentice." There is not the slightest indication whatever of the causes of the disease, unless the imagination seize upon the term, "weaver's apprentice," and survey Ch. Kämpfer wearily passing the shuttle to and fro in some dark damp cellar in Berlin, with typhus raging around, himself insufficiently fed, pale, languid, and anemious, previous to an attack of abdominal typhus. Again, the tongue is briefly described as being coated with a whitish fur: a description giving the reader no idea whatever of the true state of the tongue without again drawing on his imagination. Was it a thick coat, or a thin one? Was the tongue dotted with red points? Was it flat, or round, or jagged at the edges, or *tremulous?* On all these points Schönlein is silent.

In fact, the symptoms do not warrant the diagnosis at all as a case of abdominal typhus, taking even his own description of that disease, as given in his Pathology. Abdominal tenderness is one of his pathognomonic symptoms, yet here we have it expressly stated that there was *no* tenderness on pressure, and although we think that abdominal tenderness is in no respect pathognomonic, because we know, from our own experience, that there may be extensive ulceration of the intestinal mucous membrane, without any tenderness whatever, yet we would leave this unstated, and judge Schönlein by his own views. There are a few symptoms referrible to the head, the tongue is whitish, the pulse accelerated in the evening (as it is in all cases,) and scarcely accelerated in the morning, a little feverish heat of the skin, and it is pronounced a case of abdominal typhus.

We will say nothing about the treatment; our readers are well able to judge of that; but we do hope that the Germans will no longer blame English physicians for administering large doses of calomel. From three to four grains is three or four times a greater dose than we ordinarily administer, but when we read of scruple doses!—

Of what disease did the young weaver die? Evidently not of abdominal typhus, as maintained by Schönlein, but of subacute arachnitis, consequent on either hydatid or tubercular disease of the brain. We say *either* hydatid or tubercular disease, because it is impossible to place any reliance on the description of the post-mortem appearances. The tubercles the size of beans in the cerebellum may possibly have been masses of echinococci or acephalocyst hydatids: similar smaller masses may have existed in the cerebrum, lungs, &c., and have been easily overlooked by so careless an observer as Schönlein here shows himself to be. Surely something might have been said respecting the state of the pulmonary mucous membrane of the heart and large vessels, stomach, liver, spleen; of all, or each of which there is not the slightest mention.

Turning from a case of acute to one of chronic disease, we select case 17. This was an individual suffering from a series of symptoms of very common occurrence. In the month of November, 1840, a weaver, aged 34 years, complains that for the last nine months he has experienced pain in the chest, when going up stairs or running, dry cough, palpitation, shortness of breath, and lassitude. He has an appetite, but after eating he feels a sensation of fulness and dragging in the epigastrium. He says he works in a spacious room, and has always been healthy, with the exception,

that about nine years ago, he had the itch for three weeks, which was cured by sulphur ointment; but subsequently he occasionally felt little pimples between the fingers when warm in bed, but which were never permanent. On a stethoscopic examination of the heart, signs of dilatation of the left ventricle, with hypertrophy, and valvular disease, were observed. The left lobe of the liver was found to be tumid, and the feet œdematous, particularly in the evening. The skin and bowels acted regularly, but the urine was scanty, and high-coloured. The case being diagnosed as one of cardiac disease, with partial enlargement of the liver and incipient dropsy, Schönlein next inquires, "what are the causes of this disease?" and the answer is, "we find in the anamnesis no other disease than the itch." Thereupon we have a dissertation on the nature of metapsoral diseases, of the existence of which Schönlein declares he has no doubt whatever. It is impossible to quarrel with a man's convictions, but we must be permitted to declare as firm a conviction on our part, that in this case a three weeks' attack of itch was not the cause of the disease. It is not improbable that rare ablution of the skin, and a neglect of personal cleanliness (so frequently coincident with scabies,) by impairing the cutaneous function, has occasionally induced renal disorder, and from this *fons et origo malorum*, other chronic diseases have arisen, which may thus have been mistaken for the legitimate sequelæ of scabies. We have noticed this case, however, rather to show the mode in which Schönlein hastily jumps to a conclusion in accordance with some preconceived hypothesis, rather than from a scientific and logical inquiry. The whole of the evidence from which the conclusion just stated is deduced is, we assure our readers, before them. There is positively no reference whatever to the condition of the kidneys, no inquiry as to the presence of albumen in the urine, nor in fact any suspicion of it.

In coming to the conclusion, that the three weeks' attack of the itch which occurred nine years previously to the examination of the patient, was the cause of the disease, Schönlein expressly observes, how important it is in the treatment of chronic diseases to know their causes. For this reason, he states why he came to the conclusion just mentioned: 1. The appearance of little papulæ between the fingers, after an attack of scabies, is, according to his experience, characteristic of a metapsoral disease. 2. To the objection, that so great an interval elapsed between the cause and the effect as nine years, he advances that diseases of the heart are insidious, and that in this case, the disease may have existed long before the patient became conscious of it. 3. No other cause to which the cardiac affection might be attributed could be detected: the man had never suffered from any rheumatic affection. We have just indicated a cause of the disease in the disordered renal function, but how could it be possible to learn the causes of the affection without due clinical inquiry? We have not a word as to the habits of the patient; whether he was cleanly or uncleanly, intemperate or sober. We are in ignorance too of the temperament and constitution of himself or his parents. Were the latter subject to rheumatic or arthritic diseases? Was the patient himself corpulent, or "pasty," or lean? Was his hair turning grey, his teeth firm or decayed, his eye dull and muddy, complexion florid or bilious? Had he had hemorrhoidal affections? These and similar inquiries are altogether omitted; but the itch was inquired after, and successfully, for

the chances are rather in favour of a poor weaver, aged 34, catching the contagion some time or other during his 34 years of life; consequently, no other disease can be discovered, and ergo the itch nine years ago was the cause! Can there possibly be a more lame, more impotent, more ridiculously absurd conclusion than this? After five days' treatment, Schönlein at last discovers that his patient had had an attack of rheumatic ophthalmia, previously to the cardiac affection, that his occupation would predispose to hepatic disease, and that he had in fact, on closer inquiry, no aversion to spirituous liquors.

Our readers will ask how the patient was treated. Digitalis, acetate of potass, and taraxacum, were given with the effect of improving the urinary secretion, both in quality and quantity, and with manifest relief to the patient. When it was found, that he was addicted to drinking, the moxa was ordered to be applied about half an inch from the left nipple, and the wound to be kept open as an issue.

We here close our criticism. We need not sum up. The candidate for fame has had a fair reviewal; the facts are before the large and intelligent jury constituted by the readers of the British and Foreign Medical Review. Their decision will, we believe, accord with our own,

The print and paper of the large volume on Pathology are wretched; but we believe Professor Schönlein is not responsible for the publication of this volume, or of that containing his clinical Lectures, as we understand they are given to the public without his imprimatur.

ART. III.

On Diseases of the Liver. By GEORGE BUDD, M.D. F.R.S. Professor of Medicine in King's College, London, and Fellow of Caius' College, Cambridge.—*London*, 1845. 8vo. pp. 402.

DR. BUDD commences his volume with observing that "there are no other diseases of such frequent occurrence, which it is so difficult to discriminate, and for the treatment of which, the medical practitioner has so few trustworthy guides," as diseases of the liver. (p. 1.) Surely diseases of the cerebro-spinal system must be excepted from this remark, of which the diagnosis is as difficult, the pathology as obscure, as any of those of the liver, and which, in a therapeutical point of view, are even more intractable and more perplexing. Still we admit that there is considerable truth in Dr. Budd's observation, and no one of any practical experience can fail to be sensible of the frequent unsatisfactory results of even the most carefully-considered treatment of hepatic diseases.

ANATOMY OF THE LIVER. We presume our readers to be acquainted with all the important points connected with the anatomy and physiology of the liver. Of these the author gives a sufficiently distinct sketch, for which we must refer to the work itself. We may here observe that a few points of curiosity, rather than of practical interest or importance, are still unsettled, in regard to the structure of the liver. Thus it is still *sub lite*, whether the blood of the hepatic artery passes into the extreme branches of the portal vein, *before* entering the hepatic veins. Mr. Kiernan believes that it does. It is not obvious that any change in therapeutics would be rendered necessary by the absolute decision of the question one way or another. Müller, among others, supposed that what has been so long

called the capsule of Glisson, was prolonged, as a sheath of cellular membrane, investing each lobule and separating it from contiguous lobules. Mr. Bowman, whose opinion is paramount with Dr. Budd, thinks he has discovered that there is no areolar tissue between the lobules. These lobules consist of masses of nucleated cells, which, as well as the ducts (which it seems also to secrete,) are the elaboratories of the bile. Among doubtful points, is the exact mode of connexion of the hepatic ducts with these nucleated cells.

As the nucleated cells, along with the capillaries in the meshes of which the cells lie, compose the lobules, and as the lobules compose the substance of the liver, with the exception of the several systems of vessels, it follows that nucleated cells and capillary vessels compose nearly the whole of the organ.

Mr. Kiernan is of opinion that no arteries enter the lobules.

In the nucleated cells Mr. Bowman thinks he has discovered globules of oil or fat, and believes that it is in the increase of the size and number of these globules, that the fatty degeneration of the liver of phthisical and other patients consists. It is probable that these lobules must be, in some way, disintegrated or dissolved before they pass from the nucleated cells into the hepatic ducts.

The Bile.—*Composition and properties.* With the appearance and properties of the bile, we must presume our readers to be acquainted. The degree of fluidity and its colour are extremely various, so that it is, perhaps, impossible to determine exactly what should be considered normal as regards either of these qualities. The darkness of tinge and the degree of dilution of bile depend on the length of time it has remained in the hepatic ducts, or in the gall-bladder. Consequently, several hours after the *completion* of digestion, and more especially after protracted abstinence from food, it is peculiarly dark and inspissated. At other times, its colour varies from yellow to a deep brown-black; but there is generally a shade of green mixed with the yellow, more especially in the bile of the gall-bladder. It feels oily and slightly adhesive, tastes bitter, but has little or no smell. It is readily raised, by agitation, into froth or foam. Schültz stated the specific gravity of ox bile at 1026 to 1030: and we are not aware that there is any authentic determination as to the gravity of human bile. The same physiologist had stated bile to be alkaline; and that one ounce of it, when tolerably inspissated, required one drachm of acetic acid for its neutralization; but Bouisson and Dr. Kemp have more recently determined that when normal and fresh, bile is neutral. When concentrated, it contains grayish-white or greenish particles, and some of the prism-like cells which bestud the mucous membrane of the gall-bladder. To these may be added, cholesterine scales and globules of oil. The former, Dr. Budd seems to think, seldom or never appear except as a consequence of disease of the gall-bladder. The following, according to Berzelius, is the constitution of cystic ox bile:

Water	90·44
Biliary matter with fat	8·00
Mucus	30
Osmazome, chloride of sodium, lactate of soda	74
Soda	41
Phosphate of soda, of lime, and traces of a substance insoluble in alcohol	11
	100·00

Cholesterine, dissolved and in small quantity, appears also to exist in bile, but does not, unless in cystic disease, appear in the form of scales.

Sources and uses of the bile. The views entertained of the sources and uses of the bile have, of late years, undergone very important alterations. At one time, this fluid was regarded as almost wholly excrementitious; another supposed principal use was to promote the peristaltic action of the intestines. The present belief is, that the bile, far from being to a great extent excrementitious, is almost entirely *re*-crementitious; and though it seems probable that the evacuatory movement of the intestines is in part owing to this "natural purgative," as Dr. Budd terms it (p. 30,) yet doubtless that effect is less due to any isolated action of the bile, than to the normal stimulus of the fecal mass, as a whole.

The bile, in common with the excretions of the lungs and kidney, is probably, to a great extent, derived from the decomposition of the tissues of the body, but part of it also is, no doubt, educed from non-nitrogenized articles of food. Carbon is the substance chiefly eliminated by the liver, in like manner as it, along with hydrogen, is the chief constituent of the pulmonary excretion. But an important distinction obtains between the two cases. While the carbon of the lungs is united to oxygen, and excreted in a non-combustible state, the carbon of the liver is non-oxygenized, is still combustible, and is intended, not for excretion, but absorption. But the fact that the two organs secrete carbon, gives them a complementary relation, of which we avail ourselves in therapeutics.

The quantity of bile secreted by a man, daily, has been variously estimated, from a very few ounces to 24 ounces. Before the important and large recrementitious uses of the fluid were ascertained, and when the comparatively small quantity evacuated in the fæces was believed to form the whole amount, the bile secreted in the twenty-four hours was computed at from 3 to 5 or 7 ounces. Yet even Haller, who guessed that the bile served other purdoses than excrementition, estimated the quantity at from 17 to 24 ounces; and this calculation Schültz and Liebig adopt. There appear to us reasons (which we shall not here detail) for believing that from 11 to 14 ounces is the average quantity of the healthy adult secretion of bile, in the space of four-and-twenty hours.

To return now to the consideration of the uses of the bile. These seem to be various. First, then, the liver is, *in part*, an excrementory organ. The resin and the colouring matter are excrementitious matters. Secondly, the liver is a depuratory organ. The abdominal circulation returns through it, and, as Dr. Budd justly observes, (p. 28,) "the blood which has come from the stomach and intestines must necessarily be charged with many impurities besides those derived from the mere decay of the tissues. Along the extensive mucous tract with which everything we eat and drink is brought in contact, absorption is constantly going on, and various matters must therefore enter the portal vessels, not fit by their nature to form blood, or to serve any other purpose in the body. Many of these substances are removed from the blood in its passage through the liver." The part which the bile plays in digestion is more obscure and uncertain. Dr. Budd agrees with those who think that the share which the liver has in assimilation, has been over-rated. According to the most recent and approved views of physiologists and chemists, the *solution* of our food is

all that is required, and not, as was formerly supposed, a mysterious *production* from it of albumen, fibrin, and casein, which it did not previously contain. For we know that food, both animal and vegetable, *does* contain, ready formed, these constituents of the blood. Now, this solution is effected, that is, chymification is completed, without the aid of the bile. It was till lately supposed that one use of the bile was to neutralize the acid of the chyme, and this notion rested on the assumed fact of the bile being alkaline. But if Bouisson's and Dr. Kemp's late researches are accurate, the bile is neutral, and therefore the use referred to is imaginary; unless, as Dr. Budd (p. 30) conjectures, the bile is decomposed in its passage along the bowels, its soda uniting with the acid of the chyme. Dr. Prout's opinion (see Stomach and Renal Diseases, p. 467, ed. 1843,) seems to be, that the chloric acid of the chyme is derived from the common salt existing in the blood and in the stomach: that the salt suffers decomposition; chloric acid is set free, and the soda remains in the stomach, or is absorbed (both of these probably happen) again into the blood. Part of it, a small portion, goes, as Dr. Prout thinks, to maintain the weak alkalinity of the blood: the greater portion returns to the liver, there to be eliminated and to reunite, in the duodenum, with chloric acid. Thus, as it were, an endless cycle of union and separation of part at least of the constituents of the common salt of the blood, takes place. And may we not suppose that something of the same kind happens as regards the carbon of the decomposed tissues, elicited in the bile? Having undergone some purifying process in the course of that elimination, it is fitted for again serving as *respiratory* food, and is, if we may use the phrase, economically reabsorbed. This, supposing the present theories of respiration and animal heat to be true, appears to us the true explanation and history of the series of changes going on in the duodenum.

It must not, however, be supposed that the bile furnishes anything approaching to the principal proportion of the carbon, excreted by the lungs and skin. The quantity of carbon supposed to be eliminated by these two channels, is said to average $13\frac{9}{10}$ ounces daily, (this is Liebig's computation.) Now, the carbon of the bile, even at the highest estimate, does not exceed three ounces; and if our calculation of from 11 to 14 ounces be regarded as expressing the average daily secretion of bile, the carbon contained in it is about one ounce only.

Agents affecting the bile. At the conclusion of his introductory chapter, and before he proceeds to the main business of the work, Dr. Budd makes some pathological and therapeutical observations which perhaps might have been reserved for a subsequent part. Among cholagogue medicines he specially mentions mercury, iodine, muriate of ammonia, and taraxacum. For our parts, we have never observed peculiar cholagogue properties in muriate of ammonia, nor yet in rhubarb, which Dr. Budd subsequently instances. Bonnet enumerates rhubarb as one of the irritating substances, and unskilfully employed by some practitioners in hepatic disease. We cannot say we have found it to be irritating; assuredly we have not found it to be specially cholagogue. We are surprised that in the brief list of drugs particularly adapted for "rendering the liver more active, and increasing in this way the secretion of bile," (p. 35,) Dr. Budd should have omitted colchicum, and even colocynth,

but the former more particularly. Perhaps next to mercurial preparations, there is no agent more remarkable, in that way, than the one just named. Nitric acid also, not here referred to by Dr. Budd, seems in some cases notably to promote the action of the liver.

Diseases of the liver. The first morbid condition which Dr. Budd treats of is congestion. (p. 38.)

Congestion. The common and usual form of congestion of the liver is that of the hepatic vein and its capillaries; and that form may be caused by any disease in the heart or lungs tending to obstruct the return of blood by the hepatic veins. If, while the hepatic veins and their capillaries are thus congested, the portal vein and the capillaries immediately branching from it are empty, the appearance called mottled liver is presented, caused by the central vessels of the lobules being full of blood, while those on their margins are void of it. And just in proportion as the vessels continue to be distended in a direction distal from the heart, and as the portal capillaries which form the margin of the lobules become filled gradually, will the mottled appearance merge into a homogeneous redness. And now, in addition to sanguineous, will biliary congestion begin to take place; and this plainly in consequence, in the majority of cases at least, of the pressure of the distended blood-vessels on the minute branches of the biliary ducts, whereby the discharge of bile along these ducts is impeded. The general result of this state of things is hyperemia, and a state of distension of the biliary vessels analogous to hyperemia, but for which we have no exact name. The liver, of course, becomes enlarged, and its colour, when an incision is made, is a deep reddish-brown or black.

The diagnostic indications of this state are, the dropping of the liver several inches (more or less, according to the degree of enlargement,) below the ribs, and a fulness felt by the patient and perceived by the physician in the right hypochondrium. There is not "in general" pain (p. 41); never, we should say, in this simple form of congestion, until its prolongation begins to give rise to inflammatory action. And even then pain is hardly or not at all experienced, until the peritoneal investments begin to suffer, and the affection becomes what Bonnet calls hepato-peritoneal. (See Article V. of our Number for July, 1843.)

Besides affections of the heart and lungs, the hot stage of ague, and occasionally purpura hæmorrhagica, (if we are to credit Dr. Budd,) are attended with hepatic congestion. Andral states (p. 10, tome 4, of his Clin. Med. ed. 1827,) that he has often observed great congestion of the liver in persons dead of scorbutic disease. In these cases the spleen was also hyperemic, but the congestion of neither organ had any connexion with inflammatory action. He seems to be of opinion that it is in diseases attended with a great diminution of the fibrin of the blood, that congestion of the liver and spleen are most apt to occur.

Congestion confined to the portal system is, according to Mr. Kiernan, who alone gives any account of it, a rare affection, occurring, so far as his experience goes, only in children.

Dr. Budd's remarks on the treatment of congestion of the liver, are extremely concise, and as vague and general as possible. (p. 43.) In congestion depending on obstructed circulation through the heart, he advises bleeding, purgatives, diuretics, rest,—those measures, in short, which

peculiarly relieve cardiac disease. He does not mention blistering and friction over the heart, yet these are important means. "When the liver," he remarks, "cannot free the blood from the principles of bile, or the skin becomes sallow, the patient should carefully abstain from rich meats and fermented drinks, which would render the liver still more inadequate to its office, and increase the biliary disorder." (p. 43.)

Purpura hæmorrhagica, scorbutic disease, and any affections tending to diminish the fibrin of the blood, require, we need not remark, the mineral acids and the bitter extracts. We have witnessed great advantage from the compound infusion and spirit of armoracia, taken twice or thrice a day, with ten drops of nitric or chloric acid added to each dose.

We may remark, before quitting this lesion, that in long-continued congestion of the liver, the nucleated cells, the seats of the biliary secretion, are apt to be obliterated, or at least to suffer in their structure, and, in this way, the function of the liver is liable to be materially and perhaps permanently impaired.

Dr. Budd devotes chapter second to the consideration of "Inflammatory Diseases of the Liver." The first section of the chapter is allotted to *suppurative* inflammation and abscess. Before proceeding, however, to the discussion of the matters just named, he touches on and condemns the usual classification of inflammatory diseases of the liver. The wonted division of such diseases is into acute and chronic, which he characterizes as "essentially faulty," (p. 46;) and on the very just grounds, that it is inaccurate, and corresponds not with, or rather misleads us in regard to, the actual facts. Deep-seated inflammation of the liver may, if not very extensive, run rapidly into abscess, *without* very striking symptoms of any kind, and might therefore be called "*chronic* during the life of the patient, while inflammation involving the surface of the liver, even of such a kind as causes the slow effusion of coagulable lymph only, will be attended with well-marked local symptoms, with great pain and tenderness, and would be termed *acute*." (p. 46.)

The author therefore thinks that, in the case of liver-disease, as well as of every other, morbid affections should as nearly as possible be named from their causes. He therefore divides inflammatory affections of the liver into—1, Suppurative; 2, Gangrenous; 3, Adhesive.

Suppurative inflammation. Perhaps this section on suppurative inflammation and abscess of the liver is the most interesting and important in the entire volume before us. Dr. Budd's object is to show, that by far the majority of cases of abscess of this viscus are owing to "suppurative inflammation of some vein, and the consequent contamination of the blood by pus." (p. 49.) It had been long observed that, subsequently to large wounds or surgical operations, and where suppuration had taken place, pus often formed in remote parts, as, for example, in the interior of joints, in the lungs, in the liver. This was called and supposed to be a *deposit* of pus: it was imagined that the pus was not formed at the place where it was found, but was brought thither from the primary suppurating surface by the vessels, and then deposited or excreted. But when it was ascertained that pus-globules are nearly twice as large as blood-globules, it became manifest that if the vessels deposited the former, they must, *à fortiori*, let the latter also escape. But in these secondary abscesses, while there was purulent, there was no sanguineous extravasation or

deposition. It hence became manifest that the pus must be *formed* at the place where it was found, by the usual process of a local inflammation. The experiments of MM. Dance and Cruveilhier and of Dr. Saunders also showed that the veins must be the channel which transport the globule or globules of pus, which, entering these at some suppurating surface, and becoming arrested in the minute capillary vessels of the lungs or liver, cause those secondary abscesses of which these two organs, both from their peculiar structure and from the fact that all the blood of the body passes through them, are so frequently and remarkably the seat. Now, for the important practical elucidation derivable from the explanation just given, in regard to hepatic abscess so often consequent on dysenteric affections, and on wounds or diseases of the intestines, in which suppuration had taken place.

The author gives a good many cases from Cruveilhier, Andral, Louis, Annesley, and from his own practice on board the Dreadnought, illustrative of the relation of cause and effect, in the majority of cases, between ulcerated states of the intestinal mucous membrane and hepatic abscess. And, in our opinion, he most satisfactorily establishes this connexion. Louis, Andral, and Annesley notice the connexion, and, indeed, it had been long observed; but none of them appear to have surmised the nature of it. Dr. Budd would appear to hold (p. 62) that even in cases where hepatic disease seems to *precede* the dysentery and to cause it, yet that the formation of the hepatic abscess is still consequent on, and subsequent to, the dysenteric affection. In India, there are often cases which commence with biliary disturbance; by and by, fatal and rapid dysentery ensues, and the patient, on post-mortem inspection, is found with abscess of the liver, fully formed. Now, it might at first be suspected that the formation of the abscess, or the peculiar form of inflammation which caused it, preceded the dysentery.* If we understand Dr. Budd aright, (pp. 61-3,) the contrary is the case. The primary hepatic affection was *not* suppurative, but *adhesive* inflammation, the result of spirit-drinking, and liable to terminate in induration and cirrhosis,—seldom or never in *abscess*. But, possibly, the passage of acrid bile, the result of this primary affection, may have caused or predisposed to dysenteric inflammation, and during the resulting suppuration of the intestine, a globule (for often one seems sufficient) or globules, passing up a hemorrhoidal or other intestinal vein, has become arrested in the portal capillaries of the liver, inducing suppurative inflammation with abscess, in addition to the prior-existing adhesive inflammation of the organ.

But it is not necessarily dysenteric affections of the bowels alone, and the consequent ulceration; but ulceration from any cause, and situated in any part, *distal* of the liver, in consequence of which the veins proceeding from the part discharge into the *portal vein*, which may give rise to hepatic abscess. Accordingly we have (at pp. 65-7) cases referred to by Dr. Budd, in which the ulceration was seated in the stomach, in the gall-bladder, and hepatic ducts respectively. The author cites a case of Mr. Busk's, in which the splenic vein was the seat of the primary suppuration or ulceration.

* This view was taken many years ago by Dr. Archibald Robertson in his excellent Thesis (Edin. 1817,) "De Dysenteria regionum Calidarum."

At pp. 70-1-2-3, the author proceeds to confirm the views which we have now described, but there is nothing in this part requiring particular remark.

The "changes of structure," to which suppurative inflammation of the liver leads, is next considered, (pp. 74 et seq.)

Suppurative inflammation of the liver commences in, and often does not extend beyond, the lobular substance. If, indeed, the inflamed part approach, in any direction, the peritoneal surface of the organ, or be closely contiguous to a trunk of the hepatic vein, adhesive inflammation of the peritoneal lining, in the former case, and internal inflammation of the hepatic venous branch, in the latter case, may ensue. However, these secondary results are not usual. Dr. Budd has never seen any branch of the portal vein thus secondarily involved, though Dr. Russell of Birmingham has. The author attributes this difference to the circumstance of the coats of the branches of the portal vein being thicker than those of the hepatic, and also surrounded by areolar tissue.

The abscess or abscesses, if they have led to a *speedily* fatal issue, are bounded simply by red and softened parenchyma; and it may be laid down as a general rule, that, in proportion to the length of time the abscess has existed, (supposing the consequence not to be immediately fatal,) do the walls of the abscess assume more and more density and thickness. Sometimes, however, the contents of the cyst augment, and the pressure causes irritation of the internal walls, followed by ulceration and ultimate gradual extension of the abscess. The abscess, may of course, discharge in any direction; as it reaches the surface of the liver, it excites inflammation in the hepatic peritoneum, by which adhesion between this and the parietal or diaphragmatic peritoneum is effected: or else the adhesion takes place between the hepatic surface and that of the stomach, duodenum, or bowels; or the abscess may discharge, no such adhesions having been formed, directly into the abdominal cavity. This termination, as it is more hazardous, so it is rarer than the others.

The sizes of these abscesses are sometimes amazing. In one of Annesley's cases, the abscess contained 90 ounces: in one of Dr. Inman's of Liverpool, the quantity was 13 pints!

The *symptoms* of suppuration of the liver and of the formation of abscess, are generally obscure or rather equivocal, and sometimes extremely so. Though there occur occasionally, as concomitants of inflammation tending to abscess, pain, fulness in the hepatic region, and jaundice, yet all of these may be absent, or present in an almost unappreciable degree only, and may moreover be caused by other affections; such as irritation or inflammation of the gall-ducts. When the abscess or abscesses are small and central, and when they do not cause any general obstruction to the passage of the bile, neither tumefaction nor jaundice ensue; nor, in the same circumstances, is pain usually felt. Our diagnosis will, however, be materially facilitated should dysentery be present, or there be any suppurating mucous or cutaneous surface, or any extensive wound, accidental or surgical. Pain in the hepatic region, or fulness there, or jaundice occurring in these circumstances, must awake our anxiety and suspicion. The occurrence of a well-marked rigour would, of course, give additional force to other proofs or presumptions.

As regards pain in the right shoulder, which Dr. Budd considers in connexion with hepatic abscess, it occurs, according to our experience, less in acute than in chronic affections, and in all forms of hepatic disease or irritation. Andral and Louis appear to doubt whether it is ever present except when the lung or pleura is affected, and to place little or no faith in it as a sign of *hepatic* disease. It is usually said that it can indicate inflammation of the *convex* surface and of the right lobe of the liver alone, but it was present in a case of Andral's, cited by Dr. Budd himself, in which the abscess was on the *under* surface of the right lobe. "The pain," observes the author, "is, in fact, as it has always been represented to be, a *sympathetic* pain, like the pain of the knee from disease of the hip." (p. 84.) Dr. Budd is, no doubt, aware, that the pain is *accounted* for from the fact, that the phrenic nerve is derived from the fourth cervical nerve, and that this nerve also supplies the muscles and integuments of the neck and shoulder: it follows that, if the diaphragm should be irritated or stretched by enlargement of, or inflammatory action in, the liver, the nerve named from it may be affected; and, circuitously, the branches of the fourth cervical nerve which go to the neck and shoulder. We have often met with cases of hepatic disease in which the pain was felt in the left shoulder, and about the scapular muscles: it may be from the centre or left portion of the diaphragm having been peculiarly affected.

As to rigidity of the right rectus muscle, stated by Mr. Twining to be one of the most certain indications of *deep-seated* (?) abscess of the liver, we, for our parts, have never met with it, unless when the *parietal peritoneum participated in the inflammation of the subjacent hepatic peritoneum.* Then there was a slight apparent tension, probably owing to irritation of the rectus muscle. But why deep-seated abscess of the liver should cause rigidity of this muscle, unless that abscess, by enlarging greatly the bulk of the organ, gave rise to distension of the muscle, and thus elicited natural or spasmodic contraction of it, it were difficult to divine.

As to cough and vomiting, both of which Louis distrusts as symptoms of hepatic abscess, and which Dr. Budd confides in, describing them merely as "sympathetic disorders, depending solely on irritation of the liver," (p. 85,) the former is to be accounted for in the same way as the pain of the shoulder, the cough being simply caused by spasmodic action of the diaphragm, in consequence of irritation of its nerve by the adjacent enlarged or inflamed viscus; the latter occurs when the irritation of the liver has involved the duodenum, and when, besides, this intestine is excited by the acrid bile, which, when inflamed, the liver is apt, in the first stage of inflammatory action, plentifully to excrete.

At page 89, occurs a remark so extraordinary that we must quote, and we shall do so without a single observation. "The fatalness [of hepatic abscess] has no relation to the outlet by which the matter is discharged. I have met with several cases in which the abscess opened through the abdominal parietes, and all of them proved fatal; so that it seems doubtful whether such an opening be more favorable than one into the intestine or lung."

The author's remarks on the treatment of suppurative inflammation

and abscess of the liver are, on the whole, judicious, yet, as we shall presently see, there are some things to be objected to. He advises that, if the inflammation be due to phlebitis, consequent on a wound, &c., means should be taken to prevent the ingress of more pus into the circulation. How is this to be effected? The strength of the patient is also to be supported, and the hepatic inflammation abated, as much as possible, by antiphlogistic means. The author then approaches the *quæstio vexata* of the value of mercury in hepatic affections:

"In this country, mercury has generally been resorted to, when the local symptoms have led to the suspicion that the liver was diseased; but, I fear, with no benefit to the patients. It has been well observed by Abercrombie, 'In the liver-diseases of this country, mercury is often used in an indiscriminate manner, and with very undefined notions as to certain specific influence, which it is supposed to exert over all the morbid conditions of this organ. If the liver be supposed to be in a state of torpor, mercury is given to excite it; if in a state of acute inflammation, mercury is given to moderate the inflammation and reduce its action." (p. 90.)

Now, greatly as we respect the authority of the late Dr. Abercrombie, and that of Dr. Budd, we must yet be permitted to express our opinion, that the use of mercury is defensible *both* in many states of torpor and in many also of acute inflammation of the liver. In many cases of tumid and tender liver, accompanied with equivocal marks of diminished secretion, as for example pale or white stools, no agent more rapidly reduces the volume and restores the action of the organ, than mercury. Again, he would be a bold man, who in almost any case of "acute inflammation" of the liver, except that caused by occlusion of the common duct, would *refrain* from using mercury, to a greater or less extent, and in conjunction, of course, with other means. We, for our parts, should consider that a practitioner who had failed to do so, had hardly given his patient all the professional aid which he might and ought to have done. We do not, of course, advocate the use of mercury *after* suppuration or abscess. Its very object is to prevent the occurrence of these, by resolution. After either of these events has taken place, mercury is to be withdrawn, if it have been used, and all our attention given to support the strength of the patient.

In reference to opening, by incision, abscesses of the liver, we *entirely* agree with the following observations of Dr. Budd; and the result of his experience of the method suggested by Dr. Graves, deserves publicity, though it would have been desirable had he entered into some further explanation and details.

"I would, then, never recommend opening an abscess of the liver, unless assured by circumscribed œdema, or a slight blush on the skin, that union had taken place between the integument and abscess. When these signs are wanting, and the skin has its natural appearance and colour, we can never be sure that adhesions have formed, and if we thrust a knife into the abscess, we run the risk of discharging the matter into the cavity of the peritoneum.

"Dr. Graves has ingeniously recommended a mode of proceeding, by which he supposes this danger to be obviated. It is, not to open the tumour at once, but to make an incision across the most prominent part of it through the abdominal muscles, so as to reach the peritoneum, without dividing it, and to fill up the wound with a pledgit of lint. The object of this is, to excite circumscribed

inflammation of the peritoneum, which may produce adhesion between the reflected layer of the peritoneum and the layer covering the abscess. The abscess is then allowed to open of itself. I have tried this mode of proceeding twice, but with very unsatisfactory results." (pp. 92-3.)

The author, we need scarcely observe, condemns the use of the exploratory needle, as practised in India, and most truly observes:

"An occasional instance of success will, I fear, be a poor set-off against the cases in which the operation has done mischief, or failed of doing good." (p. 94.)

Gangrenous inflammation. The next section of this chapter is devoted to the consideration of "gangrenous inflammation" of the liver. It is very short, but we shall not make any remark upon it, except that Dr. Budd entertains the opinion that gangrenous inflammation is often disseminated from the extremities or from remote parts to the liver, in precisely the same way as suppurative inflammation is shown often to be in the former section, namely, by the septic matter entering a vein, passing to the liver, and there causing the same species of inflammation as the primary one. He gives a case (p. 99,) in which gangrene of the liver, lungs, and spleen, appeared to result from mortification of the toes from cold.

Adhesive inflammation. Section third (pp. 105 to 135 inclusive) is allotted to the description of adhesive inflammation of the liver. After briefly noticing or rather simply enumerating several causes of adhesive inflammation of the peritoneal covering of the organ, by which it is apt to be agglutinated to contiguous parts, the author proceeds (p. 107,) to consider "deep-seated adhesive inflammation." Sometimes the effused lymph is confined wholly to the cellular tissue surrounding the larger portal canals, the other parts of the viscus being healthy; at other times, the effusion of lymph takes place around the smaller twigs of the portal vein, those, namely, which divide the lobules. Hence the whole substance of the liver is rendered tough and somewhat white, by this deposition of coagulable matter, and the capsular as well as the peritoneal investment of the liver, are puckered by the new tissue contracting in the tracks of the *inter*lobular veins, the lobules themselves retaining their volume. This form of adhesive inflammation and pathological change, called "hobnailed-liver" in this country, is designated "*cirrhosis*" by the French, from the Greek word *κιρρος*, fulvus, yellow, which is the colour which the liver presents, in this disease. This colour seems to be owing to the contractions caused by the effused lymph about the small gall-ducts interrupting the flow of bile among these, and thus giving to the lobules in which the bile is retained, a flavous hue.

Cirrhosis. In cirrhosis, the liver is at first much larger, afterwards much smaller than in health. In the first stage of the disease, the effusion of lymph and serum, together with the accumulation of bile in the portal capillaries, causes a large increase in the bulk of the organ; but when the inflammation has run its course, absorption removes the serum: those portions of the lobular substance, from which the escape of bile is permanently obstructed, by the contractions already referred to, become atrophied; and hence the liver subsides to a volume less than the normal. Dr. Budd mentions two facts illustrative of the great augmentation and subsequent diminution of the liver.

"Some time ago, in a case of advanced cirrhosis, I found a band of cellular tissue some inches in length, uniting the liver to the spleen. The adhesions must have formed when the organs were in contact, and have been drawn out as one or other contracted.

"In another case of advanced cirrhosis, I found the convex surface of the liver united to the diaphragm by tufts, or bands of false membrane, an inch in length. The parts of the liver at which these tufts were inserted, were *hollow or depressed*, and when all the tufts were divided, the surface of the liver was very uneven.

"Here, as in the case in which the liver and spleen were united, the adhesions must have formed when the surfaces were in contact, and the bands have been drawn out as the surfaces receded from each other. In both cases, these tufts or bands were evidence of the contraction of the liver, after adhesions had been formed. The degree of contraction being different in different parts, the surface of the liver becomes uneven." (114-15.)

Cirrhosis, both in this country and in France, seems to be chiefly caused by the constant use of alcoholic liquors. Andral considers that the hepatic disease is the consequence either of previously induced irritation on the duodenal mucous membrane, or that the alcohol is directly absorbed and carried into the liver by the portal veins. Dr. Budd regards the latter as the correct explanation. (p. 117.)

Cirrhosis is, commonly, insidious in its progress. It, like most hepatic diseases, is attended with structural change, is not accompanied with much or any suffering, unless the peritoneal investment of the liver is implicated. At other times, accidental circumstances may bring on a sudden access of inflammation, when, the train having been long prepared by spirit-drinking, the change constituting cirrhosis takes place promptly and with emphatic signs of inflammation. More usually, however, as we have remarked, the gradually increasing condensation of the liver and the abolition of the cells and their function in a great part of it, gives rise to sallowness of complexion, pale stools, and at length to ascitic symptoms, owing to embarrassment of the portal circulation. Ascites, indeed, Dr. Budd observes (pp. 121-2,) is an important symptom, because it occurs in few other diseases of the liver. "Frequently, however," he says, further, "together with the ascites, there is œdema of the legs, but unless there be some disease of the heart or of the kidneys, the œdema of the legs is always consecutive to the ascites." (p. 122.)

A jaundiced colour of the countenance is, however, less common, and is less in degree, than in abscess of the liver, for in the latter disease, it is only a part or parts of the organ that are affected; the remainder being usually competent to their function: in the former disease, the whole liver is generally involved.

The livid complexion and the languor accompanying the disease are thus explained:

"The obstructed circulation through the liver serves also to explain in part, the sallow, dingy complexion, so constantly observed in advanced stages of cirrhosis. Part of the portal blood, instead of traversing the liver, finds another way, through the abdominal parietes, to the heart. This part of the blood cannot be purified, or freed from the constituents of bile, as it should be, and must consequently contaminate the whole mass of blood with which it is mixed. In this respect, cirrhosis offers an analogy to those cases in which there is a mixture of venous and arterial blood, in consequence of communication between the two sides of the heart.

"The emaciation and the loss of strength—other constant symptoms in cirrhosis

—depend perhaps in part on impairment of all the assimilating functions, by the habits of life that induce cirrhosis: but they are no doubt mainly owing to the obstructed circulation through the liver, and the imperfect secretion of bile.

"The obstructed circulation impedes, as we have seen, the absorption of water, and also of other substances that contribute to nutrition, by the veins of the stomach and intestines. Imperfect secretion of bile tends to impair nutrition in two ways. The bile, which no doubt performs an important part in digestion, flows in too small quantity into the duodenum, and digestion is in consequence imperfectly performed; and, on the other hand, some of the principles which should be eliminated as bile, remain in and contaminate the blood; causing languor and drowsiness, and weakening in some degree all the assimilating functions." (pp. 124-25-26.)

Cirrhosis is distinguished from chronic peritonitis, by the sallow hue of countenance, the amount of ascitic effusion and the permanency of it, and by the former occurring generally in spirit-drinking subjects, and usually not till after the 30th year. As regards *treatment*, it is obvious, that *after* effusion of lymph into the substance of the liver, no means are the least available. It is only therefore during the stage of the malady *prior* to effusion, in short, during the acute inflammatory stage, that treatment can be serviceable. But, it has been already remarked, that the disease is extremely obscure, in many cases, in its commencement. Hence Dr. Budd's remark is a very just one:

"In the person of a spirit-drinker, we should never neglect pain and tenderness in the region of the liver, especially if associated with some degree of fever." (p. 129.)

Bleeding of course will be requisite, but this measure ought to be practised with the recollection that drinkers are easily depressed by slight depletion. As Dr. Budd observes, an incautious bloodletting may, in such subjects, lead to delirium tremens or some even more formidable affection. Blistering, mercury judiciously employed, combined with the iodide of potassium, diuretics, light tonics, warm clothing, friction, a restricted use of alcoholic drinks, or a total abandonment of it, are the curative means which we must have recourse to. Friction, with mercury, and iodine over the region of the liver will also be expedient.

We pass over, entirely, section 4 of chapter ii, since great part of the information it contains has been incidentally given in the course of the quotations and observations already made. This section is occupied with the subject of "inflammation, suppurative and adhesive, of the branches of the portal vein, and with inflammation of the hepatic vein."

Inflammation of the gall-bladder and ducts. Section 5 (pp. 149 to 195 inclusive) is one of considerable practical interest and importance. In it are discussed various forms of inflammation of the gall-bladder and ducts. Catarrhal and suppurative inflammation is the species first handled. The former of these is a mild, and for that reason, a not easily detected disease. It is seldom or never fatal. "It happens, however, not unfrequently that on squeezing the hepatic ducts, a viscid whitish fluid oozes out, which, on examination, through the microscope, is found to be chiefly made up of the prismatic epithelial cells of the gall ducts." (p. 151.) The author then adds that the symptoms we might "*expect*" in catarrhal inflammation would be slight feverishness, and slight enlargement of the liver and jaundice, slight also, if the ducts were obstructed by the viscid secretion

or the tumefaction of the viscus. In our opinion the disease and its symptoms and effects, are rather conjectural than ascertained.

Suppurative inflammation of the gall-ducts, is a severe affection. In a well-marked case, recorded by Dr. Olliffe of Paris, to which Dr. Budd refers, there were debility, slight nausea, daily rigors; but no jaundice. The gall-ducts through the entire organ were filled with pus; the gall-bladder was filled with pus and bile. Other organs seem to have been healthy.

In some cases part of the small gall-ducts become closed, in the course of suppurative inflammation, and the part beyond, that is, the lobular end of the duct, becomes dilated into a small pouch, being "converted into small permanent cysts, filled with a glairy fluid, more or less tinged with bile." (p. 154.) These may be mistaken for tubercles.

However, the cystic and common ducts and the gall-bladder are more frequently the seat of inflammation than the hepatic ducts. At p. 155, Dr. Budd quotes a case from Andral, which he represents as "the best example he has met with of acute inflammation of the common duct only." From this language, we are led to infer that the common duct was the primary seat of inflammation, and indeed the only diseased part. But, in our opinion, Dr. Budd has entirely mistaken the nature of the case. Andral's words are: "the coats of the common duct were much thickened and easily torn, and the canal almost closed;" and elsewhere, "the entrance of the common duct was marked by a small round tumour, rising three lines above the surface of the intestine, and pierced at its summit by a capillary orifice, the opening of the duct." (p. 157.) Not a word about *inflammation* of the common duct. But—"*the inner surface of the duodenum was intensely red,*" (same page)—and here, according to our belief, was the *punctum saliens* of disease. It confirms this view, that the disease commenced after indulgence at table. The tumefaction of the duodenum had closed the common ducts, but there is no proof that the duct was inflamed, and the current of bile, obstructed, had ruptured the hepatic duct, just beyond its junction with the cystic, giving rise to fatal peritonitis in consequence of the escape of the bile into the abdominal cavity.

In this case of Andral's, and in another quoted by Dr. Budd from the same authority, there was a moveable pear-shaped tumour, immediately under the false ribs. This was the distended gall-bladder. Dr. Budd's comments on these two cases are deserving of quotation:

"There can be little doubt that this case, like the former, was one of acute inflammation of the common duct, not extending to the gall-bladder. The symptoms in these cases were just what might have been expected: pain in the situation of the duct, followed, at the end of one or two days, by jaundice and by distension of the gall-bladder; a certain degree of fever; constipation. It is worthy of remark that in neither case does Andral notice among the symptoms, vomiting, or nausea, or rigors

"It is probable that similar cases now and then occur, and are treated as inflammatory jaundice, without their real nature being discovered. The symptoms differ from those of ordinary cases of jaundice, chiefly in the pain being limited to a small spot in the situation of the common duct, and in *the early appearance of a large, moveable, pear-shaped tumour,* not painful or tender, which may be recognized by these characteristics as the gall-bladder distended from closure

of the common duct. The absence of pain or tenderness of the tumour, shows that the gall-bladder is not inflamed.

"If the inflammation should involve the cystic and hepatic ducts, as well as the common duct, distension of the gall-bladder would perhaps not take place, and the symptoms would be merely those of inflammatory jaundice." (p. 158.)

Inflammation of the gall-bladder, and even permanent structural change in it, are not necessarily fatal, provided the cystic duct remains patent.

Rokitansky has observed, in "the secondary fever of cholera" and as a result of typhoid fever, what he calls *croupal* or plastic inflammation of the mucous membrane of the gall-bladder and ducts. "It produces," to use Dr. Budd's words, (p. 163,) "within the gall-ducts, membranous tubes, in which the bile (?) forms tree-like concretions." It is a rare affection.

The author next makes a few remarks on, and gives a case from his own practice, and that of Andral, and Cruveilhier, and Mr. Bowman respectively, illustrative of ulceration of the gall-bladder. This form of disease and gall-stones are often found coexistent; and though, no doubt, the latter sometimes cause the former, yet Dr. Budd well remarks, that in many cases "it seems most reasonable to refer both ulcers and gall-stones . . . . to an unhealthy state of the bile." (p. 164.) This inference is legitimate, in those cases in which the gall-stones are found not resting on the ulcers, and when it is recollected that the gall-stones *float* in the bile, and are found thus floating after death.

It is important to keep in mind, "that ulceration of the gall-bladder, like ulceration of other mucous surfaces which return their blood to the portal vein, may lead to scattered abscesses of the liver." (p. 175.)

At pages 180 et seq. we have some observations on occlusion of the cystic and common ducts. Closure of the cystic ducts most commonly occurs from a biliary calculus, which, floating in the bile of the gall-bladder, is carried into the duct, gets impacted there: inflammation may then set in and fix it, or it may pass through the walls of the duct. If the stone remain fixed in the duct, or should the duct be permanently closed by any other cause, the bile in the gall-bladder is absorbed, (supposing the bladder to have been healthy,) and a glairy fluid alone remains. The bladder also shrinks to the size of the smallest pear, or even to that of an almond. But, from various cases, it appears that the effect of closure of the cystic duct simply, may be but little detrimental to digestion, to health, or to longevity.

Occlusion of the common duct is an occurrence much more serious and urgent. If the duct have been closed suddenly, and the secretion of the bile have been going on actively, and in a liver comparatively healthy, the rapid distension of the gall-bladder and ducts may be such, that rupture of one or other ensues. Not a few cases of this kind are on record. If the closure of the common duct has been gradual, the bile, of course, slowly distends the gall-bladder and ducts throughout the liver, which thereby acquires a deep brown colour. It may happen (in this slow form of occlusion) that before rupture anywhere takes place, the structure of the liver is destroyed, and the nucleated cells are either broken down or absorbed. This occurred in a case recorded by Dr. Thomas Williams, and still more remarkably in a case recorded by the author himself, (pp. 183-7.)

In cases of complete closure of the common duct the liver generally enlarges at first, but by and by, if life lasts for some time, it subsides into a size more or less below the natural.

Though our limits will not permit us to quote the case referred to in the conclusion of the second last paragraph, we must yet give the reader the benefit of one or two useful practical comments which the author makes on it:

"The constipation, and the relief she derived from purgatives, and once from diarrhœa, that occurred without purgative medicine. Much of the pain and tenderness of the belly of which she complained, was probably owing to distension of the intestine by fæces and gas, and to irritation of its mucous membrane by the contact of matters chemically different from those natural to it.

"The fetid urine, which was at times turbid with pale lithates, but never had a *pinkish sediment*. The absence of a pink sediment may help to distinguish such cases from cases in which the common duct is closed by the pressure of a cancerous tumour, and in which a sediment of this tint is often observed in the urine.

"But perhaps the most striking circumstance of all was that, although for a long time before death the liver must have ceased to separate bile from the blood, there were no symptoms of cerebral poisoning, and the mind remained clear to the last. This circumstance will appear still more remarkable, if we compare this case with other cases in which suppression of bile is attended with delirium, or with stupor and convulsions, soon ending in fatal coma. Dr. Alison, in a paper published in the Edinburgh Medical and Surgical Journal for 1835, has collected many cases of this latter kind, and from a review of them he concludes that it is jaundice from *suppressed secretion*, and not from obstructed gall-ducts, that is peculiarly, if not exclusively, liable to be followed by delirium, coma, and speedy death. He explains this, by supposing that 'the retention in the blood of matter destined to excretion, is much more generally hurtful to the living body than the *reabsorption* into the blood of matters which have been excreted at their appropriate organs, but not thrown out of the body, in consequence of obstruction at their outlets.' The fact is, I believe, correct, but Dr. Alison's explanation is not satisfactory, since in this case, for a long time before death, there could have been no bile secreted, and yet there was no disorder of the brain.

"The case further shows us, that the secretion of bile by the liver is not immediately necessary to life; that a person may live a considerable time when the liver, as an instrument of secretion, is completely destroyed. This destruction of the secreting element of the liver proves fatal, however, in the end, by impairing nutrition, and causing gradual but progressive wasting." (pp. 188-90.)

Dr. Budd refers to a case of Mr. Busk's, on board the Dreadnought, in which, in the author's opinion, no bile had passed into the bowels during four years! (p. 190.)

Dr. Budd repeatedly, throughout his work, refers to the fact that disease of the gall-bladder, more especially fatty degeneration, is "always attended with a large secretion of cholesterine, ... which frequently leads to the formation of gall-stones, and all the evils they occasion." (p. 191.) The author sums up his history of the inflammation of the gall-bladder and ducts, with the remark that the immediate cause of most of the forms of inflammation of these parts, is the passage of irritating bile or gall-stones.

The author's remarks on treatment are concise and rather vague. Leeches, blisters, mercury, not with the view of obtaining its "more powerful and constitutional action," (p. 194,) but of increasing the quantity of bile, and rendering it less irritating, which the author conceives

mercury, judiciously administered, effects: diluents which, "by filling the stomach" may "empty the gall-bladder by their pressure," and also by "passing out again of the circulation by the liver," may thus "dilute the bile;" soda which, in the catarrhal form of inflammation of the ducts, may "probably render the secretion from them less viscid;" taraxacum which, according to the author, "like cholagogue medicines generally, is more likely to be under than over-rated," (p. 195)—(is such Dr. B.'s opinion?)—these are the means suggested in the work before us for the cure of the lesions to which our attention has been directed.

Chapter third, which occurs next, is one of considerable interest.

Fatal jaundice. It occasionally happens that fatal jaundice occurs in cases where, on a post-mortem examination, the gall-ducts are found patent, nor are there any of those effusions which indicate that inflammation has been at work. In such cases we must infer that the jaundice is not caused by obstruction to the passage of bile, in so far as the hepatic ducts throughout are concerned; nor can we suppose, of course, that the morbid changes which we sometimes detect in the liver, are owing to the current of bile being thrown back. On the contrary, the jaundice, in the cases we are now discoursing of, is the consequence of no bile being secreted, the cell-laboratories in which that secretion is carried on being either greatly diminished in number, or in great part destroyed. In some cases, however, while the cell-apparatus remains, so far as we can judge, in perfect condition, yet these secrete no bile. This is a mysterious paralysis or stoppage of the *function* of the cells, the precise cause and nature of which our present pathological knowledge does not enable us to appreciate.

In those cases in which the cells are diminished in number or disorganized, there are sometimes seen, on examining sections of the liver with the microscope, irregular collections of oil-globules, biliary particles, and amorphous granules.

The functional suspension of secretion is often due to sudden, violent, and depressing moral emotions; but according to the author, "it is far more frequently (?) produced by some poison introduced from without or generated in the body by faulty assimilation or digestion." (p. 224.) The author notices one point in diagnosis which we shall quote:

"When delirium, or coma, or convulsions supervene, we may be almost sure that the jaundice results from suppressed secretion; because these symptoms seldom occur in jaundice that results from mere obstruction of the ducts." (p. 255.)

In regard to this form of hepatic disease, we cannot find fault with the author's customary brevity of therapeutical remark:

"Until more is known of the causes of this form of disease, and until it can be detected with more certainty, we cannot expect to have satisfactory proofs of the good or ill effects of particular plans of treatment. The conclusion that may be most safely drawn from the foregoing cases is that, in some instances, coma may probably be prevented or removed, and the life of the patient saved, by active purging." (pp. 225-6.)

We pass over entirely sections 2d and 3d of this chapter (third,) in which are respectively discussed fatty degeneration and scrofulous enlargement of the liver. In section 4th we come to the extremely interesting

subjects, excessive and defective secretion of bile and unhealthy states of that secretion.

Biliary derangement. The causes of derangement of the biliary secretion are stated to be structural disease of the liver, or morbid states of the portal blood from medicines, unwholesome food, "faulty digestion or assimilation," or the deficient action of some other excreting organ. (p. 256.)

Excessive secretion of bile is characterized by bilious stools, ardor fæcum, nausea occasionally, bitter taste, yellow tongue. A more severe form of the disease conjoins to the symptoms just enumerated, fever more or less lively. The fever is probably owing to the acrimony of the bile irritating in its passage the gall-ducts and small intestines; for, observes the author (p. 258,) "There can be little doubt that the bile, while it is increased in quantity, is also altered in quality and irritating," a conclusion to which we only partially assent. The cure for these forms of biliary irritation and excitement are saline purgatives, *no* calomel (as we think,) though the author and Mr. Annesley recommend it; diluent drinks, and phlebotomy or cupping, which Mr. Annesley directs, but which we think need be resorted to *only* if there is *considerable* fever and hypochondriac pain, provided purgatives and abstinence be practised.

To the medicinal treatment the author appends certain hygienic suggestions so proper and useful, that we shall present them in his own words:

"These measures are generally sufficient for the time, but they do not strike at the root of the evil. Exemption from future attacks, and from the manifold and greater evils to which these disorders may lead as age advances, can only be procured by a change of habits. One of our objects in directing this should be to increase the amount of oxygen inspired, and thus to consume in respiration, or burn off, materials that would otherwise be left for the liver to excrete. The means most efficacious for this purpose are sea-voyages, riding, or other exercise in the open air, well-ventilated rooms, early rising, the cold or shower-bath, &c. Too much indulgence in sleep, which so much reduces the activity of both respiration and circulation, must be especially injurious, more particularly in rooms that are ill-ventilated, as most bed-rooms are.

"Another object of equal or still greater importance, should be to limit in the food the supply of those materials—such as spirituous liquors, butter, cream, fat, sugar—which contribute directly to form bile, or which increase the quantity of bile indirectly, by serving as fuel for respiration. Some of those aliments, as cream and porter, for instance, seem to be not only pernicious in this way, but also by directly embarrassing the secreting function of the liver.

"From these considerations it follows, that it must be especially injurious for persons who suffer from the disorders we are considering, to indulge in sleep immediately after a full meal. To lessen by sleep the activity of respiration at the very time when the materials consumed in this process are being poured in large quantity into the blood, must lead in a twofold way to accumulation of bile in the system, and favour the occurrence of a bilious attack. In this way may be explained the ill effects of suppers in disorders of this class, and the well-known fact that a single indulgence of this kind may bring on a bilious attack, in a person predisposed to it." (p. 259.)

Deficiency of bile is the next derangement. This, according to the author, may be of two kinds: one in which enough of bile is eliminated by the liver to purify the blood, but not to effect digestion properly;

another, in which there is not enough of bile secreted either to purify the blood or produce perfect digestion. In the former case, digestion is slow, nutrition imperfect, the bowels irregular, "and the contents of the large intestine often become too acid." (p. 260.) Dr. Budd attributes the disease to too great abstemiousness, (hear this, ye abettors of feeble, insipid diet!) and regards the cure to lie in "less abstemiousness." Alas! how few diseases can boast of the *cause!* to how few is the *cure* suitable!

Another form or variety of this first kind sometimes occurs in young people. There is diarrhœa which resists all sedatives and astringents. The discharges are entirely *un*tinged with bile. As soon as the bile flows, the disease stops. Dr. Budd thinks the disease owing to irritating matter causing a temporary spasm or inflammation of the mouth of the common duct. The irritating matters referred to the author states to be morbidly acid, and that relief is afforded by magnesia. The author further quotes from Dr. Prout a description of a somewhat similar spasm of the ducts, attended with intolerable headache, which ceases so soon as the released bile is supposed to have reached the cæcum, showing the affection probably to depend upon unnatural acidity of the contents of that portion of the intestine.

The second form of deficient secretion of bile is when this fluid "may be in excess as regards the intestines (p. 263,) .... and yet may be in too small quantity to purify the blood, and the complexion be bilious or sallow." We have noticed frequently such a state of matters. The author states the cure to be "a dose of calomel and a few brisk purgatives." We have not found the affection always yield to such homely treatment.

The second and more serious variety of this second form of deficient secretion of bile is, when the inadequate action of the liver results from any of the inflammatory changes of the organ, noticed in the former part of this article, such as adhesive inflammation of the areolar tissue surrounding the branches of the portal vein, and impeding the passage of bile along them, or interstitial fibrinous deposit, as in cirrhosis; or else from any cause by which the function or structure of the nucleated cells is abolished. In such cases, of course, the defective secretion of bile is a mere effect of a specific change, and can only be cured by the removal of that change.

But though, as the author observes (p. 264 et seq.) health cannot, in these circumstances, be perfectly restored, something may be done to prolong life and to palliate the situation of the patient. All articles of diet tending to favour the formation of bile are to be abstained from; much pure air is to be inhaled, by which the lungs are made to supplement the liver, thus relieving the latter organ of some labour; a pill of aloes and rhubarb to be taken habitually at dinner; and certain medicines to be employed, some of which appear to act by making less secretion of bile necessary; others by increasing the activity of the liver, thereby making up for the disorganized condition in which part of the organ is, by the augmented action of those portions of it which are still sound. Of the former sort of medicines nitro-muriatic acid, used externally and internally, is a specimen: among the latter, the author cites mercury and taraxacum.

A short notice of "unhealthy states of the bile" next follows. On this

subject there seems to be extremely scanty information. In cirrhosis and the fatty degeneration of the liver, the colouring matter and the bitter taste of the bile are somewhat wanting. But the author has found the same peculiarities in cases in which the liver itself was quite healthy, but where there was granular kidney in one instance, and purulent phlebitis with pulmonary abscesses in another. (pp. 267-8.)

In other cases the bile is dark and inspissated. This is more especially the case in diseases of India, where the bile seems to be secreted in a more concentrated form, if the word may be used, than with us, and where the occasional irruption of such bile, after temporary retention in the gall-bladder and ducts, often causes great and even dangerous disturbance.

Gall-stones. Section 5 is devoted to the consideration of gall-stones. Our notice of this section and of the remainder of the work must be exceedingly brief.

There are two kinds of gall-stones, the encysted, those namely, found in the substance, as it seems of the liver, and those found in the gall-bladder. The former, called hepatic gall-stones, are usually very small, and merely consist of inspissated bile and mucus; they result from inflammation and stoppage of some hepatic duct or ducts on the side next the gall-bladder. Gall-stones also of similar kind, namely, of inspissated biliary matter mixed with mucus, are found in the gall-bladder itself. When in this situation, and if unincased in cholesterine, they show that the bladder is healthy, for otherwise cholesterine would be deposited around them. When the gall-stone, as sometimes happens, is entirely composed of cholesterine, it is white, and resembles a ball of camphor. (p. 274.)

Gall-stones are sometimes single, and the size of a hen's egg; sometimes in numbers of several thousands and proportionally small. Where there are but a few, they come to resemble (as remarked by Haller,) the small bones of the wrist, in consequence of the attrition one with the other.

It is an important fact that the gall-stones found in the same bladder usually resemble each other. Their nuclei and laminæ are the same: if one shows a layer of cholesterine, all do so. Hence we must infer they were all formed simultaneously. To what serious and permanent consequences may even a temporary abnormal condition of the bile give rise!

Gall-stones always indicate a deranged condition of the bile at the time they were formed. They almost always consist of cholesterine, and biliary matter and mucus. They seem seldom to result from organic disease; never, according to the experience of Dr. Prout and the author, from the granular change of the organ caused by dram-drinking. They are sometimes, however, found associated with cancer of the liver, or with the cancerous *diathesis.* The deposition of cholesterine seems to be associated with fatty degeneration, not of the liver, but of the coats of the gall-bladder.

There seems a greater liability in women than in men to gall-stones. They most usually occur in persons of full habit, who use rich food and take little exercise. Dr. Prout remarks a relation between the tendency to cholesterine gall-stones and the uric-acid deposits.

A gall-stone may remain in the bladder or even in the ducts (provided it does not interrupt the passage of bile,) for an indefinite period without causing any pain, or a single symptom to indicate its existence. Its passage, however, along the cystic and common ducts, is usually attended with exquisite suffering. The pain comes on suddenly, and is usually not continued but paroxysmal. There is often vomiting. If the stone is long in passing, and becomes impacted in the common duct, jaundice ensues. If it remains permanently impacted there, death may take place from coma, or inflammation may occur in the duct, the stone along with bile escaping into the cavity of the abdomen; or, what more usually happens, adhesion takes place with some neighbouring organ, often the stomach or duodenum, into one or other of which the calculus eats its way. Another occasional result is that the gall-bladder, suddenly distended beyond what it can bear, bursts, and its contents escaping into the abdominal cavity, the patient perishes from acute peritonitis.

The most safe and, fortunately, the most common occurrence, is that the stone slowly and painfully passes the *straits* of the common duct, and emerges into the duodenum, with *instantaneous* and *complete* cessation of the patient's sufferings.

Of course inflammation and fever may occur in the course of this process, if protracted and severe. To combat these is almost all we have to do or can do. Dr. Prout suggests, as a mode of giving relief, large draughts, repeated, of hot water (if necessary with some laudanum in it,) containing carbonate of soda. Fomentations with hot water and poppies, or, as some direct, pounded ice, is to be applied to the epigastrium. A warm bath also gives relief sometimes. Leeching of the hypochondriac region is proper, if the pain is extreme. Antispasmodics are also proper with a view of promoting dilatation of the duct.

In regard to stones presumed to remain still in the bladder, and in respect to the formation of new ones, Dr. Budd thinks that *soda* may have a tendency to accomplish the solution of the former, by forming "a soluble compound of cholesterine." (p. 296.) The second end is to be attained by regulating the bowels by "the habitual use" of rhubarb or rhubarb and aloes, by mild saline purgatives, as Püllna water; by keeping up the action of the skin by the warm bath; lastly, by an occasional use of mercury; all which suggestions have in general our concurrence.

Chapter iv is occupied with diseases of the liver which result from some growth foreign to the natural structure; and section first is devoted to cancerous tumours and "encysted, knotted tubera" of the organ. (p. 299.)

Cancer of the liver. We do not mean to pause upon these matters. One remark of Dr. Budd's, however, takes us by surprise: "No serious organic disease of the liver is in this country (at least among people who have never drunk hard,) so frequent as cancer." Can Dr. Budd furnish any statistical proofs of this?

Cancer appears sometimes ubiquitously and simultaneously; sometimes it is plainly successive and disseminative by the lymphatics. Dr. Budd shows this in various parts of his work. (p. 310, et seq.) We shall quote some brief remarks on the diagnosis:

"We are ignorant of the conditions which dispose to primary cancer of the liver, or which immediately excite it, so that in the diagnosis of this disease, we are little helped by knowing the previous habits of the patient, or the circumstances in which he has lately been placed. We know only that the disease does not occur before the age of thirty-five. In persons above this age it can only be discovered by the intrinsic import of the symptoms. But in the early stages of the disease, and while the liver is still shielded by the ribs, the symptoms are vague, and such only as are common to various derangements of this organ. They may justly excite our fears, but they cannot give us assurance that the liver is the seat of cancer.

"The most significant symptom is enlargement of the liver. When this comes on in the middle period of life, and especially when it is progressive, and when other conditions that may equally give rise to it are wanting,—when there is no obstacle to the circulation in the chest, when the patient is not consumptive, and when his habits have not been such as to lead us to suspect that he may have cirrhosis,—it affords of itself strong presumption of the presence of cancerous tumours. When the liver is of very great size, and its surface can be felt to be nodulous or uneven, there is no longer room for doubt.

"Another symptom which is of very frequent occurrence, and which may help us to distinguish this disease from some others in which the liver is likewise enlarged, is constant pain and tenderness.

"A small, permanent collection of fluid in the cavity of the peritoneum, when there is no reason to believe it to be the result of cirrhosis, is another significant token of the presence of cancerous tumours in the liver. A large quantity of fluid in the peritoneum is less significant of itself, and it may even increase the difficulty of diagnosis, by preventing our feeling the large and nodulous liver.

"When cancer of the liver is consequent on cancer of some other part, its detection is much easier, because, from our knowledge of the frequent dissemination of cancer, symptoms which are in other circumstances trivial, then acquire great significance. In a woman who has ulcerated cancer of the breast, with the general symptoms of the cancerous cachexy; or in one who has cancer in the uterus which has eaten into the intestine; or in a person who has presented for some time the symptoms of cancer of the stomach,—pain and tenderness in the region of the liver, or a slight increase in its volume, with jaundice, or slight ascites, or even one of these symptoms, are evidence enough that cancerous tumours have formed in this organ. The same symptoms, occurring soon after an injury to the head, or after amputation of the leg or arm, together with the constitutional symptoms of suppurative phlebitis, would scarcely leave a doubt that abscesses were forming in the liver. Our conclusions are drawn, not so much from the intrinsic value of the symptoms, as from the significance which these derive from the circumstances under which they occur." (pp. 325-7.)

Jaundice. Though jaundice is justly stated by the author to be a mere symptom, and though it is, on various occasions, incidentally referred to in previous parts of the work, yet Dr. Budd thinks it proper (and justly so,) to devote to its consideration the concluding chapter of his work. He here collects into one view all the notices on the disease scattered through other parts of his volume. We shall endeavour to give an extremely condensed recapitulation of a few of the principal particulars.

Jaundice, as is well known, arises either from the retention of the colouring matter of the bile in the blood, or from its absorption. In many of the circumstances causing jaundice, the yellow hue of the skin has both of these origins. It seems still somewhat doubtful whether bile, as bile, exists in the blood, or only its elements. Lecanu, whose researches

into the composition of the blood have been of a very rigorous kind, has only detected the colouring matter. The non-secretion or the absorption of bile is, after a time, attended with diminution of the number of the red globules; but this effect seems to be rather an indirect consequence of impaired nutrition, than owing to any immediate influence of the bile on the blood.

In approaching jaundice, the presence of bile in the urine is sometimes detectible, before the yellow hue of skin is manifested, by the test of sulphuric acid, which causes a greenish colour in the urine, changing to purple or red.

As the yellow colour of the skin is some time of showing itself, so it often remains after traces of bile have disappeared from the urine and *re*appeared in the fæces; in short, after the action of the liver has been restored. We must therefore be cautious not unnecessarily to persist in the use of mercury or other measures, on account of the continuance of the yellowness of skin merely, which diaphoretics and warm baths will gradually remove.

In regard to the causes of jaundice Dr. Budd, after mentioning gallstones as one of these, observes: "A more frequent cause of jaundice from obstructed gall-ducts is cancerous disease of the liver, or of the pancreas," which he adds is permanent. (p. 381.) We can scarcely admit with Dr. Budd as to the greater frequency of jaundice from cancerous disease than from gall-stones.

Many cases of jaundice are owing to the structural affections of the liver described in the earlier part of Dr. Budd's volume. These it is needless to recapitulate. The general inference deduced by the author is of practical importance, namely, that in jaundice, springing as it may do from such a great variety of causes, and these of such various shades or rather kinds of disease, the prognosis must be very difficult until we have arrived at certainty as to the lesion on which the jaundice depends. This is in some cases much more easy than in others. The previous habits of the patient in a case of cirrhosis; the sudden incidence of symptoms marking the passage of a gall-stone; or the gradual appearance and persistent presence of jaundice without pain or stupor in the course of the disease, indicating some gradual obstruction of the ducts, and not a case of suppression, that is non-secretion of bile, which is always attended with coma; the accompanying circumstances now detailed, may enable us, in cases characterized by them, to come to pretty definite conclusions as to the cause of the jaundice. But in many other cases the diagnosis and prognosis will be far from easy.

Jaundice, *per se*, requires of course no remarks as far as regards treatment, which must have in view the cause, and not the symptom.

We have thus brought to a conclusion our notice of Dr. Budd's able and practical volume. We have no hesitation in pronouncing it an opportune and useful publication. Were we to find any fault, we should say that, in the treatment of the subject, pathology commands too much attention, therapeutics too little. The work is well written, at least perspicuously written; it pretends to no peculiar elegance, liveliness, or finish of style, nor to any great philosophy of thought or doctrine. But

the language is clear; there are no redundant matters or language; and on the whole, this treatise on "Diseases of the Liver" is one which none but a man well versed in his profession could have composed. And we do not doubt that the tolerably full digest which we have given, and the quotations we have made, will induce our readers to seek in the work itself that large amount of valuable pathological facts and inductions which we confidently promise them.

ART. IV.

1. *Odontography; or a Treatise on the Comparative Anatomy of the Teeth; their Physiological Relations, &c.* By RICHARD OWEN, F.R.S. &c. &c.—*London*, 1840-45. Royal 8vo, pp. 656. With 168 Plates.
2. *Report on the Microscopic Structure of Shells.* Part I. By WILLIAM B. CARPENTER, M.D. F.R.S. 8vo, pp. 24. With 20 Plates.
3. *Observations on the Structure of the Shells of Molluscous and Conchiferous Animals.* By JAMES SCOTT BOWERBANK, F.R.S. &c. Vol. I.—*London*, 1844. Royal 8vo, pp. 34. With 5 Plates.

IT will be in the recollection of our readers, that, in our last Number, we devoted a considerable space to the analysis of Prof. Owen's admirable researches on the comparative structure and development of the teeth; of which the publication is now complete. Our survey embraced a general account of the elementary types of Dental tissue; together with an outline of the principal forms which the apparatus presents, in the classes of fishes and reptiles. We now take up the subject where we left off; and, passing over the class of birds, whose total want of teeth leaves us nothing to say on the subject, we proceed with the dental apparatus of mammals; after which we shall give a brief notice of the recent discoveries made in a corresponding field of investigation, by Dr. Carpenter and Mr. Bowerbank.

MAMMALS. It is in the class of mammals, that the Dental apparatus presents its most special development, and is of the greatest importance in classification. This importance may be over-rated however; and has actually been, in Prof. Owen's opinion, by Frederick Cuvier. It is requisite to bear in mind, that there is a considerable diversity in the limits of variation, among different orders; thus the dental system of the *Cetacea* and *Bruta* has a much greater range of variation, and a less constant relation to the other characters on which the families and genera are founded, than in the ungulate and higher unguiculate groups.

"It is true, indeed, that the most manifestly natural mammalian genera are those, the species of which are provided with absolute similar molar teeth; and that those genera, which include species with molars of different forms, do not present the same character of unity. But it does not follow that, by combining species of mammals with similar molars, a group will be formed perfectly analogous to those which may be considered as the most natural or perfect. Neither the molar teeth, nor any other solitary character will serve to establish a natural classification.

"The molar teeth will least mislead in this respect, where their modification is most extreme, as when they are adapted to divide the flesh of animals, in which case they must of necessity be associated with the faculties and instruments for seizing and destroying prey. But molar teeth may be similarly modified, and

equally well-adapted for crushing vegetable substances, which substances may be sought for by one species on the dry land, by a second in marshes, and by a third in the sea, or on the banks of rivers. The grinding surface of the molar tooth, for example, may for this purpose be elevated into a pair of transverse ridges; and we find such molars in the kangaroo, the tapir, and the manatee, as also in the extinct *diprotodon, nototherium,* and *dinotherium*. The small anterior molars of the *mastodon giganteus* likewise present this form. It would be difficult to select from the mammalian class the constituents of the more heterogenous group, than would be constituted by the character which M. F. Cuvier has assigned as the true guide in the formation of the most natural and uniform genera in mammalogy." (Introduction, p. lxxi.)

Even in regard to teeth adapted to carnivorous habits, if these characters were to form the sole guides in classification, species of placental mammalia would be associated with those of the ovo-viviparous sub-class. This was actually done by F. Cuvier; and other zoologists have followed in the same track with wonderful pertinacity. There is no limit to the absurdities which result from the adoption of any single character as the uniform guide in classification, however valuable such a character may generally be as an exponent of others. There is certainly no single character, which can be so much relied on in the arrangement of Mammals, as the conformation of the teeth; yet, as we have seen, the exclusive adoption of it leads to the most erroneous results. Next to the teeth, we may rank the extremities, as serving to afford important grounds of union or distinction; but to rely exclusively upon these, as is done by a British zoologist of some eminence, is to bring together the monkeys and opossums, because they have an opposable thumb, although in other respects they are nearly at the opposite extremities of the scale; and to reunite the carnivorous and herbivorous cetacea, which all truly scientific zoologists have agreed to separate, on account of their difference in all *essential* characters, their agreement being only in those which *adapt* them to the fish-like form and habits.

The class of Mammals, like the preceding, includes a few species which are entirely devoid of teeth, or of any representatives of those organs; these all belong to the group of anteaters, with the exception of the monotreme Echidna, which resembles the rest in regimen, though differing so remarkably in its generation. A few mammals have the jaws provided with horny substitutes for teeth, as the whalebone-whales and the ornithorhyncus. In the rest of the class, true teeth are present; these are always confined to the jaws; but the lingual and palatal teeth of reptiles and fishes are not without their representatives.

"In the feline tribe, the epithelium of the tongue is thickened at the fore part of its dorsum, and invests the papillæ there with hard sheaths like prickles, which are analogous to the lingual teeth of certain fishes and batrachians. The back part of the dorsum of the tongue in the echidna is provided with a plate of horny denticles, which bruise its food against the hard and prickly epithelium covering the palate. Horny processes, analogous to the palatal teeth of fishes and reptiles, are likewise developed upon the roof of the mouth of the great bottle-nose dolphin, thence termed *hyperoodon* by Lacépède." (p. 296.)

The average *number* of teeth in mammals, is that which characterizes man, the apes of the old world, and the true ruminants;—namely, 32. In some species of cetacea, however, this number is reduced to two; and in

the narwhal or sea unicorn, only one is ordinarily developed, but this grows to an unusual length. The elephant has never more than one entire molar, or parts of two, in use on each side of the upper and lower jaws; making, therefore, at the most, eight molars in all; to which we have to add the two tusks, developed in the intermaxillary bones. The examples of excessive number of teeth are presented, in the order *Bruta*, by an extinct armadillo, which has 98 teeth; and among the *Cetacea*, by the cachalot or sperm-whale, which has upwards of 60 teeth, though most of them are confined to the lower jaw,—by the common porpoise, which has between 80 and 90 teeth,—by the gangetic dolphin, which has 120 teeth, —and by the true dolphins, which have from 100 to 190 teeth, yielding the maximum number in the class Mammalia. Where the teeth are in excessive number, as in the species just cited, they are, as among reptiles and fishes in similar circumstances, nearly alike in form; being for the most part simply conical, and evidently destined rather for the prehension than for the mastication of the food. In several of the bruta, again, such as the smaller armadillos, the orycterope, and the three-toed sloth, in which the anterior teeth are deficient, the remainder are nearly all of one form, being sub-cylindrical with broad triturating surfaces; they differ, however, in size. In almost all the other Mammalia, particular teeth have special forms for special uses; thus, the front teeth, from being commonly adapted to effect the first coarse division of the food, have been called cutters or incisors, and the back teeth which complete its comminution, grinders or molars; large conical teeth, situated behind the incisors, and adapted by being nearer the insertion of the biting muscles to act with greater force, are called holders, tearers, lamaries, or more commonly canine teeth, from being well developed in the dog and other carnivora, although they are given, likewise, to many vegetable-feeders, for defence or combat. These names are not, however, by any means indicative of the shape of the crowns of the teeth which occupy the respective positions just described; and it has been found necessary to consider the position, and also the mode of succession of the teeth, in order to make them of definite use in classification.

"Comparative anatomists, by common consent, now apply the term '*incisor*,' arbitrarily to those teeth which are implanted in the intermaxillary bones, and in the corresponding part of the lower jaw. When the tooth which succeeds the incisors, or the first of the upper maxillary bone, is conical, pointed, and longer than the rest, it is called a '*canine*,' as is also its analogue in the lower jaw, which always passes in front of it when the mouth is closed Of the remaining teeth, those which are shed and replaced vertically, or which have successors descending into their place in the upper, and ascending in the lower jaw, are called '*premolars*,' or false molars, and in human anatomy '*bicuspides*;' the remaining teeth, which are not displaced by vertical successors, but which follow each other from behind forwards, in both jaws, are called '*molars*,' or true molars." (p. 298.)

The ordinary teeth of mammalia have so much more definite and complex a form than those of fishes and reptiles, that we can usually recognize in them the *crown* or exposed part, as not only distinct from the *fang* or inserted part, but as divided from it by a constricted portion or *neck*. In those teeth which grow uninterruptedly, however, the exposed part is not separated by a neck from the implanted part, which generally maintains

to its extremity the same size and shape as the implanted crown. It is peculiar to the class Mammalia to have teeth implanted in sockets by two or more fangs; but this can only happen to the teeth of limited growth, and generally characterizes the molars and premolars. In no mammal does anchylosis of the teeth with the jaw constitute a normal mode of attachment, as in the cold-blooded vertebrata. Each tooth has its particular socket, to which it firmly adheres by the close co-adaptation of their opposed surfaces, and by the firm adhesion of the alveolar periosteum to the organized cement, which invests the fang or fangs of the tooth; but in some of the cetacea, at the posterior part of the dental series, the sockets are wide and shallow, and the teeth adhere more strongly to the gum than to the periosteum; so that Prof. Owen has seen, in the cachalot, all the teeth brought away with the ligamentous gum, when it has been stripped from the sockets of the lower jaw. True teeth implanted in sockets are confined, in this class, to the maxillary, intermaxillary, and lower maxillary bones, and to a single row in each. In general, teeth are situated in all these bones; but they may be absent in the intermaxillaries, as in the ruminants and most bruta; or they may be present in those bones only, as in the narwhal. In man, the intermaxillaries coalesce with the maxillary bones at an early period; it was formerly supposed that he was the only mammal in which there is no interspace in the dental series of either jaw,—a character resulting from the shortness of the jaws, and the equal lengths of the crowns of the teeth; but the importance of this character as associated with the peculiar attributes of human organization, has been somewhat diminished by Cuvier's discovery of a like contiguous arrangement of the teeth, in the jaws of the extinct *Anoplotherium*. For a general account of the *substance* of the teeth of mammals, we shall have recourse to Prof. Owen's unabridged description.

"The teeth of mammalia usually consist of hard or unvascular dentine, defended at the crown with an investment of enamel, and everywhere surrounded by a coat of cement. The coronal cement is of extreme tenuity in man, quadrumana, and terrestrial carnivora; it is thicker in herbivora, especially in the complex grinders of the elephant; and is thickest in the teeth of the sloths, magatheroids, morse, and cachalot. Vertical folds of enamel and cement penetrate the crown of the tooth in ruminants, and in most rodents, and pachyderms, characterizing by their various forms the genera of the two last orders; but these folds never converge from equidistant points of the circumference of the crown towards the centre. The teeth of the quadrupeds of the order bruta have no true enamel; this is absent likewise in the molars of the dugong, zeuglodon, and the cachalot. The tusks of the narwhal, walrus, elephant, mastodon, and dinotherium, consist of modified dentine, which in the large proboscidian animals is properly called 'ivory,' and is covered by cement.

"The central part of the fully-formed tooth in man and most other animals, contains an irregular kind of osseous substance, which is most abundant in the cachalot, and forms around foreign bodies which may gain admission to the pulp-cavity of tusks. A fifth substance which, from the number and regular position of the vascular canals in it, I have termed the 'vascular dentine,' forms the body or axis of the tooth in the sloth tribe, and is present in smaller proportion in the centre of the teeth of the armadillos. The teeth of the *orycteropus* consist of congeries of long and slender prismatic columnar denticles, each consisting of a body of dentine, with a coating of cement, by which they are united together to form a composite tooth, as in some of the cartilaginous fishes." (p. 302.)

"The dentine of the long and slender prismatic denticles, which are aggregated to form the compound tooth of the orycterope, is unvascular, and is characterized chiefly by the frequent division and wide angles at which the branches of the bifurcating calcigerous tubes diverge, before resolving themselves into their minute wavy terminations." (p. 303.)

The resemblance between one of the composite teeth of the mammiferous orycterope, as figured in plate 78 of the Odontography, and that of the saw-fish shown in plate 9, is very remarkable; and it shows what care is requisite in drawing general conclusions respecting the limitation of special forms of dental structure to particular groups, until the examination has been most extensively prosecuted.

As the account of the dental structures, which was contained in the preceding article, was principally founded upon the characters which they exhibit in mammalia, we need not stop here to go over the same ground again; but have only to add to our generalities a few words regarding the peculiarities of dental development in this class.

The primary pulp, which first appears as a papilla on the free surface of the gum, and by the calcification of which the dentine is formed, sinks into a cavity, and becomes surrounded by a closed capsule, at an early period of the formation of the tooth, in every mammal. It is also completely enclosed in the substance of the jaw-bone; from which the crown of the growing tooth extricates itself by exciting the absorbent process, whilst the cell is deepened by the same process and by the growth of the jaw, into an alveolus for the root of the tooth. In those teeth which possess enamel, the mould or pulp of that constituent is developed from the capsule covering the coronal part of the dentinal pulp. In the complex teeth of the herbivorous and some omnivorous quadrupeds, the enamel-pulp sends processes into depressions of the crown, which vary in depth, breadth, direction, and number, in the several groups; the dentinal pulp, thus penetrated, offers corresponding complications of form, and as the capsule follows the enamel-pulp in all its folds and processes, the external cavities or interspaces of the dentine become occupied by enamel or cement,—the cement, like the capsule which formed it, being the outermost substance, or having the enamel interposed between it and the dentine. The dental matrix presents the most extensive interdigitation of the dentinal and enamel pulps, in the capybara and elephant.

The ordinary process of the formation of the fang is, in its essential characters, the same with the continued production of the tooth which takes place in certain cases, as the tusks of the elephant, and the front teeth of the rodentia; the only difference being, that the pulp is reproduced in perpetually decreasing quantity in the former case; whilst it is regenerated below, as fast as it undergoes conversion above, in the latter.

"In the formation of a single fang, the activity of the reproductive process becomes enfeebled at the circumference, and is progressively contracted within narrower limits in relation to a single centre, until it ceases at the apex of the fang; which, though for a long time perforated for the admission of the vessels and nerves to the interior of the tooth, is in many cases finally closed by the ossification of the remaining part of the capsule. When a tooth is destined to be implanted by two or more fangs, the reproduction of the pulp is restricted to

two or more parts of the base of the coronal portion, around the centre of which parts the sphere of its reproductive activity is progressively contracted. The intervening parts of the base of the coronal pulp adhere to the capsule, which is simultaneously calcified with them, covering those parts of the base of the crown of the tooth with a layer of cement. The ossification of the surrounding jaw being governed by the changes in the soft but highly-organized dental matrix, fills up the spaces unoccupied by the contracted and divided pulp, and affords, by its periosteum, a surface for the adhesion of the cement or ossified capsule, covering the completed part of the tooth." (p. 306.)

The following general statements in regard to the *succession* of teeth in the mammalia are extremely interesting. We have already noticed the discovery, first made we believe by Meckel, that the matrices of the permanent or secondary teeth of man are offsets, as it were, from those of the deciduous or primaries, whose places they take. The first permanent molar, however, originates from a primary matrix; this, it will be recollected, makes its appearance *behind* the two deciduous molars, which are replaced by the bicuspids. The second and third permanent molars, which come up successively behind the first, are offsets from it; or rather the second is a sort of bud from the first, and the third from the second, The following extract will show the relation between this history in man, and that which takes place in other mammalia:

"The matrix of certain teeth does not give rise, during any period of their formation, to the germ of a second tooth, destined to succeed the first; this, therefore, when completed and worn down, is not replaced: all the true cetacea are limited to this simple provision of teeth. In the armadillos, megatherioids, and sloths, the want of germinative power, as it may be called, in the matrix, is compensated by the uninterrupted growth of the teeth. In other mammalia the matrix of the first-developed tooth gives origin to the germ of a second tooth, which sometimes displaces, sometimes takes its place by the side of its predecessor and parent. All those teeth which are displaced by their progeny are called temporary, deciduous, or milk teeth: the mode and direction in which they are displaced and succeeded, viz. from above downwards in the upper, from below upwards in the lower jaw, in both jaws vertically, are similar to those in the crocodile; but the process is never repeated more than once in any mammiferous animal." (p. 307.)

The following extract relates to the cases in which the gemmiparous process allows the newly-formed teeth to come up by the side of their parents, as in the case of the second and third true molars of man:

"In this successive germ-production, we find repeated the multiparous property of the dental matrix of the crocodile; but the concomitant growth of the jaw allows the second, third, and sometimes even fourth generation of true molars to coexist, and come into place side by side. In the unguiculate and most of the ungulate species of the placental division of the mammalian class, the fissiparous reproduction of horizontally-succeeding teeth stops at the third generation; in other words, they have not more than three true molars on each side of the upper and under jaws. In the marsupial series the same process extends to a fourth generation of true or horizontally-succeeding molars; and in most of the species, the four true molars are in use and place at the same time; but in the kangaroos, the anterior ones are shed before the posterior ones are developed. This successive decadence is still more characteristic of the grinding-teeth of the elephant, which consist exclusively of true molars." (p. 308.)

We are rather surprised that Prof. Owen should not have included,

amongst his general remarks on the teeth of mammalia, some notice of the curious arrest of development, which accounts for the absence of teeth in many species; but we may partly supply the deficiency by extracts from the subsequent detailed account of the dental apparatus in the several families of mammalia:

"The great whales, before they acquire their peculiar array of baleen plates, manifest in their fœtal age a transitory condition, a true dental system, which, though abortive and functionless, beautifully typifies that which is normal and persistent in the majority of the order. In an open groove, which extends along the alveolar border of both the upper and the lower jaws, there is a series of minute, conical, acute, or obtuse denticles, with hollow bases inclosing the uncalcified remains of a vascular pulp. These were first noticed by the philosophical anatomist, Geoffroy St. Hilaire, and have subsequently been described and figured by Prof. Eschricht. The subject examined by the latter author was the fœtus of a balænoptera, the jaws of which were about four inches in length. The inclosed alveolar groove of the upper jaw contained twenty-eight denticles, that of the lower jaw forty-two. The anterior denticles in both jaws were the smallest; but they increase in size more gradually, and maintain a greater regularity of form, in the lower jaw, where they are most numerous, and in which the typical dentition of the carnivorous cetaceans first manifests its plenary development in the great cachalot. In the upper jaw of the fœtal whale some of the denticles are double, two adhering side by side; and they offer in their varying extent of confluence no unapt resemblance to the stages of the fissiparous multiplication of an infusorial monad. There can be little doubt, indeed, that these literally 'double-teeth' have resulted from a spontaneous fission of the primordial pulp-cell, the divisions of which growing to a certain size, have again coalesced in the progress of their calcification, like the two primitively detached summits of the ornithorhyncus's grinder. We cannot avoid recognizing in these bicuspid denticles the representatives of the gigantic extinct cetacean, called *zeuglodon;* and they also call to mind the similarly-shaped ultimate molar in the dugong.

"These small teeth and their matrices entirely disappear before birth; yet the fetal whale comparatively long retains and palpably exemplifies the earliest stage of dental development in the higher mammals, viz., the open fissure, which in these is so rapidly closed, especially in the human subject. But beyond this stage the true dentition of the balænidæ is not destined to proceed; and they thus manifest, agreeably with the general laws of unity of organization, their closest relations to the typical characters of their order at the early periods of development, divesting themselves of part of the more general type, in order to assume their special and distinctive characters as they advance towards maturity." (p. 347.)

A similar development of tooth-germs which are never to attain maturity, presents itself in the ruminants, in which it was first detected by Mr. John Goodsir in 1839. The number of *germs* of deciduous teeth in this group is in all 32; namely, *six* molars, *two* canines, and *eight* molars, in *each* jaw. But the number of teeth which actually appear is usually only 20; namely, *six* incisors, *two* canines, and *six* molars in the *lower* jaw; and *six* molars, without either incisors or canines, in the upper. The rudiments of the deficient incisors and canines of the upper jaw were discovered by Mr. Goodsir in the embryos of the cow and sheep; and those of the deficient molar have been detected by Prof. Owen, very near the canines, in the fœtus of a fallow-deer. These rudiments do not usually attain to that stage of calcification which distinguishes the rudimental hidden teeth of the whales; but some of them go on to their full

evolution, in certain aberrant forms of the order. Thus in the camel tribe, the third or outermost of the upper incisors, together with the canine and the first premolar, are completely calcified, and have permanent successors; and in the musk-deer and the muntjac the permanent canine attains unusual size, and becomes a formidable weapon; whilst the primitive rudiments of all their upper incisors and first molars disappear, without giving origin to any successors.

We do not find any notice of similar conditions in the early condition of that group of animals, which was designated by Linnæus as *bruta*, and which has received from Cuvier the less appropriate name of *edentata*. Comparatively few of them are truly *edentate*, or destitute of teeth; whilst, on the other hand, only two or three species have any teeth in the intermaxillary bone. It would be very interesting to know if the general rule of the presence of the germs, even where teeth are not developed from them, would hold good in this instance also; but opportunities for obtaining the necessary specimens, in the right stage of their development, are not of frequent occurrence.

It is in vain for us to think of following our Author, even in the most cursory manner, through the detailed description of the dental apparatus in mammalia, which occupies more than half of the entire work; and we must content ourselves, as we have been obliged to do in regard to the two previous classes, with adducing one or two remarkable examples in illustration of the value of his researches, and of the generalizations which he founds upon them. We alluded in our former notice to the important assistance which had been given by Prof. Owen's microscopical analysis of the structure of the teeth, in the determination of the marsupial nature of the Stonesfield fossil; and to the new light it had thrown on the character of certain remains, which had been appealed to in support of the reptile affinities of that fossil. Those remains, first described as saurian by Dr. Harlan, were at once referred by Prof. Owen to the mammalian type, on account of the implantation of the teeth by double fangs in deep sockets; the microscopic analysis of the teeth confirmed this view, and indicated the cetacean affinities of the fossil, which was at that time only known by fragments of the jaws; and the subsequent discovery of a nearly complete skeleton has demonstrated the truth of Prof. Owen's deductions. The name of *zeuglodon* has been given to this animal from the peculiarity in the form of the posterior teeth, which are formed of two lobes united by a narrow isthmus, so that a transverse section presents somewhat the form of an hour-glass. In Prof. Owen's opinion, the pulp was originally simple, and became subsequently divided into two parts by a sort of fissiparous reproduction, but the complete separation never took place, as it does in the cetacea usually. After the division of the pulp has taken place, its calcification proceeds by two distinct centres, which are each separately surrounded by concentric striæ of growth. From the other remains of this animal which have been discovered since the examination of its teeth by Prof. Owen, the cetacean nature of the animal is fully demonstrated; and its place is shown to be at the extremity of the series of carnivorous whales, approaching the dugong and other apodal pachyderms. Its length must have been near seventy feet. Here then a case, which seemed to call in question Prof. Owen's gene-

ralization respecting the mammalian nature of all two-fanged teeth, implanted in a distinct socket, was found, when fully investigated, to afford a powerful support to it.

The structure of the teeth in the various recent and fossil species of the order *bruta,* affords many points of great interest, of which we may stop to notice a few. We have already alluded to the remarkable organization of the teeth in the orycterope, or Cape anteater, certain peculiarities of which did not escape the notice of Fred. Cuvier. These teeth, which are in reality made up by the union of several distinct denticles, are continued solid and of the same dimensions to the bottom of their sockets, where they terminate by a truncated and undivided base, appearing as if destitute of a pulp-cavity at every period of its growth. This anomaly, however, is more apparent than real; for though the tooth as a whole has no such cavity, each of the component denticles has its base excavated by a conical pulp-cavity, as in other animals; and this pulp is persistent, as in the rest of the order bruta. The wide inferior apertures of these cavities constitute the pores observable on the base of the compound tooth of the orycterope, and give to that part a close resemblance to the section of a cane. The vascular pulp becomes ossified near the grinding surface of the tooth, and consequently a transverse section taken from this part presents the centres of the radiation of the calcigerous tubes filled up with bone. The base of each pulp is surrounded by a delicate vascular capsule, which becomes ossified about a line above the base, and forms the cementing substance of the congeries of denticles. Hence:

"The teeth of the orycteropus offer, when rightly understood, no anomaly or exceptional condition in their mode of development. Each denticle is developed according to the same laws, and by as simple a matrix as those large teeth in other mammalia, which consist only of dentine and cement. The dentine is formed by the centripetal calcification of the pulp, and the cement by the ossification of the capsule. Both pulp and capsule continue to be reproduced at the bottom of the alveolus, *pari passu* with the attrition of the exposed crown; and the mode and time of growth being alike in each denticle, the whole compound tooth is maintained during the life of the animal. The augmentation in the size of the whole tooth, during the growth of the jaw, is effected by the development of new denticles, and by a slight increase of size in the old ones at the base of the growing tooth, which in the progress of attrition and growth, becomes its grinding surface." (p. 320.)

We alluded in our former notice to the remarkable fossil genus *glyptodon,* whose extraordinary tesselated armour was long supposed, by many zoologists, to appertain to the gigantic *megatherium.* The two animals are now known, however, chiefly through the discoveries of Dr. Lünd and Mr. Darwin in South America, and Prof. Owen's investigations at home, to be quite distinct; the former being referrible, as might have been supposed from its armour, to the armadillo group, and the latter being the representative of the extraordinary family of *gravigrades,* of which the existing sloths are diminutive arboreal forms. Many of our readers, we hope, have seen and admired the gigantic cuirass of the glyptodon, which was set up a few years since in the Hunterian Museum, after the expenditure of a vast amount of time and ingenuity in the recomposition of the scattered elements. This extraordinary beast seems to have surpassed the rhinoceros in size; but in its dentition it was adapted to a more exclu-

sively vegetable diet than its allies among the armadillos, which are somewhat omnivorous. Remains of at least six species, differing most characteristically in their *tails*, have been recently added to the British Museum, and will shortly, we hope, be displayed to the public. The teeth of the armadillo tribe are harder than those of any other species of the order *bruta;* but, as in all that order provided with teeth, they are wholly devoid of cement. They consist, in both the existing and extinct armadillos, of three distinct substances, of which the unvascular dentine is present in greatest proportion, and forms the main body of the tooth; but it includes a small central axis of vascular dentine, and is surrounded by an extremely thin coating of cement. The calcigerous tubes of the dentine are continued from the vascular or Haversian canals of the central axis; many of them appear to anastomose, either directly or by intermediate cells, close to the cement; others are directly continued into that substance, and terminate in its cells. The canals of the central axis are few in number, have a regular or parallel arrangement, and do not anastomose by loops; some of them are continued along with short processes of the osseous substance a little way into the dentine; and a rich brush or pencil of calcigerous tubes radiates in strong irregular curves from the obtuse ends of these processes, before falling into the common parallel course. The following are the differences presented in the structure of the tooth of the glyptodon:

"The vascular osseous texture occupies a larger proportion of the centre of the tooth than in the small armadillos; it is harder than the dentine or cement, and rises upon the grinding surface in the form of a ridge, extending along the middle of the long axis of that surface, and in three shorter ridges at right angles to the preceding, at the middle of each of the three rhomboidal divisions of the tooth. The medullary canals are surrounded by fine compact concentric strata, but are wider than in true bone, and the calcigerous cells are fewer and less conspicuous: the canals bend towards the dentine, but without any regular or parallel arrangement. The calcigerous tubes assume their parallel and regular course sooner than in the armadillos; they have the same relative diameter, arrangement, and terminal ramification. The outer coat of cement, though not exceeding 1-10th of a line in thickness, is relatively thicker than in the armadillos: a large proportion of the terminal branches of the calcigerous tubes is continued into it, and these branches anastomose with the plexiform tubes which surround the calcigerous cells.

"Although the teeth of the largest of the extinct loricated bruta present so much more complicated a form than do those of the small existing species, their intimate texture and composition are essentially the same, and very distinct from what has been described in the orycteropus, and will subsequently be shown to characterize the dentition of the family of the sloths. Nevertheless the modified proportions of the constituent textures of the molars of the glyptodon, which produce the inequality in the grinding-surface of the crown, coincide with the more complicated form of that surface in adapting the tooth for a more strictly vegetable diet, than is indicated by the more simple molars of the existing armadillos." (p.325)

It is interesting to find this inference confirmed, by the presence in the glyptodon of a descending process of the malar bone, which in the sloths affords the masseter muscle a more extensive and favorable attachment for producing those horizontal triturating movements of the lower jaw, which are necessary for the comminution of their food,—consisting as this does of the leaves and young shoots of trees. If we restrict our survey of the

dental system to existing mammals, that of the sloths appears anomalous, both as respects the number, the form, and the intimate composition of the teeth. But when their type of dentition is viewed in connexion with that of the extinct bruta of the megatheroid family, the repetition of all its essential characters, under various minor modifications, in the already known species and genera (to which number additions are being continually made) of those once numerous and gigantic quadrupeds, gives it as important a place in the classification of the mammalian teeth, as that of the dentition of the proboscidian pachyderms or of the ruminants. These phyllophagous *bruta*, both recent and extinct, are characterized by the following remarkable dentition: the teeth are implanted in the maxillaries alone, never in the intermaxillary bones; they are few in number, not exceeding five on each side of the upper jaw, and four in the lower, making sixteen in all; they are mainly composed of vascular dentine, having a thin investment of hard or unvascular dentine and a thick outer coating of cement; and, in common with the teeth of the other bruta, they grow from persistent pulps, and are implanted by a simple deeply-excavated base, not separated by a cervix from the exposed summit or crown. The intimate structure of the vascular dentine resembles that of the entire tooth of psammodus, ptycodus, and some other cartilaginous fishes; it is perforated by vascular or medullary canals from 1-600th to 1-1000th of an inch in diameter, with intervals of twice or thrice that breadth; and these canals proceed in a slightly undulating and sub-parallel course, from the internal surface connected with the pulp, to the hard dentine, at right angles for the most part to the plane of the pulp-surface, and obliquely to that of the hard dentine,—those vascular canals which proceed from the summit of the pulp being parallel or nearly so with the axis of the tooth, whilst those from the base of the pulp are transverse to the same axis, and the intermediate canals have an intermediate course. Processes of the vascular pulp are continued along these canals; consisting essentially of tracts of the primitive pulp which have remained uncalcified. The medullary canals are usually cylindrical, but not of equal diameter throughout; sometimes they present alternate expansions and constrictions; sometimes they bifurcate; and more rarely they anastomose by loops, the convexity of which is turned towards the hard dentine. They everywhere give off minute calcigerous tubes, which have a wavy course, and the finest branches of which terminate in the minute cells of the intertubular tissue. The hollow cylinder or thin layer of hard dentine, resembles very closely that of the human tooth in structure. At the first formation of the tooth, this substance covers the crown as well as the sides of the tooth; but the coronal portion, although originally nearly three times the thickess of the cylindrical or parietal portion, is worn away almost before the sloth reaches maturity, and is never reproduced. The depressed centre of the grinding surface is then formed by the vascular dentine only; that surface having its inequality maintained by the edge of the cylinder of hard dentine, in place of enamel, and being thus adapted to the comminution of the softer vegetable substances, as the leaves, buds, and tender shoots of trees. The cemental sheath presents some interesting peculiarities. The number of radiating cells which it contains, is very large; and the system of tubes with which they are

everywhere connected, is very rich and minute. The principal trunks of this system resemble the calcigerous tubes of the dentine in size and arrangement; presenting, in many parts of the cement, a parallel course, nearly at right angles with its surface, especially where it is in contact with the dentine; and communicating freely with the dentinal tubes, as well as, by their fine subdivisions, with the calcigerous cells. The substance, which is thus evidently intermediate between the dentine and ordinary cement, is harder than the vascular dentine, but softer than the unvascular dentine; and it wears away the bevelled edge around the latter, so as to give it support, but not to interfere with the projection of its margin.

The following passage concisely expresses the characters of the enormou extinct congeners of the existing sloths:

"The larger leaf-devouring species of the order bruta, being too bulky to obtain their food by climbing, were compensated by such colossal proportions and Herculean strength, as to enable them to uproot and prostrate the trees on which they feed; certain toes on each foot were modified for support and progression of the body by hoofs on level ground; all the known species had complete clavicles, closed zygomatic arches, and a powerful tail as long as the hind legs, ancillary to the support of the enormous hinder parts. Every member of this most extraordinary family of quadrupeds, which was peculiar to the American continents, is now extinct; their teeth, which governed their diet, closely resembled those of their nearest existing congeners, the sloths." (p. 333.)

The general plan of the dental structure was in fact precisely the same, in these gigantic megatheroids, as in the comparatively feeble sloths of the present time; and the nature of their food *could* not have been essentially different. In particular, it may be affirmed, that these teeth could not have served the purpose of grinding down hard roots, which has been supposed by some to have been their destination. The train of reasoning by which Prof. Owen was led to reconstruct, as it were, not only the animals, but their habits and mode of obtaining their food,—with a fidelity and consistency which carries conviction to every one who will follow him through the details,—we shall perhaps take another opportunity of bringing under the attention of our readers, in a notice of the Catalogue of the Fossil Mammalia and Aves contained in the Hunterian Museum, which has been recently published by the College of Surgeons. At present, we must restrict ourselves to an indication of the chief points, wherein their teeth differed from those of the existing sloths. Each molar tooth of the megatherium is excavated by an unusually extensive conical pulp-cavity; from the apex of which a fissure is continued to the middle concavity of the grinding surface of the tooth. The central axis of vascular dentine forms a large portion of the cylinder; this is surrounded by a thin layer of hard or unvascular dentine; and this, again, is coated by the cementum, which is of very great thickness on the anterior and posterior surfaces, but which is thin where it covers the outer or inner sides of the tooth. The medullary canals of the vascular dentine very commonly terminate close to the hard dentine, by anastomosing loops, the convexity of which is turned towards the origin of the tubes of the hard dentine; and occasionally, one of the medullary canals may be observed to extend across the hard dentine, and to anastomose with a corresponding canal in the cement. The interspaces of these medullary canals are principally occupied by calcigerous

tubes, which have an irregular course, form reticulate anastomoses, and terminate in very minute cells, at least one hundred times smaller than the calcigerous radiated cells of the cement. The tubuli of the hard dentine, which are given off from the convexity of the terminal loops of the medullary canals, are more regular in their arrangement; as they run in a parallel course from one surface of the layer to the other, at right angles with the axis of the tooth. As they approach the cement, they divide and subdivide, and become more wavy and irregular; their terminal branches take on a bent direction, and form anastomoses, dilate into small cells, and many are seen to become continuous with the radiating tubes of the contiguous cement. This last-named substance, which enters so largely into the composition of the grinders of the megatherium, is characterized in that extinct animal by the size, number, and regularity of the vascular or medullary canals which traverse it. Commencing at the outer surface of the cement, they run in a direction slightly inclined from the transverse axis towards the crown of the tooth, for the most part parallel to each other; in this course they divide a few times dichotomously, and finally anastomose in loops, the convexity of which is directed towards the layer of hard dentine, and in most cases is in contiguity with it. Fine calcigerous tubes are sent off, generally at right angles, from the medullary canals; these quickly divide and sub-divide, form anastomosing reticulations, and communicate freely with the similar tubes that radiate from the calcigerous cells. The calcigerous tubes that radiate from the cells nearest the hard dentine, and from the terminal loops of the vascular canals, intercommunicate freely with the calcigerous tubes of the hard dentine. There is thus a very strong analogy in *structure* between the vascular dentine of the megatherium, and its cementum; for both have vascular or medullary canals (representing the haversian canals of bone), calcigerous cells, and a system of tubuli connecting these with each other and with the vascular canals. But their *origin* is different; the one being formed by *centripetal* calcification of the dentinal pulp, whilst the other originates in the *centrifugal* calcification of the capsule. The direction of the continued nutriment, also, is opposite; but there is an evident facility, in the tooth of the megatherium, for the passage of nutrient fluid from each of these substances towards the other, not only by the anastomosis of their tubuli through those of the hard dentine, but by the inter-communication of their vascular canals.

"In the structure which the fossil teeth of the megatherium and its extinct congeners clearly demonstrate, we have striking evidence of their rich organization, and that they were once pervaded by vital activity. All the constituents of the blood freely circulated through the vascular dentine and the cement; and the vessels of each substance intercommunicated by a few canals continued across the hard or unvascular dentine. With respect to those minuter tubes, the more important as being more immediately engaged in nutrition, which pervade every part of the tooth, characterizing by their difference of length and course the three constituent substances, and which are derived, like the hypothetical 'vasa lymphatica' of the old physiologists, from the ultimate blood-vessels, they form one continuous and freely intercommunicating sytem of strengthening and reparative vessels, by which the plasma of the tooth was distributed throughout the entire tooth for its nutrition and maintenance in a healthy state.

"The grinding surface of the close-set molars of the megatherium differs, on

account of the greater thickness of the cement on their anterior and posterior surfaces, from those of all the smaller megatheroids, in presenting two transverse ridges; one of the sloping sides of each ridge being formed by the cement, the other by the vascular dentine; whilst the unvascular dentine, as the hardest constituent, forms the summit of the ridge, like the plate of enamel between the dentine and cement in the elephant's grinder. The great length of the teeth and concomitant depth of the jaws, the close-set series of the teeth, and the narrow palate, are also strong features of resemblance between the megatherium and elephant, in their dental and maxillary organization. In both these gigantic phyllophagous quadrupeds, provision has likewise been made for the maintenance of the grinding machinery in an effective state; but the fertility of the creative resources is well displayed by the different modes in which this provision has been effected; in the elephant, it is by the formation of new teeth to supply the place of the old when worn out; in the megatherium by the constant repair of the teeth in use, to the base of which new matter is added, in proportion as the old is worn away from the crown. Thus, the extinct megatherioids had both the same structure and mode of growth and renovation of their teeth, as are manifested in the present day by the diminutive sloths." (p. 345.)

Our limited space forbids our entering any further into the elaborate details of this truly splendid work; although there are many points which we should gladly have brought under the notice of our readers; especially the comparison of the teeth of man with those of the lower tribes,—the carnivora in particular. By a long and patient study of mammalian dentition, Prof. Owen has been led to adopt views on this subject, which differ from those of the Cuviers and of Blainville, and which lead to an alteration in the *dental formulæ* which are usually received upon their authority. The characters upon which *their* classification of molar teeth is founded, are derived from their "form, size, and relative position;" whilst Prof. Owen, adopting the principle of "development" as his guide, has arrived at results which appear to us much more consistent and unquestionable. This is only one out of the many examples we might cite, of the philosophic character, which pervades the whole of this treatise, and which gives it a value far greater than it would acquire from the collection of details of which it is chiefly made up,—important as these are in themselves and their applications, and lucidly as they are arranged and described. But we trust that we have said enough to indicate the value which we set upon the 'Odontography,' and to give our readers some conception of its surpassing merits. Although its price may unfortunately place it beyond the reach of a large proportion of the members of our profession, and although the mass of details which it includes is such as to constitute a larger demand upon their time and attention than they can afford, yet we trust that no medical library will long remain without a copy of it, as a standard work of reference, we venture to say, not only for the present, but for all future generations, as long as anatomy and zoology shall be cultivated as sciences.

The thread of inquiry, which has become, in the hands of Prof. Owen, so valuable a clue to the mysteries of Nature, has been taken up and followed by other investigators, with distinguished success. The structure of the skeletons of invertebrated animals has been recently made the subject of minute observation by various microscopists; and the result has been precisely what might have been anticipated on *a priori* grounds.

Thus in Dr. Carpenter's 'Principles of General and Comparative Physiology,' published in October, 1841, the following passage occurs:

"The dense calcareous shells of the Mollusca, and the thinner-jointed envelopes of the crustacea, have been commonly regarded as mere exudations of stony matter, mixed with an animal glue secreted by the membrane which answers to the true skin. The hard axes and sheaths of the polypifera, however, have been also regarded in the same light; and yet, as will hereafter appear, these are unquestionably formed by the consolidation of what was once living tissue. From the analogy which the shells of mollusca and crustacea bear to the epidemic appendages of higher animals, there would seem reason to believe that the former, like the latter, have their origin in cells; and that these are afterwards hardened by the deposition of earthy matter in their interior." (§ 44.)

Acting upon this view, Dr. Carpenter commenced, in the spring of 1842, a series of inquiries into the structure of the shells of mollusca, crustacea, and echinodermata; which he has since continued to prosecute on an extended scale, embracing several hundred species, recent and fossil, in his investigations. About the same period, Mr. Bowerbank, who had previously communicated a paper to the Royal Society on the structure of coral, commenced an independent series of observations on the structure of the shells of mollusca; which had reference, however, rather to the formation of shell, than to its microscopic characters when complete; and which have been limited, in consequence, to a comparatively small number of species. Finding that their paths of inquiry were so distinct, these gentlemen, in a spirit of mutual good feeling, which we should be glad to see more common among the votaries of science, agreed to prosecute them independently of each other; and the results of their researches were simultaneously communicated, on Dr. Carpenter's part to the Royal Society, and on Mr. Bowerbank's to the Microscopical Society, in January 1843. Since that time, the observations of Dr. Carpenter have been carried on under the encouragement of the British Association, and in part with assistance afforded by its funds; and the Report before us is the first part of a detailed account of the varieties in the structure of shell in the different families and genera of mollusca; of which the second, completing the bivalves, was presented at the recent meeting of the Association, but has not yet been published. Having early found a much greater difference in the intimate structure of different shells, than might have been supposed from their external aspect, Dr. Carpenter was naturally led to inquire whether, as in the case of the teeth, these peculiarities are characteristic of certain natural groups; and he soon arrived at numerous highly-interesting conclusions on this point. The following quotation will show on what an extent of evidence these conclusions rest. After mentioning that, during the preceding year, he had made little short of 1000 preparations of shell-structure—in addition to the hundreds previously in his possession—he thus continues:

"A considerable part of my labour has been directed to the determination of the questions: whether an uniform structure prevails through every part of the same shell, so that the structure of the whole shell may be predicated from that of a small portion of it,—and whether the same structure is found in different individuals of the same species, and among different species of the same genus. It is obvious that a settlement of these questions must be of great importance in the application of the microscope to the determination of fossil shells; and I think

that I am now entitled to answer them with some degree of confidence. I have, in a considerable number of instances, submitted *every portion* of a shell to microscopic investigation, selecting such specimens as, from the peculiar characters of their structure, would serve as types to which to refer others; and I have invariably found that an uniform structure pervades the whole of each; so that the examination of but a very small fragment is sufficient to determine the structure of the entire shell. I feel equally certain with respect to the correspondence between the structure of different individuals of the same species; as I have never found any decided variation, although I have in some instances examined several specimens of one kind." (p. 2.)

This constancy of structure is precisely what might have been expected upon *a priori* grounds; but it was necessary to meet the objections of those, who were not prepared to admit the existence of any organic structure in shell; and who, considering it in the light of an inorganic exudation, would regard any variation in its microscopic characters as the result of accidental circumstances. We happen to know that a distinguished foreign geologist, who has lately been in this country, alleged most emphatically and repeatedly, that it was *impossible* that any organic structure could exist in shell or any similar formations; and when invited to an ocular demonstration of the fact, he would only reply—"The microscope frightens me." Such men, like the Lord of Clumber, seem to be living half a century too late.

In a subsequent part of his first Report, Dr. Carpenter gives the following statement of the general results of his researches into the minute structure of shell, in regard to their zoological applications:

"My inquiries, so far as they have yet proceeded, tend to establish this position, *that where a recognizable and constant diversity presents itself in the elementary structure of the shell, among different groups that diversity affords characters which are to a very high degree indicative of the natural affinities of those groups.* It is not always that peculiarities sufficiently distinctive present themselves, even between what are regarded zoologically as distinct families; but where a marked diversity *does* exist, I believe that it will always be indicative of the affinities of the animal. Thus the conformity in structure between all the shells of one natural family is usually so close, than any strongly-marked difference in a particular genus would make me hesitate in admitting it into the group. I think it well at once to premise, that the characters derived from the intimate structure of the shell are not likely to serve for the distinction of *species* from each other, and that they will not often distinguish *genera;* but for the separation of some *natural* families, I believe that they will furnish the best *single set of characters* that the naturalist possesses, especially among particular groups, in which the application of other characters is very uncertain. (p. 16.)

We shall now briefly notice the principal forms of shell-structure described by Dr. Carpenter and Mr. Bowerbank; between whom there is a very general agreement as to all the leading facts. The most remarkable is that designated by both of them as *prismatic cellular substance;* and we may most briefly describe it by saying, that it precisely resembles the enamel of teeth, upon a large scale. It consists of a series of elongated prismatic cells, the parietes of which adhere together so as to form a consistent tenacious membrane; and the surfaces of this membrane display the extremities of the prisms, which are rather irregularly hexagonal or pentagonal. The cells are filled with carbonate of lime, which is evidently secreted into them by their own inherent powers. When a layer of this substance is

broken across, the fracture is evidently fibrous; the fibres passing from one surface to the other. When the animal matter has been removed by decomposition, the calcareous *casts* of the interior of the cells remain *in situ* until disturbed, still forming a continuous layer of prismatic fibres; but as these fibres are no longer held together by any connecting medium, they are at once separated when a portion of the layer is rubbed between the fingers. This condition is sometimes met with in recent shells; but more frequently in fossil. The prisms do not always pass continuously from the outer to the inner surface. They are more numerous and smaller on the exterior, and several of them terminate in points about the middle of the layer; their places being taken by the remaining prisms, which are continued inwards, and enlarge so as to remain in contact with each other. The prisms show the transverse striæ, which have been noticed as visible in the enamel-fibres. Mr. Bowerbank considers these striæ as constituting a system of vessels; but this view is objected to, on various grounds, by Dr. Carpenter, who gives the following account of them:

"By submitting the *cut edges* of the membranous wall of the prismatic cell (in a vertical section) to a high magnifying power, under favorable circumstances, I have been able to discover what I believe to be the real cause of the transverse striation in question. The membrane evidently projects inwards at those parts, not in consequence of being pushed inwards from without, but because *its own thickness* is there increased. This appearance corresponds well with the conclusion already drawn in regard to the *progressive* formation of each layer of shell; and I am much inclined to believe that each transverse marking indicates a distinct deposit. Whether, during the time when this succession of deposits was taking place, the prismatic cells grew at their bases, and these lines indicate the additions which were progressively made to the length of the cells; or whether the long prismatic cells, as we now find them, are made up by the coalescence of a number of layers of flat pavement-like epithelium-cells, placed one upon another, and the lines indicate their points of junction,—I do not feel warranted in affirming with certainty, as the question could be only rightly decided by examining the shell in the progress of its formation, which I have not yet had the opportunity of doing. I am much inclined, however, to adopt the latter view, which was suggested to me by Prof. Owen. The coalescence of cells linearly arranged, so as to form a single long cell or tube, is an occurrence with which animal and vegetable physiologists are alike familiar. The idea derives strength from the fact that I have occasionally met with a layer of prismatic cellular structure, of such extreme tenuity that it was almost impossible to separate it, lying between thicker layers of the same shell of *pinna*. The cells of this layer, instead of being elongated prisms, were flat and pavement-like, resembling the epithelium of serous membrane; and it was in such that I have found the cytoblasts most perfectly preserved. It is hardly to be supposed that this layer was produced by a distinct act of shell-formation, as it would not add in any appreciable degree to the size or solidity of the shell; and it seems probable that it was a supplemental portion which had not coalesced with the remainder of the layer, of which it should properly have formed a part." (p. 8.)

This prismatic cellular substance always forms the *exterior* layer of the shells in which it occurs; and there seems much reason to regard it in the light of a *calcified epithelium.* The following account is given by Dr. Carpenter of its development:

"Although the prismatic cellular structure has not yet been observed in actual process of formation, yet certain appearances which are occasionally met with in the marginal portions of its newest layers, throw great light upon its mode of

growth, and indicate its strong resemblance to cartilage in this respect; for in these situations we find the cells neither in contact with each other, nor polygonal in form, but separated by a greater or less amount of intercellular substance, and presenting a rounded instead of an angular border. Upon looking still nearer the margin, the cells are seen to be yet smaller, and more separated by intercellular substance; and not unfrequently we lose all trace of distinct cells, the intercellular substance presenting itself alone, but containing cytoblasts scattered through it. This appearance has been noticed by myself in *perna* and *unio*, and by Mr. Bowerbank in *ostrea;* so that I have no doubt that it is general in this situation. We may, I think, conclude from it that the cells of the prismatic cellular structure are developed, like those of cartilage, in the midst of an intercellular substance, which at first separates them from each other; that as they grow and draw into themselves the carbonate of lime poured out from the subjacent surface, they approach each other more nearly; and that as they attain their full development, their sides press against each other, so that the cells acquire a polygonal form, and the intercellular substance disappears." (p. 9.)

Under the general head of *membranous shell substance,* Dr. Carpenter describes various forms of structure, which agree in not presenting distinct traces of a cellular origin, and which, in particular, leave only a *membranous* residuum when treated by acid. This membrane is evidently analogous to the "basement-membrane" of Mr. Bowman, the "primary membrane" of Mr. Goodsir; and the varieties in the appearances presented by it are thus described by Dr. Carpenter:

"In its simplest condition it is a pellucid structureless pellicle of extreme delicacy and transparency, exhibiting no trace either of cells, granules, or fibres. I have occasionally found it, however, of a somewhat granular appearance, as if formed by the solidification of a thin stratum of fluid, including an immense number of minute molecules. In other cases, again, I have found it studded here and there with what seemed to be incipient cells; and lastly, I have occasionally found these cells more developed, and forming an almost continuous layer on the surface of the membrane. In this state they somewhat resemble the incipient form of the prismatic cellular substance. These cells may be occasionally seen in sections of the shell itself; and they will be often found in very different degrees of development even in the corresponding layers of two shells of the same species. Coupling the appearances which I have myself observed with the observations of Mr. Bowerbank, on the *formation* of shell, and keeping in view the general doctrines of cell-action, which I have elsewhere endeavoured to develope, I am inclined to believe that these cells are, like the cells of the prismatic cellular structure, the real agents in the production of the shell, it being their office to secrete into their own cavities the carbonate of lime supplied by the fluids of the animal. But whilst the cells of the prismatic cellular structure advance in their development, so as to form a perfect tissue, the 'calcigerous cells' of which we are now speaking, appear to burst or liquefy, and to discharge their contents upon the surface of the subjacent membrane, on which a shelly layer is thus formed. A greater or smaller proportion of these being left entire, and being included in the substance discharged from the rest, would present the appearances I have mentioned as occasionally manifesting themselves in sections of membranous shell-structure, and in the decalcified membrane." (p. 10.)

In his second Report, Dr. Carpenter described a considerable group of bivalve shells constituting the family *myidæ*, in which the cellular character is very distinctly exhibited in sections of the unaltered shell, but which yet presents no cellular residuum; and he attributed this to the want of that horny intercellular substance which holds together the cells

in the prismatic cellular structure, and which gives a degree of consistence to the animal basis that is altogether wanting elsewhere.

We must pass over Dr. Carpenter's observations upon the peculiar form of the membranous shell-substance which gives rise to the *nacreous* lustre, to extract his statements regarding the tubular form of structure, which evidently bears a close analogy to dentine:

"All the different forms of membranous shell-structure are occasionally traversed by tubes, which seem to commence from the inner surface of the shell, and to be distributed in its several layers. These tubes vary in size from about the 1-20,000th to the 1-2000th of an inch, but their general diameter in the shells in which they most abound is about 1-4500th of an inch. The direction and distribution of these tubes are extremely various in different shells; in general, where they exist in considerable numbers, they form a network which spreads itself out in each layer, nearly parallel to its surface, so that a large part of it comes into focus at the same time, in a section which passes in the plane of the lamina. From this network some branches proceed towards the nearer side of the section, as if to join the network of another layer; whilst others dip downwards, as if for a similar purpose. . . . In no cases have I seen any such variation in the size of the tubes of the same shell, as would convey the idea of their resemblance to blood-vessels; and even where a division occurs, the size of each of the branches is usually equal to that of the single trunk. Sometimes these canals are quite straight, whilst in other instances they are sinuous. That they are not mere channels or excavations in the shell-substance, is proved by the fact that they may be seen in the decalcified membrane. I have frequently seen in them indications of a cellular origin, as if they had been formed by the coalescence of a number of cells arranged in a linear direction; and I find that Mr. Bowerbank has come to the same conclusion." (p. 14.)

The larger of these tubes are designated by Mr. Bowerbank as haversian canals; but we scarcely see how they can deserve that title, as they are manifestly too small to contain a network of blood-vessels on their walls. The term might perhaps be more appropriately applied to certain perforations in the shells of the *terebratula* and allied genera, which pass almost directly through the shell, from surface to surface, and have a diameter sometimes amounting to about 1-400th of an inch. These are lined by a distinct tubular membrane; and they are stated, in Dr. Carpenter's second Report, to be occupied by cells. They consequently bear a strong analogy to the medullary canals, described by Prof. Owen as existing in vascular dentine; but no systems of radiating calcigerous tubuli have their origin in them.

A most remarkable analogy to unvascular dentine is found in the shell of the crab, and particularly in the end of the claw, where the calcareous envelope is thicker and of denser texture than in other parts. This is described by Dr. Carpenter (Annals of Natural History, Dec. 1843,) as "composed of a substance exactly analogous to ivory; being very transparent and apparently homogeneous when cut into very thin slices; and being perforated by an immense number of minute sinuous tubuli, which run nearly parallel to one another from one surface of the shell to the other. A transverse section of the claw shows the tubes radiating from the central cavity towards the external surface, and would, I feel assured, be regarded by the most experienced observer as the section of a tooth, if he were not informed of its real nature."

The following is an illustration of the value of the microscopic examina-

tion of the intimate structure of shells, in determining the place of a specimen, whose position was otherwise doubtful. We should premise that all the shells of the family *margaritaciæ* agree with that of the *avicula* (or pearl-oyster,) in having a nacreous interior, and an exterior of prismatic *cellular* substance; whilst in the neighbouring family *pectinidæ*, (of which the common pectens or scallop-shells constitute the type,) the most characteristic feature is the coarsely-corrugated disposition of the *membranous* basis, and the occasional presence of tubuli. Now this corrugated structure, with a greater or less amount of tubular perforation, is characteristic of several other families of bivalve-shells, in which the mantle is wholly or partially closed, and which are therefore organized on a different plan from the pectenidæ, which have it open; and it would not therefore serve by itself to distinguish a fragment of a shell of this family from those alluded to. But it is *quite sufficient* to distinguish a shell of this family from any of the neighbouring families, to which in its general characters it might present an affinity; and it therefore affords the most assistance where it is most wanted, as in the following case:

"A shell was described by Prof. Philips, in his 'Geology of Yorkshire,' as an *avicula*, which had been previously described by Messrs. Young and Bird as a *pecten*. The same species, or one closely allied to it, found near Bristol, was described by Mr. S. Stutchbury as an avicula, he not being at the time aware that it had been met with and described elsewhere. The mixture of characters is such as would sanction its being placed in either group, according to the relative value attached to them. Thus in the form of its hinge, it is most allied to avicula; whilst in the flatness of its under valve, and in the disposition of its costæ, it rather corresponds with the pectens. The intimate structure of the shell here serves, I think, to decide the point; for we find no trace of either the prismatic cellular substance or the nacre, which are characteristic of avicula, but we meet, on the other hand, with the coarsely-corrugated and somewhat tubular structure of the pectenidæ." (p. 20.)

In the same manner, Dr. Carpenter has been able to give important assistance in the determination of the true affinities of the curious aberrant genus *etheria*, an inhabitant of the *rivers* of Africa, and yet so *oyster-like* in its aspect, that it gave rise to Bruce's statement that he had seen oysters above the cataracts of the Nile; which, like many other assertions of that enterprising traveller, was the subject of much ridicule at the time, but has been since shown to have a strong foundation in truth. By most conchologists it has been arranged among the *chamaceæ*, chiefly on account of its tendency to attach its lower valve to solid bodies; this tendency, however, exists equally among the true *oysters*, from which, in many points of its organization, as well as in its habitat, it seemed widely separated. A better knowledge of its anatomy, however, has shown that its place could not properly be among the chamaceæ, and that it should be referred to the margaritaceæ, or to one of the groups bordering upon it, and more nearly allied to the oyster; and this view exactly corresponds with the characters presented by the intimate structure of the shell, when examined by Dr. Carpenter, for it was found to consist of a prismatic cellular substance with a lining of nacre.

We must now quit Dr. Carpenter's Report, to give an extract from Mr. Bowerbank's 'Observations,' which will convey an idea of his results in regard to the subject to which they are chiefly directed:

"If the newly-formed lip of the common garden-snail be carefully removed, so as to include a portion of the shelly matter as well as the membrane, and be put into a little water, covered with a thin slip of glass, and examined by transmitted light with a power of 280 linear, it presents the appearance of a thin, yellow-coloured, horny membrane, thickest at its junction with the old lip of the shell, and becoming gradually thinner towards the outer margin. There are an infinite number of minute globular vesicles scattered over the whole of its surface, which vary in diameter from 1-50,000th to 1-3000th of an inch. These vesicular bodies are incipient cytoblasts and cells; for although in the early stage of their growth we cannot discern the nucleus, they may be readily traced through their various stages of development, until in the larger vesicles it may be frequently observed; and the young cells approaching each other are compressed closely together, and become the first superficial layer of cellnlar structure. . . . The completion of the process of the development of the cellular tissue may be best observed in the immediate neighbourhood of the old lips of the shell; and passing thence gradually towards the outer margin of the newly-formed membrane, they may be seen in all their different stages. While the young cells are detached from each other, and retain their globular forms, they are perfectly transparent, the secretion of calcareous or other matters not appearing to have commenced; but we frequently observe in the neighbourhood of the newly-formed stratum small patches of cells congregated together, which are of a deep yellow tinge, and of a semi-opaque appearance, as if they were separate centres of ossification, arising in the same manner that we observe in the production of the calcareous matter in the young cartilages of the bone in the higher classes of the animal kingdom. Amid the great mass of vesicular bodies destined to form the cells for the secretion of the calcareous deposits of the shell, we find other cytoblasts dispersed over all parts of the membrane, which are developed in the form of tesselated cellular structure, by which means the original membrane is very much increased in substance. This production of tesselated cellular structure is more particularly abundant when the calcigerous cells are in their advanced stages of development. If a portion of the newly-formed stratum of shell be removed from the membrane on which it reposes, we observe that the horny structure continues to abound in the vesicles in all their earlier stages of growth; from which it would appear that its office is not confined to the production of the first stratum of calcigerous cells only, but that it is destined to originate the greater part, if not the whole, of the calcigerous cells of the shell; and this appears the more probable, as we may now observe that the membrane has attained a higher stage of organization, minute vascular tissue being frequently observed imbedded aud ramifying amid its structure, and oftentimes projecting from its torn edges. These vessels are very minute, none of those which I have observed exceeding 1-14,000th of an inch in diameter. If the uncovered portion of the membrane nearest to the old lip of the shell be examined with a power of 500 linear, these minute vessels may be seen in the course of formation; presenting the appearance of long ramifying lines of exceedingly minute gelatinous molecules, closely adhering together in a single series, and frequently appearing cylindrical and tubular in one part, while in another they have quite a moniliform appearance. This production of vascular structure by the arrangement of minute molecules or cytoblasts in single series, I have frequently observed in the membranous tissues of the corallidæ." (p. 6.)

Thus it appears that the hard skeletons which afford support and protection to the soft bodies of animals belonging to the widely-different families among the invertebrated classes, are formed upon a plan essentially the same with that which may be traced in the production of the various kinds of dental structure in the vertebrata. This analogy might in some degree be predicated; when it is considered that the teeth are

produced from the mucous membrane, and may be regarded as parts of the *dermo-skeleton*, which is developed to the exclusion of the internal or *neuro-skeleton*, in the lower divisions of the animal kingdom. We have no doubt that the careful study of the history of their development, pursued in connexion one with the other, will lead to some very interesting general results, or rather to an extension of those which have been already attained. Thus it appears from the observations of Mr. Bowerbank, that the tubuli in shell have precisely the same origin as in tooth, being the spaces originally occupied by nuclear particles arranged in rows, and therefore not filled up in the process of calcification. In the larger tubuli these particles are fully developed into cells, which Mr. Bowerbank states that he has frequently met with in cylindrical series; each cell having a distinct nucleus, a transverse diameter of 1-5000th of an inch, and a longitudinal diameter of from 1-3000th to 1-200th of an inch.

The following observation confirms Dr. Carpenter's views, founded upon the structure of the complete shell, in regard to the succession of deposits in each new formation:

"From a careful examination of many new lips in various stages of progressive development, I do not think that the whole of the coagulable lymph of which the new membrane is formed, is exuded from the surface of the animal at one operation, but rather by a series of exudations, each one of which emerges from beneath the terminal margin of the last one; for if the membrane be carefully examined from the line of advance of the newly-formed cells towards its extreme edge, it will be seen that there are several lines of thickened tissue, which correspond in distance and appearance with the lines of growth on the old shell; and this is the more probable, as I have observed in young shells which I have taken from their places of hybernation in November, that the extension of the lip is still going forward, or rather perhaps has been so up to the very period of their retirement for the season." (p. 7.)

We have given a less full account of the researches of Dr. Carpenter and Mr. Bowerbank than of those of Prof. Owen, not because they are less interesting in themselves, but because they have less bearing upon the chief objects of our Journal. None of our readers, we hope, will any longer retain any scepticism with respect to the value of the achromatic microscope as an instrument of research. It may almost be said to furnish the physiologist with a sixth sense, so greatly do its capabilities surpass those of the older instruments.

ART. V.

A Treatise on the Diseases and Special Hygiene of Females. By COLOMBAT DE L'ISÈRE. Translated from the French, with Additions, by CHARLES D. MEIGS, M.D. &c.—*Philadelphia*, 1845. 8vo, pp. 720.

IN the year 1838 M. Colombat de l'Isère published two volumes of a treatise on the Diseases of Women, and completed it by the publication of a third volume in 1842. He gives an account in this work of all the diseases of the impregnated and unimpregnated conditions, as well as of the puerperal state. Unfortunately, however, he possessed few qualifications, besides industry, for the performance of his difficult undertaking. His

work is a mere compilation, often extremely ill-executed, and showing but little critical knowledge; the author's aim appearing to be that of quoting as large a number of writers as possible, while he pays but little regard to the comparative importance of their opinions.

Such being the character of this book, we cannot but feel surprised that a gentleman of Dr. Meigs's great experience should have thought it worth his while to engage in the wearisome occupation of translating it into our own language. He would seem, indeed, to have found it an irksome labour, if we may judge from the small amount of pains that he has devoted to the performance of his self-imposed task. We strongly suspect that Dr. Meigs has intrusted the main drudgery of translating to some inferior hand, thinking that the supervision of the proofs and the addition of notes to the text, would sufficiently justify the announcement of his name on the title-page as translator.

To show our readers how well Dr. Meigs is qualified to write on the subjects treated of in this volume, we will here notice some of the principal additions made by him to it—more especially such as are of a practical nature. We are sure that no one who peruses these additions will fail to express his regret that Dr. Meigs did not give us an original work of his own instead of a careless version of M. Colombat's.

Epoch of puberty. To the chapter on the "phenomena of puberty," Dr. Meigs appends a note on the epoch of puberty and the critical age of the females of the republic of Venezuela, founded upon the researches of Dr. Vargas of Caraccas. From these it appears that in 70 per cent. menstruation occurs from the age of 13 to 15 years, while the most common period for the cessation of menstruation is from the age of 45 to 48 years. Precocious menstruation is more common in the white than in the negro race.

Menstruation. We have been much surprised to read the following statement at p. 33. "It is well known, that in a great metropolis like Paris or Philadelphia, there exist multitudes of women who do not take the least precaution to prevent the blood of the menses from soaking through their clothes, and exposing their condition to the public eye in the street or in the market-place!" From a very extensive acquaintance with the habits of the poorer females of this metropolis (London), we are convinced that every precaution is taken by them to prevent such an indecent exhibition.

To the chapter on the cessation of the menses, Dr. Meigs adds a tabular statement, showing the age at which this function ceased in 183 females. His observations agree with those of M. Brierre de Boismont.

Anatomy of the vagina. At p. 48 he directs special attention to an omission in the original text in the anatomical description of the vagina, viz. that while the posterior and part of the lateral walls are inserted into the soft and distensible parts, as the perineum, the anterior and part of the lateral walls are firmly attached to the pubic arch; so that in labour, when the vagina becomes lacerated, it is the anterior or antero-lateral surfaces which give way, these not yielding so readily by reason of their firm attachment. In remarking upon the distensibility of the vagina, Dr. Meigs makes some judicious remarks upon the necessity of remembering this quality of it when the tampon is employed to arrest hemorrhage in

early abortion or menorrhagia. The tampon he recommends "consists of portions of linen torn into squares of three or four inches, of which the pieces are successively introduced until the cavity is quite filled."

Enlarged clitoris. At p. 83, Dr. Meigs relates a case of enlarged clitoris, which was under the care of Dr. Norris, of the Pennsylvania Hospital. It occurred in a patient 36 years of age, had existed fourteen years, and commenced after she had received a blow on the part. It gave to the hand the sensation of indistinct fluctuation, and was not painful even when *smartly* compressed.

A common lancet introduced into the base of the tumour gave exit to about twenty-two ounces of a thick, blackish fluid, of the consistence of tar, perfectly inodorous, and very similar to retained catamenial fluid. After relating this case, the Doctor remarks, "so far as my knowledge extends, there is no example of blood detained for months and years in cavities, without undergoing decomposition, except when it is detained within the generative tissues."

Malformations of the vagina. At p. 91, he relates a singular case. The patient was in labour, and considerable delay and difficulty were experienced by the division of the vagina into two lateral halves by a septum which extended from the external orifice or os magnum to near the uterine extremity of the canal; the head of the child forced into the right half compressed the other against the left side of the pelvis. Dr. Meigs delivered the infant alive, "without accident or any untoward result." After the patient's recovery an accurate examination was made, when it was found that the septum had not given way during the distension of the right canal.

At pp. 95-99, some cases of congenital narrowness of the vagina are related, and in the remarks appended to these cases the translator takes occasion to recommend "simple graduated dilatation," and strengthens his recommendation by the advantages which follow gentle dilatation of the urethra in strictures of that canal; by the usual effects of the pains of labour on the cervix uteri, vagina and perineum, which he has "*familiarly*" noticed; by the gradual distension which the female urethra and rectum allow, the former permitting the index-finger, the latter the hand to be introduced. He strongly advises against the use of cutting instruments in such cases.

At p. 101 he records an interesting case of occlusion of the vagina after parturition, without any assignable cause. Fortunately Dr. Meigs was consulted before the cohesion which had taken place between the anterior and posterior wall of the vagina was very firm, and he therefore was enabled to separate them with the bulb of a probe, the patient losing not more than two or three drachms of blood. A second case is described, taken from the 'Philadelphia Practice of Medicine.'

Absence of the womb. To M. Colombat's chapter on this subject Dr. Meigs appends a note recommending that in cases of protracted emansio mensium, marriage should be postponed until after the menses have appeared, or until a competent inquiry has been made as to the fitness of the female to enter into the marriage compact, illustrating his observations by the recital of the case of a "handsome woman" by whom he was consulted, and who, although 22½ years of age, and married upwards of two years, had never

menstruated. The mammæ were well developed, the pudendum amply supplied with hair, in fact there was perfect development of the sexual system, but total absence of menstruation. For two years and upwards the husband had failed in his attempts to accomplish sexual congress, when at length Dr. Meigs was consulted. Careful investigation discovered the vagina to be a cul-de-sac, scarcely two inches in length, and the uterus to be wanting. A similar case has lately fallen under our notice. Such cases, however, must be of rare occurrence, for when proceedings were instituted to obtain a nullity of marriage, no precedent was discovered in the records of the court.

Prolapsus. Dr. Meigs is of opinion that prolapsus uteri in many instances is occasioned by the floor of the pelvis, or tissues of the perineum losing their full power and energy, and especially by the weakness and loss of power of the levatores ani. He differs from M. Colombat in the opinion that "intense peritonitis" is induced as one of the complications of prolapsus, but regards it as simulated peritonitis, the diagnosis of which he admits may be very difficult.

Polypus. Dr. Meigs wisely cautions his readers against placing implicit confidence in a statement of Colombat when speaking of the diagnosis of polypus uteri—viz., that "polypus uteri is irreducible, and any attempt at reduction causes insufferable pain, while, on the contrary, prolapsus uteri is easily reducible, and its reduction gives great relief to the patient." Dr. Meigs most properly says we sometimes meet with examples of polypus uteri which are occasionally accessible, and at other times retire beyond the reach of the finger; for instance a pediculated polypus may be attached to the fundus, and may be forced partially through the os uteri by contraction of the parietes, and when such contraction ceases it will be again drawn into the cavity. (p. 126.) A case in point is at the present time under our care.

Use of the pessary. At pp. 136, 140, and 150, Dr. Meigs makes some judicious remarks on the pessary and its employment. We fully agree with him that "great abuses are to be met with in the prescription and use of this instrument," while "many persons are restored to health, or procure a tolerable state of health by its use." He urges that every case of prolapsus or procidentia uteri should be regarded as an affection of the vagina, and that the indication of cure should be "the restoration of this canal." He strongly advises against treating prolapsus by long continued rest in the horizontal posture, as it is apt to exhaust the "muscular force of the patient," "weaken the levatores ani," and "relax the perineum," which becomes nearly horizontal, or quite even with the tubera ischii; and, on the contrary, he recommends "exercise, air, and light, a nutritious diet, wine, and malt liquor," believing that in proportion as the general health improves, the local disorder will gradually lessen and finally disappear. When a pessary is indicated Dr. Meigs prefers the globular form of instrument, as it cannot become displaced by turning on its axis, and one made of silver, or silver washed with gold, of about two inches and a quarter in diameter. He advises "some time" to be taken in the introduction; for if it enter "too readily, or if it be not properly adjusted," it will be expelled at the first bearing down in defecation or in evacuating the urine. It is to be placed in the vagina beyond the constrictor muscle.

Antiversion and retroversion. At p. 159 we find some remarks on antiversion and retroversion. Dr. Meigs states that he has seen but one decided case of antiversion, although he has met with many of retroverted uterus. In many points he differs from M. Colombat in his account of these displacements. He maintains that women who habitually permit the bladder to be considerably distended will in the end cause the round ligaments to be so relaxed and stretched as to be perfectly useless. He relates the case of a lady whose round ligaments are so useless that the womb falls over into the hollow of the sacrum from the slightest effort, so that on many occasions he "has had to *reposit* it." He believes in the contractility of these ligaments.

In cases where extreme difficulty has been experienced in the attempts to reduce a retroverted womb, Dr. Meigs directs the patient to be placed upon her knees, with her thighs at right angles to the bed, and the sternum in contact with the mattress. By such a position the weight of the viscera is taken off, the power of "tenesmic" resistance is wholly removed, and the reposition of the womb is favoured. We perfectly agree with Dr. Meigs that this mode of preventing the "tenesmic power" is preferable to bleeding "ad deliquium," as recommended by Dewees. Few cases will resist reposition under such circumstances, and when the attempts at the restoration fail, it will in most instances be caused by adhesions.

Obliquity of the uterus. At p. 175 the translator protests against the statement of M. Colombat that "right lateral obliquity takes place in ninety-nine cases in a hundred," and affirms that "left lateral obliquity is not less frequently met with than that of the right side;" we must beg leave to differ from Dr. Meigs, and maintain that right lateral obliquity is more frequently met with than left lateral obliquity, and we must express our astonishment that a contrary opinion is entertained by a physician of so large experience.

Inversion of the uterus. To the chapter on "Inversion of the Womb," Dr. Meigs appends two most interesting cases of reposition of the womb after inversion, and in which both the women afterwards became pregnant. These extraordinary cases are confirmed by the testimony of several other respectable practitioners.

Labour complicated with enterocele. At p. 207 a most instructive case of labour, complicated with vaginal enterocele is recorded; it had lasted fourteen hours and a half. The hernia was reduced while the patient was on her left side, by the fingers of the right hand introduced into the vagina. After the reduction, four pains expelled the child.

Prolapsus vaginæ. At p. 211 a case is mentioned where the vagina prolapsed to the extent of five inches, and was as large as a man's arm.

Support of the perineum. In a short note at p. 218, the Professor gives some valuable advice as to the mode in which the perineum should be supported during labour. He properly directs that the perineum should be so pressed that the head should be made to turn upwards in front of the symphysis in proportion as it emerges more and more from the pelvis in the direction of the circle of Carus. Attention to this rule would prevent many perineal lacerations.

Prurigo vulvæ. At p. 271 Dr. Meigs speaks highly of the efficacy of the following lotion in purigo of the vulva. R.. Sod. biborat. ℥ss, mor-

phie sulph. gr. vj, aq. rosæ ℥ vj; m. ft. lot. It should be applied three times a day by a piece of sponge or linen, the surface being previously washed with tepid water and then well dried.

Abscess of the labia. To the chapter on phlegmonous inflammation of the labia the translator appends a caution, guarding the young and inexperienced practitioner against mistaking a vulvar enterocele for a supposed abscess.

Physometra. Professor Meigs does not believe that the disease termed physometra does or can really exist. In one case only has he known gas discharged from the uterus, and that was during an embryotomic operation, the placenta which was subsequently removed being putrid, black and emphysematous. He does not believe that "the inner walls of the womb" have the power "to excrete gaseous fluids." Neither will he admit that the os or cervix uteri can become "air-tight." He ridicules the opinion of Dr. Waller, who believes the air to be secreted by the "*menstruating membrane.*" He regards all cases of supposed physometra to be cases of chronic tympanitis.

Polypus uteri. At p. 388 Dr. Meigs differs from M. Colombat in his views of the sensibility of polypus. M. Colombat having stated that the existence of nerves, although not demonstrable, may be proved by the pain resulting from the constriction of their pedicles. Dr. Meigs, believing the tumour to be insensible to pressure, supposes that the constitution "disturbs or distracts the sensitive part upon which it sits." Its organization and nutrition, prove that it has blood-vessels, and such vessels must have accompanying nerves. At p. 396 a case is reported in which a large polypus, weighing eleven ounces, was expelled from the uterus two days after the delivery of a fine healthy male child. Dr. Meigs is of opinion, and we agree with him, that the development of the tumour must have proceeded at a great rate during the latter months of gestation.

Ovarian abscess. At p. 412 the translator relates the case of an "ovarian abscess" occurring in a young woman after her third confinement. It pointed in the lower part of the left iliac region, and on a puncture being made with a lancet, a pint of pus was discharged. This seems to have been a case of common pelvic abscess after delivery. In the next page Dr. Meigs combats the idea of treating as "ovaritis" those inflammations of the ovary which are met with during the progress of puerperal fever. He admits that deposits of pus are sometimes found beneath the ovarian peritoneum, but he regards it as "disparaging the idea of a puerperal fever to call it an ovaritis."

Extirpation of the ovary. Dr. Meigs fully concurs with M. Colombat in his disapprobation of operations for the extirpation of the diseased ovary, regarding such operations as not to be "justified by the most fortunate issue in any ratio whatever of the cases."

Treatment of ovarian diseases. Dr. Meigs gives some valuable advice as to the best means of checking ovarian disease when discovered early. "The diet" he says, "should be regulated, it should neither be too spare nor too nutritious; the clothing should be warm, especially about the pelvic region and lower extremities; the bowels should be kept in a soluble state; the patient should take a bath at 97° three times a week, and go from her bath to her bed; leeches sufficient to take four or five ounces of blood

should be applied to the left groin, over the round ligament, four or five days before each menstrual period. She should confine herself to the house for three or four days of the period of return, and for some months she should take the hydriodate of potassa in three to five grain doses, thrice a day."

Menstruation. At p. 459 Dr. Meigs condenses and recapitulates the opinions of Lee, Negrier, Gendrin, and Raciborski, with respect to the changes produced in the ovary itself at the period of the catamenial flow; and is of opinion that the "hyperemic affluxion," which is "coincident with the stated periodical, monthly completion of a Graafian vesicle" and "which hyperemia relieves itself by the menstrual discharge," gives us "the key to all the derangements of the catamenial office, not dependent upon obturation(?) or some sudden shock and diversion of the nervous power to other directions."

Dysmenorrhœa. Dr. Meigs approves of Dr. Mackintosh's plan of dilating the cervix uteri, which he says has been productive in his hands of advantage in several instances.

Metrorrhagia. In this disease Dr. Meigs recommends a decoction of the roots of the common black currant, ripe, and the roots of the dewberry. A handful of each is to be boiled with two quarts of water, and after straining the liquor, the patient is to take a cupful occasionally for a dose.

Chlorosis. In speaking of the diagnosis of chlorosis, Dr. Meigs remarks that "every instance of anæmia is not discoverable upon inspection of the tint of the skin, nor a survey of the state of the patient as to her embonpoint," for some patients "grow fat during the malady." In such cases Dr. Meigs "tests the state of the lungs by asking the patient to make several forced inspirations, in order to discover whether the capacity of the lungs for atmospheric air are at all lessened by disease," and if "she appear to be able to inhale fifty or sixty cubic inches at an inspiration." Dr. Meigs would conclude that the air-cells are free, "and this view may be confirmed by percussion and auscultation of the chest." In reference to the frequency of the pulse, Dr. Meigs says, that whilst in a state of rest it may be great, as at 70, 80, or 90 beats in a minute; but if the patient be requested to walk to the head of the stairs and returns immediately to her seat, she will, if anæmic, be found to have the pulse greatly accelerated and beating in the most *troublous* manner, to the number of 120, or even 160 pulsations per minute, while her respirations may amount to 40 or even 60 per minute. In the treatment of chlorosis, Dr. Meigs recommends the citrate of iron, combined with the sulphate of quina, the following is his formula:

Citrate of iron 2 drs.
Sulphate of quinine, dr. ss.
Water one ounce. Mix

From 20 to 30 drops to be taken for a dose in syrup and water.

He advises with Raciborski that the draughts should be taken within half an hour after the meal, so that it may be carried with the chyle along the course of the bowels.

Impotence and sterility. Dr. Meigs relates the case of a lady who died with symptoms of strangulated hernia, and who upon examination was found to have a knuckle of intestine which had passed under a band within

the pelvis, one of numerous bridles which had been produced many years before, when she was 13 years of age; and had suffered from an attack of peritonitis, to which she had nearly fallen a victim. Her fallopian tubes were adherent, so as to render their physiological function impossible, and although married for several years, she had not conceived. Sterility in some cases Dr. Meigs supposes to depend upon the plugging up of the cervix uteri, by a thick, translucent, transparent, or opaque mass of mucus, in amount about a teaspoonful, cohesive and ductile, so that it may readily be drawn from the cervix upon the point of a sponge. He supposes it to be produced by the mucous follicles just within the os uteri. For its removal or treatment, he recommends the occasional cauterization of the canal of the cervix with nitrate of silver or a pencil of sulphate of copper, astringent injections, baths, a long voyage, and change of climate.

Abortion. The translator's notes to the chapter on abortion, although brief, are valuable; he is of opinion, that in the majority of cases of abortion, the cause depends upon the death of the embryo. He alludes to the case of a lady who, at the middle of the third month, had uterine hemorrhage to the amount of more than twenty ounces without losing the embryo, and who was delivered of a healthy child at full term. We perfectly agree with Dr. Meigs as to the difficulty of making a correct diagnosis in cases where pregnancy has occurred, and the death of the embryo after a time has taken place, the ovum being retained in the womb for several months. Our opinion must be formed upon the previous history, especially the dates of the menstrual periods, the form and size of the womb as determined by external and internal examination, the condition of the cervix and os uteri, together with the present and past condition of the mammary glands and areolæ.

In cases of repeated miscarriages, Dr. Meigs has a high opinion of the plan of treatment recommended by the late Dr. Physick, viz. the use of an anodyne enema, consisting of a wineglassful of starch mixed with forty drops of laudanum, introduced every night until quickening takes place. Dr. Meigs does not approve of removing the placenta after abortion by force, stating most justly that such a practice is both unnecessary and dangerous. He advises the use of the tampon in all severe hemorrhages from abortion up to the period of four months to four months and a half.

Disease of the lungs in pregnancy. Some very judicious practical remarks are added by the translator to the chapter on pregnancy. In speaking of *dyspnea* in pregnant women, he says such a complication deserves close and most careful scrutiny. Every part of the chest should be closely auscultated, the capacity of the lungs for air should be tested, by causing the patient to make a forced inspiration. "A patient attacked with inflammation of the lungs is not very easily cured, if in an advanced stage of pregnancy, until after her delivery shall have taken place." He further remarks that many women complain of difficult respiration in pregnancy and of violent palpitation and unusual disorder of the heart, in consequence of their having become anemic during the last months or weeks of gestation. In cases where the anemia proceeds to an aggravated state, the consequences are often most distressing, the patient is liable to troublesome and even very dangerous effusions of serum into the chest, pericardium, and abdomen. A mistake on the part of the practitioner would be most

unfortunate for the patient by misleading him in his treatment. He must "carefully discriminate between the effects of a pure inflammation and those often similar ones that arise out of the feeble and irregular innervation proceeding from the state of the blood."

Flooding. At p. 612, Dr. Meigs speaks most forcibly of the impropriety of using the tampon in the hemorrhages of advanced pregnancy; and further disapproves of this remedy in placenta prævia. He lays great stress upon the uses and value of position, as a means of diminishing or suppressing the flooding; the woman should lie upon the bed, with the head but slightly raised, while the shoulders are upon the same plane with the rest of the trunk. In cases of copious hemorrhage, where the pulse is full and bounding, where there are heat of skin and flushing of the face, he advises bloodletting, provided the general state of the patient's health will warrant such a proceeding; but in determining this we are to discriminate between the cases which occur from a "*nisus hæmorrhagicus*" and those simple effusions of blood which proceed from an accidental detachment of a portion of the placenta. He condemns frequent examinations, as both mischievous and useless. Some brief but valuable directions as to the best plan of arresting hemorrhage are given; he speaks, and we think justly, of the great value of opium, at the same time he affirms that the patient is not safe until the uterus is emptied, and its contraction secured. Some practical remarks are further made on that form of hemorrhage which supervenes an hour or two after delivery, arising from the dilatation and expansion of the uterus. Dr. Meigs forcibly insists on the introduction of the hand into the uterus to remove the coagula and excite contraction, and guards his readers against packing the napkins too tightly against the os internum, for thus they act as a tampon, stop the blood in the vagina, which first, and afterwards the uterus, becomes filled.

Varicose veins. At p. 623, after affirming that the evils of varices may in general be obviated by the use of the roller and other remedies, the translator states that he has sometimes seen dangerous and even fatal crural phlebitis supervene after labour, taking its rise in the diseased and distended veins of the leg.

Convulsions. At p. 626, Dr. Meigs suggests that eclampsia is more or less intimately connected with œdema of the extremities, especially in primiparæ. At p. 636 he expresses his confidence that if cases of puerperal convulsions are judiciously treated, the mortality will not exceed 14 or 15 per cent., Colombat's experience being that about one half of the cases prove fatal. Dr. Meigs agrees with Colombat in his treatment of such cases by a bold and vigorous employment of the lancet, but differs from him in the administration of opium, where full bloodletting has been premised, and after the bowels have been unloaded by active enemata. He agrees with Puzos that should bloodletting and other remedies fail in arresting the convulsions, the female should be delivered as soon as possible. He agrees with Colombat and most other pathologists, that the brain when examined after death usually presents no lesions to explain the violence of the disorder. We cannot agree with the translator in his note at p. 645, that eclampsia never occurs in anemic patients or in patients who have been drained by copious hemorrhage.

After-pains. At p. 645 Dr. Meigs most properly suggests the necessity for the greatest caution being used in deciding what is and what is not

after-pain; in doubtful cases, his practice is "to lessen the force of the circulation by a bloodletting proportioned to the exigency."

Puerperal fever. From p. 665 to p. 682 there is a long note on the subject of puerperal fever. Dr. Meigs appears to be throughly conversant with the disease in its various forms, its pathological appearances, and the several modes of treatment. He differs from Fergusson, Collins, and others who regard puerperal fever as "something over and above the local disease," "something beyond inflammation of tissues," and states "that phlebitis of the recently discharged womb alone," "gangrenous inflammation of the inner pains of the uterus," or "inflammation of the peritoneum," all or either is sufficient to produce all the "frightful rapidity which attends the disease," and to cause "the rapid and sudden overthrow of all the functions." He extols most highly the small but excellent volume published by Dr. Gordon of Aberdeen in 1795, and like this physician places more confidence in bloodletting at the commencement of the disease than in any other remedy. Gordon insisted upon 24 ounces of blood being drawn "in the early stage, within twenty-four hours of the attack." Meigs is unwilling to adopt this quantity as "an universal rule," since "the same point is attainable in some by 24 ounces, in others by not less than 30, and in others by 12 or 15." He is guided by the "pulse, breathing, cessation of pain, voice, gesture, *decubitus*, physiognomonic expression, and general sensations of the patient." He differs from Dr. Collins of Dublin, (although professing the greatest respect for his opinions,) in the preferableness of leeching in the epidemic forms of the disorder, and especially on the ground that "the uterine and ovarian circulation is in nowise directly related to the circulation in the skin," and strengthens his remarks by referring to the observations of M. Baudelocque fils, in his 'Traité de la Péritonite puerperale.'

Swelled leg. At p. 683 he very properly corrects an error of M. Colombat, who states that in "milk-leg" the pain "commences in the groin and leg" and the swelling extends "from above downwards." Dr. Meigs says, and we think justly, that the swelling "is in a great majority perceived first in the calf of the leg, which becomes painful, hard, and swollen before the woman suspects that she has any pain at the groin or in the thigh."

ART. VI.

Medical and Physiological Problems, being chiefly Researches for Correct Principles of Treatment in Disputed Points of Medical Practice. By WILLIAM GRIFFIN, M.D., Member of the Royal College of Surgeons in Edinburgh, one of the Physicians to the County of Limerick Infirmary, &c.; and by DANIEL GRIFFIN, M.D., Member of the Royal College of Surgeons in London, Assistant Physician to the County of Limerick Infirmary, &c. *London*, 1845. 8vo, pp. 356.

THE following remark appears in the preface to this publication:

"There are two modes of treating all diseases, a right and a wrong one; and it is popularly believed that the science of medicine has long since determined between them; that in every dangerous case, or, at all events, in those of ordinary or frequent occurrence, the practitioner has only to refer to received prin-

ciples or authorities on the subject, and that if he commits an error in his selection of remedies, it is entirely attributable to his want of information or ability. The uninitiated are little aware of the deep responsibility that rests with the young practitioner, when a decision is demanded of him on such occasions, or the profound judgment, as well as extensive information required to arrive at a correct conclusion between conflicting authorities of equal consideration."

It occurred to one of our authors [we cannot say whether it was the Castor or the Pollux of the Irish profession, as no signature is attached to the preface,] some years since, that it would be a most useful, practical, and interesting study to collect cases with reference to all the most important of disputed points in practice, and afterwards, as a sufficient degree of personal experience happened to be attained on any one of them, to review the opinions of all the best authors on the subject, and endeavour to solve that most difficult question—what is the correct principle of treatment? The idea was excellent, and the results of its accomplishment are in the work under consideration.

The first four problems are so closely related to each other, that we shall take them together. They virtually refer, as might be expected from the previous publications of the authors, to the diagnosis and treatment of spinal irritation, although the first seems to have no connexion with that form of disease. The four problems are:

"1. What principles should be kept in view by the physician in the treatment of enteritis?"

"2. How are nervous affections distinguishable from inflammatory?"

"3. What is the diagnosis of abdominal inflammations?"

"4. In spinal irritation, is there really any affection of the spinal cord, or of its membranes?"

ENTERITIS.—*Treatment.* The discussion of the first problem is not to the point. Its object is to show, that enteritis is best treated by opium. The history of a case so handled is preceded by extracts from systematic writers as to treatment by bleeding and purgatives, tending to show considerable discrepancy of opinion as to *extent* at least of depletion, and as to the time and mode in which purgatives should be administered.

Parr and Good advise moderate depletion as less likely to be followed by gangrene; Abercrombie advises more vigorous measures, and Elliotson directs the patient to be set upright, and bled "from a large orifice without mercy." The tone of confidence and decision usually adopted by the latter writer has gained him many disciples, but we suspect that mature experience will very much shake their trust in their leader, and we would refer to the last dictum of bleeding "without mercy," as one likely to be questioned. It is well known that cases of enteritis may be cured safely and quickly without any depletion whatever.

Authorities take two sides on the use of purgatives in enteritis. Parr, Pemberton, and Good recommend mild purgatives at first; then, if necessary, the more active. Dr. Elliotson is confident, *more suo*, and we think most dangerous in his advice. "We should first," he tells us, "bleed freely, because purgatives will not operate until we have done that; we should then give a large dose of calomel, such as a scruple, by the mouth, and a strong purgative injection, with plenty of salts, or salts and senna, or colocynth, or oil of turpentine, and repeat the calomel in ten grain doses every four or six hours, *giving purgatives in addition from time to*

time." When we consider how readily enteritis may be mistaken for ileus, or spasmodic cholic, the danger of such treatment is manifest. Dr. Abercrombie recommends that the purgatives be not administered in the early stage. This plan of treatment is that, we believe, which the majority of modern physicians adopt.

We will now give an abstract of the case before referred to.

A lady, aged 32, affected with symptoms of enteritis, was bled to eight ounces, and two dozen leeches applied to the right hypochondriac and iliac regions. Two grains of calomel and one of opium were given every second hour. No relief following, the next day twenty ounces of blood were taken from the arm; aloes and henbane given to operate on the bowels, and a terebinthinate enema administered.

"On administering this last, the patient was seized with a dreadful forcing or bearing-down pain in the rectum, and passed nothing; the pain seemed as excruciating as any that could occur in violent labour, lasting for about twenty minutes, and was then relieved by the warm bath. In two hours afterwards, the administration of a simple injection of oatmeal tea was followed by similar suffering, and was in like manner retained. The permanent pain was at this period severest in the left iliac region and about the navel, where the tenderness on pressure was extreme; the countenance was more anxious; the tumidity of the abdomen was increasing, and the stomach beginning to reject the drink. In consultation with another physician, it was now agreed to take blood again, and eighteen ounces more were drawn, being the third general bleeding within twenty-four hours. Two grains of opium and a grain of calomel were given immediately after, and ordered to be repeated every two hours through the night. In the morning (the 4th) there was a considerable improvement; the abdominal tenderness was diminished, the pain and sickness of the stomach had very much subsided, and the injections had come away with some dark, thin, feculent matter. She still, however, felt pain and a sense of great weariness at the lower end of the sacrum, shooting up through her back, and she had a great difficulty in passing water. She now informed me that a few days previous to her present illness, she was attacked with a profuse leucorrhœal discharge, attended by heat and sense of scalding, but that it had since abated or almost ceased. A fomentation to the lower part of the abdomen was ordered, and the opium was continued in two-grain doses every two hours, without calomel. In the evening the improvement appeared progressive; the skin was cool, the pulse soft at 110, the tongue cleaner; the abdomen was still full, but it had nearly lost its tenderness, and she could turn in the bed with little pain." (p. 5.)

It was now discovered that there was a purulent discharge from the rectum:

"On the next evening, as she lay on the sofa while her bed was making, she felt a solid substance passing from the rectum, which alarmed her terribly. It was found to be a rope of sloughy stuff, soft and purulent outside, but tough and fibrous within, not unlike the ischiatic nerve in a decayed state, suspended from the rectum for the length of a foot or more. On attempting to draw it away, it appeared to be still adherent within the gut, and she complained of pain. After a little, however, it was removed without much effort, and a gush of matter to the amount of perhaps two tablespoonfuls followed. The slough was about the thickness of the thumb or more, and was fifteen or sixteen inches in length. We at first supposed it was a portion of the small intestine which had mortified and been thrown off; but on close inspection no distinct traces of a canal could be found. Some time after an injection of warm water and sweet oil was administered, which came away in about twenty minutes mixed with some matter, but without any appearance of fæces. On examining the rectum, a rugged irregular edge

was felt on the posterior side, close to the sacrum, as if it was the termination of the part from which the slough had been cast off; the examination gave much pain, especially when the intestine was pressed upon within. Several days passed without much alteration in the case: there was matter daily discharging to the amount of three or four ounces, and there was at times severe dysury, at last demanding the use of the catheter." (p. 7.)

Three days having elapsed since the pain and tenderness of the abdomen was experienced, and six since the bowels were moved, an aperient was requisite, and a dose of castor oil given, followed by pills of aloes and henbane. All the symptoms now recurred in a more aggravated form. There was increasing distension of the abdomen, the pulse became feeble and rapid, the thirst extreme, the vomiting frequent, the countenance was sunken, the look anxious, and the face covered with a clammy perspiration. Three grains of opium were given as a first dose, and two every second hour afterwards; a dozen leeches applied to the abdomen, and fomentations with decoction of poppy-heads. The effect was wonderful; the pain and tenderness gradually abated, the pulse became slower, the sickness ceased, and the expression of countenance improved. Matters went on well until the seventh day from that on which the bowels were last moved, without any evacuation. At that period, however, (as Dr. Griffin anticipated,) "the tenderness in the left ilium was again felt, and it was soon followed by pain and feverishness, with a disposition to vomit."

"There was now no doubt on our minds that the recurrence of the attack was attributable to distension and not to perforation of the intestine, as we had apprehended. After giving a large opiate, therefore, she was ordered a few grains of calomel, with mild doses of castor oil and jalap every second hour, until the bowels were freely moved. Great relief was obtained, but the pain and tenderness of the abdomen finally subsided on resuming the opiates for twelve or fourteen hours after the purgative had ceased operating."

The case now gradually but slowly advanced to recovery:

"A mild purgative was given at the farthest on every fourth day, which operated without creating pain or uneasiness, and by diminishing the interval gradually, the bowels were after a little brought to act daily with a small dose of rhubarb and cascarilla. She was still, however, unable to sit up in the bed, or to turn to either side on account of the excessive soreness inside the sacrum. The motions continued to be smeared with matter; sometimes small bits of fresh slough came away; sometimes spoonfuls of healthy pus with stuff like jelly. Weak sulphate of zinc injections, and even those of simple water were made use of, but they gave great uneasiness, and served to do more harm than good. At this time, about eight weeks from the commencement of the attack, she became very hysterical, got fits of crying and laughter, which lasted for hours, and was sometimes slightly delirious. She had been kept very low all through her illness, but was now allowed nourishing diet, meat, and a little wine; there was an immediate improvement in all the symptoms; her strength and health mended; her mind became cheerful; the discharge of matter diminished, and at last was only occasionally observable. The soreness about the sacrum was also lessened so considerably that she was able to dress, lie on the sofa, and sometimes sit up for a short time. At the end of three months she could move about the room a little, and at the termination of the fourth she was perfectly recovered." (pp. 10-11.)

In commenting on this interesting case, we may fairly award to Dr. W.

Griffin the merit of saving his patient's life; but we should have been glad to have learnt more about this sloughing of the rectum. Why did that excruciating, dreadfully forcing pain come on immediately after the administration of the terebinthinate enema? *Is it possible that the nurse perforated the bowel with the enema-tube?* How was it that distension of the bowel in the left iliac region caused the sloughing of the rectum, and the rugged, irregular edge at the posterior side close to the *sacrum?*

With regard to the principles of treatment in enteritis, Dr. W. Griffin is in favour of a free and early bleeding, and if this fail, bleeding must be abandoned as the main resource, and employed as auxiliary only. With regard to purgatives, he observes that in the greater number of cases of enteritis, the favorable termination is by a free evacuation of the bowels, and that before this occurs relief is seldom obtained. To the experience of this strong fact may be referred, he thinks, the popularity of the purgative treatment. But Dr. W. Griffin shrewdly remarks:

"Effects are too often, in the science of medicine, mistaken for causes. When cholera first appeared in this city calomel was profusely employed in its cure, and it was eventually found that patients who became salivated almost invariably recovered. This was esteemed proof positive of the efficacy of the treatment, and mercurials became more popular than ever. I found, however, on examining the registries of the hospital with which I was connected, at the termination of a month, that in the stage of collapse no more than one patient in ten could be brought under the influence of mercury, so that there were only four recoveries in forty. This told little for the remedy as far as cases in relapse were concerned, and I immediately set about ascertaining what the amount of spontaneous recoveries might be in the same stage. From all I could gather from the experience of others or my own, I began to suspect that they would reach nearly the same amount; and at last I became perfectly convinced that the actual fact was, *the patients did not recover because they were salivated, but they were salivated because they recovered.* Mercury in any shape, in the stage of collapse, was thenceforward discarded from my practice in the hospital, and though it excited some observation at the time, the subsequent experience of the profession at large bore me out in the decision. I cannot but feel that somewhat of the same error prevails with respect to purgatives in enteritis; the disease is not a very common one, and the experience of an individual could scarcely warrant him in offering opinions at all confidently, when they are opposed to general practice; but certainly all the information I can glean, or the experience which has fallen to my share, would dispose me to say that, in intestinal inflammation, the relief obtained is seldom the direct effect of the purgative, and that *people do not recover because they are purged, but they are purged because they recover.*" (pp. 15-16.)

After a perusal of the preceding case our readers will readily imagine that Dr. W. Griffin is a warm advocate for the treatment of enteritis by full and frequently repeated doses of opium, and details several cases at length as illustrative of its success. There is some doubt on our minds as to the diagnosis of many, if not most of the cases of enteritis, and this doubt introduces us to the two next problems: How are nervous affections distinguished from inflammatory, and what is the diagnosis of abdominal inflammations?

Diagnosis. The term enteritis is, we believe, now generally understood to mean inflammation of that part of the peritoneum constituting the outer coat of the intestines. The characteristic symptoms of this as well as of other forms of peritonitis, are pain and tenderness on pressure of the

abdomen. Dr. W. Griffin urges that these symptoms are really fallacious in diagnosis; that they exist in an exquisite degree in affections of the spinal cord or its membranes; and that the diagnostic sign of the latter class is tenderness on pressure at the portion of the spinal column corresponding with the disturbed organ. The following paragraph expresses so clearly Dr. W. Griffin's opinions, that we subjoin it at length:

"There are three effects very common to inflammation, or disorder in the spinal cord, or at the trunks of its nerves. First, superficial tenderness, more or less exquisite, and either limited to the integument immediately over or about the affected portion of the cord, or extending thence to the front of the abdomen or thorax, in the direction of the spinal nerves which have their origin there, or in the ganglionic nerves supplying the viscera, which have connexion with that portion of the cord. Secondly, loss of power evinced in partial or complete palsy of the parts or organs to which the affected nerves are distributed. Their effects often occur simultaneously, but any one of them may also occur independently of the existence of the others, offering very strong evidence that the sensibility of the surface or skin, and that which exists in internal organs, is dependent upon nerves, which though sentient, are as distinct from one another as they are from those on which the power of motion depends. Keeping these ordinary effects of disorder of the spinal cord in view, it must appear obvious, when we detect soreness or tenderness on pressure in the region of the liver or spleen, or in the lower abdominal viscera, how important and essential it is for us to ascertain whether the soreness be superficial or deep-seated, which we can always do by careful examination. Again, when pain is complained of in the region of the liver, or middle or lower parts of the abdomen, how necessary it is to ascertain whether, as in the case of soreness, it be superficial, and if deep-seated whether it be merely an affection of the nerves of the part, and probably connected with some affection of the adjoining portion of the spinal cord; or whether the internal organ itself be in a state of acute or chronic inflammation. Finally, if there be oppression in breathing, whether it arises from deficient action in the respiratory muscles, or the imperfect performance of the process of oxygenation of the blood in the lungs, or from actual inflammation, or organic disease of the mucous membrane, or parenchyma of these organs. Or if there be obstinate constipation of bowels, whether it depends on spasm or enteric inflammation; or whether purely on deficient power, or partial palsy of the muscular fibres of the intestines. I know of no case which so often deceives inexperienced or uninformed practitioners as obstinate constipation of bowels from paralysis of the nerves, when connected, as it frequently is, with abdominal pain and exquisite soreness on pressure." (pp. 49-50.)

It seems to be generally allowed that the first symptoms of ileus or colic resemble those of enteritis. There is usually the same tenderness on pressure, the same obstinate constipation, the same kind of twisting pain with vomiting, and anxious expression of countenance. These symptoms are consequent on all violent stimulation of the intestinal canal. It is only, in fact, by the *termination* of the attack that ileus differs from colic and enteritis. If the spasmodic action constituting the disease relaxes speedily, and the transit of the fæces along the canal is re-established, then it is only a case of colic; but if the spasm becomes persistent, the compacted fæces, already acting as an irritant, continue that irritation, and ultimately inflammation of the intestine, extending to its peritoneal coat, is set up. It is manifest, then, that in such cases opium and its adjuvants must be the key-stone of the plan of treatment; purgatives will only add to the irritation. The leading indication is to unlock the stricture

of the bowel, either by opium or copious mild enemata. Unless this be done, enteritis will as assuredly follow the retention of the fæces, as cystitis follows retention of the urine in spasmodic stricture of the urethra.

In what morbid condition will this spasmodic stricture of the intestinal canal originate? It may be and probably is always connected with irritation of the muscular fibres of the canal, through the medium of the nerves distributed on the mucous surface. This irritation may depend, firstly, on the nature of the irritant. An alvine concretion, an indurated mass of fæces, or a large accumulation of the latter in the colon, as in the case before detailed, will induce a sufficient amount of irritation and consequent spasmodic action. We think the last mentioned is the most frequent irritant of all, as in most of the cases that we have witnessed relief has followed on repeated and enormous evacuations. Secondly, the exciting cause of the irritation may be in itself trifling, but there may be a too great susceptibility of (in Hallian phrase) the incident-excitor nerves of the muscular structures of the intestines, and thus spasm of those structures be excited, just as tetanus or trismus arises from impressions on the skin. Now the question arises: 1st, whether this susceptibility is connected with "irritation" of the *spinal* ganglia, or of the *sympathetic* ganglia; and, this being ascertained, 2dly, whether that susceptibility is *invariably* or even usually conjoined with a similar irritability of the spinal sensitive nerves, or the sensory tract of the spinal cord? In tetanus, we know that the sensitive nerves of the surface are not more than usually susceptible; impressions upon them excite spasm, but no pain. The seat of the irritability is in the motor tract; the pain is in the affected muscles. By parity of reasoning we may infer that the pain of colic is seated in the muscular structures of the bowel, and not in the nerves of the mucous membrane or of the skin.

According to these views, neither irritation of the spinal cord nor of the sympathetic ganglia, is *necessarily* accompanied with increased sensibility of the skin or mucous membrane of the intestinal canal, and consequently that spinal tenderness is not diagnostic of spinal irritation, nor of those spasmodic affections which originate in some morbid condition of the spinal cord. It can only be considered so when the morbid change is not limited to the motor tract, but extends as well to the *sensory*.

That the muscular contractions of the intestines depend in a great degree upon the sympathetic ganglia and their twigs, is manifest from the persistence of the peristaltic motions after entire removal from the body; consequently, it is not clear to us that the spasmodic affections of the viscera, supplied with sympathetic nerves, are in all cases connected with irritation of even the motor tract of the cord. The *ganglia* may be the primary seat of the irritation, and the *sensory* tract irritated secondarily.

We need go no farther than Dr. Griffin's Essay for pathological proofs of our views. We subjoin two cases, the one of enteritis, according to Dr. Griffin's diagnosis; the other a case of spinal irritation:

I.—*Case of enteritis.* "A strong active man, aged 30 years, was seized with pains in the *abdomen, chiefly about the situation of the umbilicus; the pain came on more violently at short intervals, but never ceased,* and was attended with *extreme soreness of the abdomen,* especially in the situation of the pain. The stomach was sick. He got castor oil and turpentine, which was vomited up again. He after-

wards got repeated doses of calomel and colocynth with injections, but the former produced no effect, and the latter came away as they were administered, without any admixture of fæces. He was subsequently blooded to the amount of thirty ounces or upwards, with some little relief to the pain, and the purgatives were continued, but after some short time all the symptoms became worse. About ten o'clock at night the patient's friends became exceedingly alarmed at his increasing illness, and desired a consultation; upon which occasion I was requested to see him.

"I found him lying on his back, *moaning faintly*, and complaining of constant pain about the umbilicus, which at intervals of a few minutes increased to a violent degree,—*he had frequent retching, and could retain nothing solid or fluid upon the stomach;* his countenance was dejected; his pulse 100; his skin rather warmer than natural. He had had no movement of the bowels since the commencement of the attack. The centre of the abdomen was covered with a blister, so that I could make no examination as to the degree of tenderness; but all the parts above, below, and at the sides beyond the margin of the plaster, were excessively sore to the touch. The medical gentleman in attendance, finding all his efforts to get the bowels moved were unavailing, was just preparing to give him some croton oil.

"I represented to him how very unlikely it was that by any purgatives he could get an inflamed bowel to act; that inflamed muscles never do. That the chief object appeared to me to be to subdue the pain and inflammation, leaving the evacuation of the bowels entirely to nature, which would probably effect all that was desirable when not interfered with by an inflammatory condition of the parts. I proposed that the blister, which had been on only about two hours, should be removed, and that eighteen or twenty leeches should be applied about the umbilicus, where the pain was most acute; that warm fomentations should be afterwards applied both to allay the suffering and encourage bleeding from the leech-bites, and that *two grains of opium with two of calomel should be given immediately, with one grain of each every hour afterwards,* until the patient fell asleep. On the medical attendant expressing some alarm at the quantity of opium which might be given in this way, and the bowels obstinately constipated, I observed, that if sleep could be procured, the man would probably awaken freed from all pain, and have his bowels moved without the necessity of a purgative.

"The leeches were applied, and the bites bled freely with the fomentations. *The opium remained on the stomach, and soon after the patient took the third dose the pain evidently abated.* In less than an hour he became easy, had a heavy look, and before the hour came round for the next dose was in a sound sleep. *At seven o'clock in the morning, after six or seven hours uninterrupted sleep, he awoke freed from pain or sickness,* and on being assisted to the night-chair, had a free, easy, and copious evacuation. He had one or two more motions in the course of the morning, and required no further treatment." (pp. 30-31.)

II.—*Case of spinal irritation.* "A young woman, aged 25 years, was attacked with pain in the bowels at night, after a feeling of chilliness. She took some essence of peppermint and went to bed, but the pain gradually increased, and at two o'clock in the morning she took twenty drops of laudanum. At seven o'clock, the pain still continuing, she took castor oil, with ten or fifteen more drops of laudanum, by the directions of an apothecary, and soon after ten drops were repeated in a saline draught. I saw her at one o'clock, and found her *writhing with pain chiefly round the umbilicus, and to the right side. It became more violent by fits, like colic, though never entirely subsiding;* and during the intervals of comparative relief, she sometimes threw her arms about restlessly, sometimes lay as if insensible, with the eyes turned up, and the lids half open. *Her complaints were low, scarcely audible;* her respiration painful when deep. Turning from side to side increased her pain, although when the paroxysm occurred she turned sometimes on her face. *There was excessive tenderness of the*

abdomen, the least pressure making her scream. ***She had been constantly vomiting for the last few hours.*** The castor oil had operated once scantily; her pulse was but little quickened, and there was no heat of skin.

"Here was a case of constant pain in the abdomen chiefly about the umbilical region, liable to severe exacerbations, attended by exquisite tenderness on pressure, vomiting and constipation, and continuing for twenty-four hours. I am convinced that almost any young physician would have felt great difficulty, indeed almost an impossibility of determining, from a consideration of the symptoms, that the complaint *was not inflammatory*; and I believe that the great majority of either young or inexperienced ones would at once infer the existence of inflammation, and bleed. I say so from having witnessed it, and from having early in my own professional life always prescribed in such cases with timidity, as if I felt that all consideration of the symptoms led to little better than conjecture. I had now, however, new means of diagnosis in the state of the spinal column, on examining which my mind was set at rest. As soon as I pressed on the upper lumbar vertebræ the girl started violently, caught my hand, and complained that I had hurt her dreadfully; the pressure, she said, had increased the abdominal pain. She had never had any hysterical attack in her life. I ordered the abdomen to be fomented freely, and gave her ***five grains of calomel with half a grain of opium***, directing five grains of aloes and five of extract of henbane to be taken every two hours. The calomel and the first dose of the aloetic pills remained on the stomach, but the succeeding doses were rejected; a purgative draught was also thrown off. She then got a purgative enema, which was repeated in an hour, but both passed off without any appearance of fæces. The vomiting, pain and tenderness of the abdomen, continued very severely throughout the evening. At night she got a turpentine enema, and was placed in a hot bath, which gave some relief and procured a scanty evacuation. The pain, however, soon recurred, and ***she was ordered a grain of opium, to be repeated every hour until it should subside.*** The bowels were freely moved soon after taking the first dose, but ***she did not experience any considerable relief until she had taken the third, after which she fell into a sound sleep, and in the morning was in every respect improved.*** The pain had altogether subsided, and the exquisite tenderness was now felt in the right epigastric region only. She had threatenings of a return of the attack in the course of the day, but it was readily subdued by a repetition of the opiate. On the following morning there was scarcely any pain or tenderness; and if the complaint had been inflammatory, I would now have left the patient's bowels perfectly at rest; but believing it to be an affection of the spinal cord, arising from disorder of the digestive functions, I ordered another dose of castor oil, which operated freely, and was followed by no recurrence of the attack." (pp. 40-41.)

Upon a comparison of these two cases, it will be seen that in their symptoms, course, and termination, they are identical. To avoid repetition, we have underlined passages in each which are almost parallel. It is true, in the one case there was no spinal tenderness, and in this no leeches were applied, or calomel administered in repeated doses, as in the other case; but in the accompanying examples of spinal irritation detailed by Dr. Griffin, simulating enteritis, we find both these means were conjoined with the opium. The principal circumstances to be observed are these: firstly, that opium is given equally in both classes of affections, and with equally beneficial results; secondly, in the cases of enteritis, that is, in those in which there is no spinal tenderness, a general bleeding is practised. These latter cases may indeed, after all, have been "cases of spinal irritation," as Dr. Griffin acknowledges that spinal tenderness is sometimes absent. We think Dr. Griffin might safely treat cases of this

kind in the same way as he treats cases of the former; that is to say, omit the general bleeding. Terebinthinate and all other stimulating enemata should also be avoided as injurious, and copious lavements of simple warm water, or medicated with æther or laudanum, should be substituted. These, conjoined with full doses of opium, leeches, and fomentations, we have invariably found to be successful in cases like those discussed by Dr. Griffin. The spasmodic stricture will sooner or later relax, the pain will cease, and copious evacuations take place. We shall recur to the consideration how far opium may be safely administered in diseases of the spinal cord, in the discussion of the next problem.

PROBLEM IV. *In those disorders which have been termed spinal irritation, is there really any affection of the spinal cord or its membranes?* The solution of this problem comprises a criticism of a critic. We gather from this chapter that Dr. Griffin, to use his own words, really knows nothing at all of the nature of spinal irritation, or of the pathological condition with which it is connected. There are no opportunities of investigating its pathological anatomy, and if there were, it is doubtful, Dr. Griffin thinks, whether any alteration of structure could be specifically connected with the symptoms; instancing the several forms of mania, epilepsy, and chorea, as analogous. There is certainly, however, "a morbid condition" of that part of the spinal cord corresponding to the seat of pain; and in proof of this proposition, Dr. Griffin gives a tabular summary of 148 cases in which there was tenderness of the cervical, cervico-dorsal, dorsal, dorso-lumbar, and lumbar regions respectively, with simultaneous functional disorder of the viscera corresponding to these regions. The solution of the problem is however imperfect, for we have no information afforded us as to the origin or nature of this morbid condition. Dr. Griffin has, in fact, failed to illustrate the class of affections to which he has devoted so much attention, by the more accurate spinal anatomy and physiology of the day. This defect is the more inexcusable, as since he first wrote on the subject one or two complete monographs have been published. For a short synopsis of our own views, we would refer to p. 96 of our 37th number; and for some views regarding the anatomy and physiology of the spinal cord, (and without a thorough knowledge of these it is impossible to understand the pathology of spinal irritation,) we would refer to our 34th number. The causal diagnosis is all-important in these cases, with reference to the treatment. Thus the neuræmia, arising from the incident excitor action of the ovaria on the spinal cord, may not be confined to the lumbar region, but, as Dr. Laycock has clearly shown, may affect the dorsal and cervical region, or even certain portions of the brain, in consequence of the extensive affinities of the ovaria to the nervous centres. Now, in such examples (as hysterical cunning, pica, &c.) a merely local treatment cannot be consistently recommended.

To furnish another example: gout in females assumes almost always the anomalous form; it most rarely attacks the serous or sero-synovial structures of the toe; more frequently those of the joints of the fingers; probably quite as frequently those of the spinal column. We should therefore be on our guard against cases of this kind, by a close investigation of the constitutional peculiarities of the patient, and we shall thereby escape treating arthritic or rheumatic inflammation of the spinal meninges,

or articulations, as if simply cases of neuræmia, induced by an incident excitor action of the kidneys, ovaria, or other viscera on the spinal ganglia. The same method will assist us in distinguishing myelitis as well from spinal irritation as from visceral inflammation. Dr. W. Griffin relates an instance of myelitis, the leading symptoms of which closely resembled those of enteritis. The captain of a merchant-vessel, a hale, hardy man, was attacked by pain in the back and belly, with obstinate constipation. After undergoing bleeding and purgation, Dr. W. Griffin was called into consultation, and found the patient complaining of great pain in the abdomen and round the loins, with difficulty of passing water, and obstinate constipation. The entire surface of the abdomen was very tender to the touch, and in the pubic region excessively so; there was also over nearly the whole trunk of the body an acute soreness of the skin, especially about the hips and lower extremities, which made it painful to turn himself in the bed, or to permit any one to assist him in doing so. There was great tenderness on pressure along the whole spinal column, but he complained of the pain only in the lumbar portion, and in the corresponding parts of the abdomen in front; his skin was hot, his tongue foul; his pulse, small and rather feeble, at 128. He had been bled to the amount of more than a quart the night before, and was continuing the purgatives and enemata. The treatment Dr. W. Griffin recommended was such that the patient took twelve pills within the same number of hours, containing two grains of calomel and one grain and a half of opium in each, without relief to the pain. Pills of crude opium (one grain each) were then substituted and continued for twenty-four hours, and at last acetate of morphia and mixture of henbane. Sleep now came on, and the pain abated. At the next visit, however, symptoms of softening of the cord made their appearance; the left arm was paralytic and tender to the touch; in five or six days after the right arm was paralyzed. Subsequently, each of the lower extremities became successively affected.

Dr. W. Griffin was of opinion from the first that the disease in this case "was some inflammatory affection of the spinal cord or its membranes." Acting upon this opinion he prescribed the calomel and opium, as already stated; but as a placebo for his colleague, "two dozen leeches were applied to the pubic region instead of the lumbar vertebræ," to which, in Dr. Griffin's opinion (and we may add in ours), they ought to have been applied; and shortly afterwards "consented to the anxious desire" of his colleague to apply a blister to the abdomen. It is manifest that the treatment of this case was wrong. We need say nothing as to the use made of the leeches and blister. No placebo should be administered to a colleague at the expense of the blood and ease of the patient. As to the opium, Dr. W. Griffin shall be his own judge. The following is an extract from the discussion of the problem, *What are the therapeutic effects of opium?*

"There are some inflammatory affections to which, after all, it does not appear that opium can be ever advantageously applied. One of its direct and obvious effects given in large doses to healthy persons is to produce congestion of the brain, and *probably in the spinal cord.* Of this we have sufficient evidence in the apoplectic symptoms it occasions, in the relief of these symptoms by large depletion; and again in the recovery of persons dying of hæmorrhage by large doses of it, which I conceive is effected purely by the slight congestion it is then

capable of inducing, restoring the healthy tension of the vessels in the brain, and so preventing syncope and death. When given in acute inflammatory affections of the brain, therefore, its tendency is to produce a state of circulation in the organ, which must rather aggravate than diminish the mischief going on there. I believe *this is true also in inflammatory affections of the spinal cord*, since it is only on that presumption I can at all account for its total failure as a remedial agent, in some cases which have fallen under my observation, and in which it got a fair and full trial." (p. 137.)

PROBLEM V. The next problem is propounded and discussed by Dr. D. Griffin: "*Under what circumstances, and to what extent, is bleeding proper in diseases of the brain?*" By the term, diseases of the brain, Dr. D. Griffin more especially means apoplexy and apoplectic affections, an indefiniteness of expression sufficient to raise an expectation of indefiniteness in the writer's views. And it is even so. Dr. D. Griffin first shows the contradictory opinions of various writers as to the utility of bleeding in cases of apoplexy; and he attributes this circumstance to varieties of structural changes now known to take place in the brain, and formerly to a great extent unknown. Dr. D. Griffin mentions "one conclusion of immense practical importance," namely, "that in considering the expediency of bleeding, *the actual degree of organic disease that exists* is of infinitely more importance than its seat or nature,—in other words, that bleeding is badly borne in all cases of extensive organic disease of the brain, *wherever the disease may be situated or whatever be its nature.*" Now, on the opposite page, Dr. D. Griffin observes, that "symptoms cannot be depended upon as indicating the particular place, or the *actual amount* of cerebral disease;" we therefore cannot see how this conclusion can be of such immense practical importance.

Dr. D. Griffin is of opinion that large bleedings are improper in all affections of the brain attended with severe and protracted pain, as such cases usually die from exhaustion induced by their sufferings.

Not one word is said of functional disease of the brain, so much more common than structural diseases; as for example, those consequent on disease of the kidneys and heart.

PROBLEM VI. "*On what morbid states does the occurrence of coma and sudden death in jaundice depend?*" Dr. W. Griffin discusses this problem and comes to this common-place conclusion: "If, with such imperfect materials, even a conjecture might be hazarded, I should, on the whole, be disposed to say, that the cerebral affection is rarely the primary disease, but is superinduced, we know not how, by the suppression of a most important excretion, as it sometimes is in the suppression of the catamenia, and almost always of the urine." This problem is discussed without any reference to the important fact, that in many cases of jaundice a large proportion of the bile carried into the circulation is excreted through the kidneys.

PROBLEM VII. "*Is the law of visible direction, as at present received, a true one?*" Dr. D. Griffin deduces, from various ingenious experiments, a solution in the negative. He thinks, "we must, for the present, rest satisfied with the simple statement, that when rays of light fall on any point of the retina, that point has the property of representing the object from which they come in *its true direction*, without any regard to the obliquity of their incidence."

PROBLEM VIII. *"Is laryngismus stridulus, or the crowing disease, a spasmodic or paralytic affection?"* This essay is a detailed criticism on the late Dr. Hugh Ley's views as to the pathology of that disease. The objections are well put, although we believe the refutation of Dr. Ley's theory is in the present day unnecessary, as few practitioners found their plan of treatment upon it. Dr. W. Griffin shall speak for himself as to the solution of the problem.

"The foregoing observations must sufficiently display the error Dr. Ley has fallen into with regard to the effects of palsy of the recurrent nerves in occasioning the phenomena of laryngismus stridulus. On the whole, after all the consideration I have devoted to the complaint, and having, I think, given other ingenious arguments of Dr. Ley their full weight, I must still confess myself a disciple of the older doctrine, that the affection is one of spasm or partial convulsion, like cramp, rather than of paralysis. The fact of its being frequently benefited by antispasmodics, with which Dr. Underwood tells us he latterly cured most cases, and by anodynes, as opium, hemlock, cicuta, &c., recommended by all modern writers on the disease, favours this view; the circumstance of the sudden occurrence of the gasping and crowing on washing with cold water, laughing, crying, or agitation of mind, also supports it, as well as the almost universal coexistence of the carpo-pedal contractions, and the frequent termination of the complaint in convulsions. But above all these, as a strong analogical evidence for its spasmodic character, I place its paroxysmal nature, and the manner in which the paroxysms occur. The office of the superior laryngeal nerves would lead us to expect a disposition to spasmodic action on the least irritation or excitement, but far more on the increase or decrease of the susceptibility of the parts, and disposition to spasmodic action..... One can well understand how dangerous any morbid increase of the sensibility of such nerves at their extremities, or any existence of irritation at their origin, might prove; why the danger should occur in irregular paroxysms, and why the exciting cause which occasioned them on one day should be altogether powerless on the next. If it be inquired further, why such a dangerous result as the suspension of respiration in the crowing disease does not occur more frequently, it can only be replied, that we are wholly ignorant of the morbid condition which disorders the functions of those nerves, or whether it exists at their extremities, or their origin in the medulla oblongata. If we suppose the affection to be organic, we should find it more difficult to account for the occasional recoveries under very mild treatment, than the usual fatality under the most active. If it be functional, and therefore symptomatic, we can better understand why it might depend on a variety of causes; at one time upon an affection of the head, at another of the bowels, at another upon dentition; we can comprehend, too, how these several affections, influencing peculiar predispositions, may in one child occasion hydrocephalus, in another convulsions, in a fourth that more rare infantile disorder, the crowing disease." (pp. 135-6.)

We are rather surprised that Dr. W. Griffin has not seen the analogy between this affection and cases of spinal irritation. A more thorough acquaintance with the physiology of the spinal cord would have enabled him to understand, how the changes induced on the jaws during the process of dentition would exercise an incident excitor influence on the central axis, and develope there the neuræmic state in those predisposed. He would also have understood the true bearing and significancy of statements like the following: "why are not grown children attacked with laryngismus stridulus? I have never seen the complaint except in mere infants; all Mr. Robertson's and most of Dr. Ley's cases appear to have been under two years of age." (p. 131.) The process of dentition is the external index

of a general change in the system,—it is a stage of development; and it is for this reason that laryngismus and other spasmodic affections are more frequent during dentition, as attacks of spinal irritation most usually occur in females about puberty. As the predisposition is the greater, so is the attack the earlier; and in those highly predisposed, it will often appear with the *premonitory* symptoms of dentition. On the other hand, it not unfrequently occurs during the *third* year, when the molares are protruding.

PROBLEM IX. "*Does suffering necessarily imply self-consciousness? Are sentient beings necessarily percipient?*" Dr. W. Griffin draws a distinction between mental consciousness and sensation. The former "implies, not only the perception of thoughts and sensations, but the reference of these to something that remembers the experience of the former thought or sensation, which believes it existed before the present moment, and that it was itself which experienced all." Sensation consists in a mere sense of the present, neither including remembrance of the past nor anticipations of the future. This may be divisible; the other is indivisible. It is thus that Dr. W. Griffin, in rejecting the reflex theory, explains the facts on which that theory is based. A decapitated tortoise, when irritated, conceals itself beneath its shell: this it does because it is a *sentient* being; it has no will, no memory, no self-consciousness; but it suffers nevertheless, for it adapts its actions to a determinate end. All this appears to us contradictory both to the ordinary meaning of words and to facts. Firstly, we cannot imagine any creature suffering without being conscious of pain; secondly, adaptation in the animal implies reasoning; thirdly, we know certainly that movements usually connected with highly pleasurable or painful feelings have occurred in paraplegic patients, without any suffering or feeling whatever on the part of the individual. We allude to examples in which sexual intercourse has been completed by persons of this class, and also to retraction of the legs on irritation of the insensible skin. But, in fact, Dr. W. Griffin grants the principle that determinate and adapted movements may take place without consciousness. He observes, "that the contraction of the iris on the admission of light, of the lids in winking, and of the uterus after death, in those cases in which labour is said to have been completed in the coffin, are sufficient to prove that sensation is not, in all cases, essential to the accomplishment of the action which usually succeeds it." This admission puts an end to the argument.

The question really with Dr. Griffin resolves itself into this, What *amount* of determinate and adapted movements may take place without sensation? Dr. Hall thinks more, Dr. Griffin less; but others go farther than either, as Dr. Griffin will have learnt already. New views as to the nature of the phenomena under discussion, and indeed, of mental phenomena generally, are gradually coming into notice. It is at all events certain that the adaptation spoken of is not done *by* the animal as a mental act, but are effected *in* the animal as the result of an inner mechanism adapted to external impressions. The closure of the eyelids, the contraction of the iris, &c. are examples.

PROBLEM X. "*What are the therapeutic effects of opium?*" The re-

sults of an opiate are sometimes sinking, and faintness without stupor, and occur many hours after exhibition. These symptoms

"May be removed or alleviated by renewing the opiate in a smaller dose, or, if other considerations render this unadvisable, by stimulants frequently repeated, and eventually by sleep. It is often of great importance in medical practice, and especially so in all dangerous cases where opiates may have been largely administered, that we should remember a period of exhaustion and sinking is likely to come on from twelve to twenty-four hours after the opiates have been discontinued. It almost invariably occurs where the opiates have previously been long continued, or taken in frequently repeated doses, or where there is a great constitutional susceptibility to their action..... I advert to the subject chiefly on account of the danger or difficulty which often arises, when the physician either forgets the quantity of opium he has been giving, or is not sufficiently alive to the probable effects of its sudden withdrawal. Finding his patient sink, his countenance pallid and depressed, and his whole appearance betraying, sometimes, signs of excessive debility, he attributes the change to some unfavorable turn in the course of the disease, becomes alarmed for his safety, and, perhaps, resorts to medical measures either unnecessary or mischievous. This is particularly apt to occur in the cases of young children or infants, when the violence of the inflammatory symptoms has gone by, and it has been found necessary to administer opiates. About the time the influence of the opiate is wearing out, if it has been a large one, the eyes look sunk, the eyelids lie half open, disclosing a small portion of the white cornea, the face is deadly pale, the skin clammy, and the whole appearance of the child suggests an apprehension that it is dying. Yet if a little nourishment or some slight stimulant be given, or even if a little time be allowed to elapse, the heart will recover its tone, the little patient will revive, and may, perhaps, finally appear to be even in an improved condition." (pp. 189-90.)

Dr. Griffin has found that an overdose of opium will produce the same effect as an underdose; that is to say, a drowsy restlessness; but sound sleep would follow during the next night. He has found also, that "in administering opium, or indeed any narcotic whatsoever, for the relief of pain purely nervous, or connected with some condition of mere irritation and periodical in its occurrence, it will be found that a third part of the dose which may be found necessary to give relief in the paroxysm, will prevent it altogether if given in the interval, a little before its accession is expected. That, in fact, a moderate dose given in the interval will sometimes prevent the accession of the fit, when no quantity however great can control it, after it has once set in." (p. 193.)

We have already noticed Dr. W. Griffin's recommendation of opium as an antiphlogistic remedy. It is useful in inflammation of the mucous as well as the serous membranes; but in all cases full doses are the best. We would however put in a caveat: let the practitioner beware how he gives full doses to delicate invalids.

"The mischiefs arising from the administration of opium in inflammation may always be fairly attributed to the practice of under-dosing. In inflammations of serous membranes, which would be rapidly arrested by large doses, small ones incite and tend to increase the mischief,—in those of mucous membranes, small opiates do still more injury apparently, because while they are inefficient to diminish or arrest the inflammation they check or arrest the diarrhœa, which, however exhausting, is a relief so long as the inflammatory action continues unabated. A large opiate suppresses the inflammatory action and the diarrhœa at the same time; and, as a general principle, I think it will be found that the efficacy of its action

will be in proportion to the degree of debility present. This would obviously suggest the propriety of abstracting blood in those cases where, from the violence of the arterial action, the antiphlogistic power of opium may possibly be resisted, or where on exhibiting it no relief has been attained. For my own part, in the treatment of most intestinal inflammations, whether of serous or mucous surfaces, I would confide more in the influence of blood-letting, large opiates, and warm poultices to the abdomen, than in all the remedies included in our materia medica." (p. 195-6)

In a deplorable case of dysentery, Dr. Griffin, following a suggestion of Dr. Gregory's, gave three grains of opium with five of ipecacuanha at a dose, and directed it to be repeated every hour until some relief was experienced, and then continued every second hour until all pain and discharge ceased. "The effect was magical! Before he had the third dose taken my patient was asleep, and enjoyed more perfect and enduring relief than he had experienced for weeks."

"Of all the wonderful influences however exerted by opium, that by which it sustains the powers of life when sinking from hemorrhage, and arrests the flow of blood, is the most extraordinary. When after severe uterine hemorrhage the countenance is sunk, the eye hollow and glassy, the lips blanched, the skin cold, and the whole person corpse-like, when the pulse is almost gone at the wrist, when the beat even of the heart is scarcely perceptible, and stimulants, even brandy or rectified spirits, are either vomited or uninfluential, there remains yet one remedy capable of restoring the patient to life, and that is opium. I believe its power of saving life in these circumstances depends principally on its specific property of producing congestion in the brain. That amount of congestion by which it occasions apoplexy when given in large doses to persons in health, seems only sufficient to sustain the natural and necessary tension of the cerebral vessels in those who are dying of hemorrhage. Persons die in cases of hemorrhage, not so much from mere debility of the heart's action, as from the loss of nervous power in the brain consequent to it, and hence a fainting from which they are never awakened. The opium in such cases not only stimulates the heart's action, but restores a sufficient degree of tension in the vessels of the brain to prevent faintness, and by the judicious repetition of the remedy, life is preserved on the very borders of death. There are no instances in which opium can be given so freely or so fearlessly as in these. When the danger is imminent, five grains may be given at the first dose, and two or three every hour or half hour afterwards, until the pulse becomes distinct, the breathing easier, and the tossing or flinging about in the bed is allayed. It is hardly necessary to observe, that in such cases, in conjunction with the use of opium, the administration of warm wine and brandy, (however inefficient alone,) and the application of heat to the extremities, are highly useful, if not absolutely essential." (pp. 201-2.)

This is a very valuable paper, and fraught with truths of the greatest practical importance.

Problem XI. "*What principles should regulate the treatment of hemoptysis?*" This is a very useful paper, inasmuch as it is opposed to that system of routinism which subjects all cases of hemoptysis to an antiphlogistic treatment, with strict abstinence. Dr. W. Griffin relates some instances in which a contrary method was eminently successful. We could add corroborate facts from our own experience; and we are strongly convinced that a thorough revisal of the pathology of this affection is absolutely necessary to a discriminating and safe treatment. Dr. Griffin thinks that the starving method increases the tendency to hemorrhage, by rendering the vascular system more lax and irritable. The hypothesis is

plausible; the fact that starvation increases the hemorrhagic tendency is certain. Another bad result of protracted low regimen and frequent depletion is referred to by Dr. Griffin,—the danger of inducing phthisis; and he thinks the need of such treatment is the less when an emetic of ipecacuanha, and the continued administration of it every hour, will subdue a frightful hemoptysis.

PROBLEM XII. "*How should acute rheumatism be treated,* so as to effect a cure in the shortest space of time, with the least pain or other symptoms likely to protract or interrupt the cure, with the least probability of inflammation of a vital organ, and with the least after ill effects, whether debility, salivation, or chronic disease, and with the least liability to relapse?" Large and repeated bleedings, and the sweating and stimulant plans are going fast out of repute; colchicum is less certain, more painful, and sometimes attended with more serious consequences than other remedial agents.

"Emetic tartar sometimes acts like a charm," Dr. Griffin observes, "but when the practitioner tests the remedy more extensively, he finds, not only it is often attended with inconvenience, and if injudiciously pushed, with hazard, but that its failures are considerable in point of number. With Lepelletier they amounted to half those in which he employed it, and the success was little better even when accompanied by bleedings. In the Limerick Infirmary I believe the failures were more than two-thirds."

Finally, Dr. Corrigan's and Dr. Hope's plans are pronounced the best. The former administers one or two grains of opium every second or third hour. The medicine should be always increased in dose, both as to frequency and quantity, until the patient feels decided relief; and should be then kept up at that dose until the complaint is steadily declining. The first indication that tells the practitioner he has reached the proper dose is the statement of the patient, who in reply to an inquiry, as to how he has past the night, probably says that he has not slept, but that he is free from pain and feels comfortable. This effect having been attained, the opium may then be continued in repetitions of the same dose as to frequency and quantity. Dr. Hope gave from five to ten grains of calomel every night with from a half to two grains of opium, and in the morning a smart pugative. Three times a day a saline draught is also given, with ten to twenty minims of colchicum wine and five grains of Dover's powder.

The last essay in the volume before us is entitled, "*Observations on the application of mathematics to the science of medicine,*" and is written by the brothers conjointly. The object of the paper is to recommend the numerical method. Our views on this question have been already very fully stated.

We have now to sum up, and give our critical opinion on the essays before us. We judge then, firstly, that the writers are well-informed and sensible men, and careful, industrious, and observant practitioners; secondly, that they are better practitioners than physiologists; and thirdly, that their pathological theories are (consequently) marked more by ingenuity than by depth or comprehensiveness. To the practising physician and surgeon, however, the book will be found very useful, and will amply repay a careful perusal: we therefore strongly recommend it to all our readers.

The volume is full of typographical defects—partly, no doubt, the consequence of its provincial origin; and it contains some odd expressions, as for instance, "and directions were given if he *got any weakness* during the night to give him wine and water." We are sure that not long ago the very respectable authors must have regretted, as much as we did, to read a long and prominent notice of their book in a London morning paper. This must have been the work of some over-zealous and ill-judging friend, who did not consider that the legitimate road to professional reputation is through the *professional* public alone. A writer must be judged by his peers, if he would have a true judgment, and not appeal to an inadequate tribunal. The merits of this publication indeed rendered such an appeal quite unnecessary; its useful and practical tendency will secure it a favorable notice by the profession.

ART. VII.

Chemistry, Meteorology, and the Function of Digestion, considered with Reference to Natural Theology. By WILLIAM PROUT, M.D. F.R.S., Fellow of the Royal College of Physicians. *London*, 1845. Third Edition, 8vo, pp. 515.

THE high merit of this work, and the respect we entertain for its learned author, induce us to make a departure in its favour from our usual practice; and to avail ourselves of the opportunity afforded by the appearance of this new edition, to bring it under the special notice of our readers, although in its original form it made its appearance eleven years ago,—one year before the commencement of our critical labours. Coming out as one of the series of Bridgewater Treatises, the origin and plan of which are doubtless familiar to all our readers, Dr. Prout's work enjoyed at the time an extensive popularity, and came to a second edition in the course of a few months. We remember hearing the opinion expressed at the time by a competent judge, that this popularity would be more durable than that of most of the other works of the series,—Dr. Prout being in many respects in advance of the general state of knowledge on his science, whilst some of the other authors were scarcely on a level with it. We rejoice to see this opinion borne out by the appearance of a third edition at this distant date, indicating as it does the continued, though perhaps not very rapid, sale of the work. A considerable amount of new matter has been introduced, by an enlargement of the page, without any augmentation in the bulk of the volume; and this is for the most part of much value. We think that Dr. Prout has done wisely in restricting his additions within moderate limits, and in not entering upon the field of controversy; and the modest apology which he makes in his Preface must disarm criticism in regard to what may be deemed errors of omission. "The author is still conscious of many imperfections, arising in no small degree from his professional avocations. The varied and difficult nature of the points to be investigated required more undivided attention than he has been able to bestow."

The work commences with a chapter entitled, "Of the leading Argument of Natural Theology; that Design, or the Adaptation of Means to

an End, exists in Nature." From this we shall extract a passage, which presents an admirable summary of the fundamental points of the argument:

"Our belief, then, in the agency of an intelligent Creator, is founded—

"On our recognition of the identity of the effects produced in external nature, with effects produced by ourselves; from which identity of effect we immediately infer identity of purpose,—the *existence of design* without reference to a designer.

"On our consciousness that the purpose effected by us proceeded from ourselves, the designers; whence we conclude that the design manifested in external nature must have had a like origin,—that the manifestation of design is demonstrative of the existence of a designer.

"On the pervading character of the design shown among the objects of nature, in which design man recognizes the *creation* of the objects designed, and is thus led to infer the existence of a *Creator*. Now the faculty of reason, which enables man to recognize the Creator of the objects around him, enables him to recognize in that Creator the *Creator of himself and of his faculties*. In reasoning, therefore, from his own acts, to those of the Creator of the universe, though conscious that he is reasoning from the finite to the Infinite, from weakness to Almighty Power; yet, when he reflects from whom he has derived his faculty of reason, man feels assured that his own reasoning, when it coincides with the reasoning evinced by his Creator, *can be no other than the same*. Nor, founded as that assurance is on the constitution of the human mind, can that assurance be impugned, without impugning Him by whom the human mind has been so constituted.

"Thus the argument of design, though not based on necessity, in the strict sense of the term, is of a validity equal to that of our knowledge of the existence of, and of our connexion with, an external world." (p. 7.)

In the second chapter are considered "The rank of Chemistry as a Science; and the Argument of Design with reference to Chemistry." The argument, as treated by Paley, is thus summed up by Dr. Prout:

"Chemistry is a department of knowledge founded solely on experience, for the phenomena of which we can assign no reason. But though the intimate nature of chemical changes be unknown to us, we see them manifestly directed to certain ends; hence, as things directed to certain ends, where the whole of the intermediate phenomena can be traced and understood, always imply design; we naturally infer design in chemical changes obviously so directed, even although we may not be able to understand their intimate nature." (p. 17.)

We are much disposed to agree with Dr. Prout, however, in thinking that the very circumstance of the mysterious nature of the operations of chemistry, gives them an impressive interest, which is wanting in those classes of phenomena which are better understood. We witness the utmost order and regularity, as is evidenced in the "Law of definite proportions;" but we cannot account either for this general uniformity, or for the limitations in the combining powers of different substances, for want of some more intimate knowledge of the forces that are concerned, and of their mode of operation. As to the bearing of this deficiency on the argument from design, Dr. Prout remarks as follows:

"Obvious mechanism, though well suited to display the intelligence and design of the Contriver, is not always so well adapted for arresting the attention of the observer, its very obviousness in some measure depriving it of its interest. But when we see the same Contriver, besides the most beautiful and complicated

mechanism, employ other means utterly above *our* comprehension, though evidently most familiar to *Him*, the employment of these means is not only calculated to arrest our attention more forcibly, but at the same time to impress us with more exalted notions of His wisdom and power." (p. 18.)

It is far from our intention to go through the work chapter by chapter, but we wish to offer a few remarks on the general nature of the argument from design, for which this will be the most fitting place, as they naturally connect themselves with the closing passages of this section. The first of these has reference to the possible denial of the reality of the changes termed chemical:

"Perhaps one of the most striking arguments in favour of the reality of chemical changes may be deduced from the subserviency to them of those mechanical contrivances and operations everywhere existing in organized beings. At least half the mechanism in a living animal is subservient to the chemical changes constantly going on in it, and necessary to its existence. Take, for instance, the circulation of the blood: what a complicated apparatus is here employed for the simple purpose of exposing the blood to the action of the air in the lungs, in order that it may there undergo some chemical change. Now, surely no one can reasonably doubt that this chemical change is as much a reality as the mechanism by which the chemical change has been accomplished; and if one chemical change be admitted to be a reality, must not all the others be equally real?" (p. 19.)

There is a class of objectors to the argument from design, who affect to believe that all the beautiful arrangements and adaptations which we witness in creation arise from what they term "the necessary and eternal laws of nature." Such persons are either pantheists, making gods of these "laws of nature;" or, if they recognize a Creator at all, it is by some other mental process. They are thus addressed by Dr. Prout:

"If there be any one who denies the existence of design, and sees nothing in all the more obvious arrangements and order around him but the necessary result of what he chooses to denominate 'the laws of nature,' let him calmly and deliberately consider the facts brought forward in the following pages; and if he can witness unconvinced all the numerous instances of prospective arrangement clearly made with reference to things not yet in existence; all the beautiful adjustments and adaptations of noxious and conflicting elements, most wonderfully conspiring together for good; and lastly, the subversion of even his favourite 'laws of nature' themselves, when a particular purpose requires it: if, we say, he can witness all these things, and still remain incredulous of the evidences of design, his mind must be most singularly constituted, and apparently beyond the reach of conviction." (p. 20.)

The fundamental error of such objectors is their assumption that those *human* expressions of a certain order and regularity in the phenomena of the universe, to which the term "law of nature" is commonly given, are the necessary and inherent properties of matter. This error we have exposed on a recent occasion, (vol. XIX. pp. 160-61.) We shall add to our quotations on this subject one taken from the recapitulation of the argument, as applied to the phenomena of organic chemistry, with which the work concludes:

"The adaptations of mechanical arrangements in the structure of organized beings to the pre-existing chemical properties of matter, afford also an evidence of design, not less impressive than unequivocal. The most determined sceptic

cannot assert that there is any *necessary* relation, or indeed any relation whatever, between the mechanical arrangements and the chemical properties to which they administer. There is no reason why the chemical changes of organization should result from the mechanical arrangements by which they are accomplished; neither is there the slightest reason why the mechanical arrangements in the formation of organized beings should lead to the chemical changes of which they are the instruments. From what cause, then, arose the association of the chemical changes with the mechanical arrangements? How were the chemical operations of digestion and respiration brought into union with the mechanical apparatus of digestion, and with the circulating system? The cöexistence of things so entirely dissimilar, and having no kind of mutual relation, can be explained only on the supposition that *a will exists somewhere*, and also *a power to execute that will*. The existence is thus unavoidably acknowleged of a Being who, knowing every pre-existing chemical property of matter, and willing to direct these chemical properties to a specific object, has contrived for that purpose an apparatus admirably fitted to attain His object. Such is the explanation—the only possible explanation, of the subservience of mechanism to chemistry, in the processes of organic life. And what is this explanation but our *argument of design*, in terms that seem absolutely irresistible?" (p. 488.)

There is perhaps no more remarkable instance to be found in the world of organization, of that *general adaptation* which seems to us to afford by far the most satisfactory foundation for theological arguments, than is presented in the concurrence here adverted to by Dr. Prout. Take, for example, the process of respiration as performed in the higher animals; and enumerate the number of operations all tending towards the same purpose, all interwoven as it were in one system, and yet all essentially independent. For the simple purpose of bringing the blood into relation with the atmosphere, and of permitting the chemical changes which necessarily result from that communication, what a multitude of mechanical provisions are required. First, an organ of most remarkable and elaborate construction, within which the blood and the air may act upon each other; this organ consisting of innumerable chambers of extreme minuteness, yet these so connected by a series of branching passages with the main outlet, that a continual interchange of their gaseous contents may be effected; whilst the walls of these chambers are covered with a capillary network, closer than that which is to be found in any other part of the body, so as almost to expose a continuous film of blood to the air within, and so communicating with afferent and efferent trunks, that a continuous supply of unaerated blood may be conveyed to every part, whilst that which has undergone the required change shall be immediately removed. And if such be the provisions required in the lungs themselves, how vastly are they surpassed in variety and complexity by those which keep the lungs in play; alternately filling their cavities with the pure air inhaled from the atmosphere, and withdrawing their contents when tainted by the poisonous exhalations of the blood, and maintaining through their capillaries a continuous flow of blood, whose rate of movement is so precisely adjusted as to permit the vivifying and restoring influence of the air to be fully exerted, and yet with the least possible delay! What an almost countless multitude of provisions and operations are destined to this one set of purposes! What an elaborate adaptation of the bony parietes of the chest to the formation of a cavity, which shall be perfectly closed and yet capable of constantly alternating dilatation and contraction

by forces acting upon its exterior,—which shall afford adequate protection to the delicate organs within, at the same time that it serves for the attachment and support of the anterior limbs,—and which shall yet be so flexible as to accommodate itself to every movement of the trunk—so light that its weight is never felt! What an apparatus of muscles, each consisting of countless fasciculi of fibres, every one having its appropriate action, neither repeating another; some being in constant and regular operation; whilst others are reserved for those more energetic movements, which are sometimes required by peculiar conditions of the system, or which serve to compensate in a degree for the occasional imperfect action of other parts of the apparatus! How wonderful is the connexion of these various muscles with the nervous system, which keeps them in constant alternating movement without any effort of the will, even without a thought, during sleep, in utter unconsciousness,—which regulates their actions as to frequency and intensity, so as to supply a quantity of air exactly proportioned to the amount of blood to be aerated, and the degree of purification which it requires,—and which is yet placed under such a degree of subservience to the will, as to allow their movements to be adapted to the needs of the vocal apparatus, and thus to contribute to that function which, more than all others, binds together the different individuals of a race by mutual intercommunication! And how inefficient would be all this mechanism, how useless all its well-devised operations, if it were not made to concur with another wonderful organ; by whose ceaseless pulsations the dark tide of blood returning impure from the body, is propelled to the lungs; whose flood-gates open wide to receive it again, as it returns purified and renovated by exposure to the atmosphere; and whose vigorous contractions force the vivifying current into the remotest corners of the body!

We have sketched but the broad outline of this argument, as suggested by our author's indication; it would occupy us too long to fill it up with anything like minuteness of detail; and we must leave it to the sagacity of our readers to trace out the various subsidiary provisions, which are requisite for the due operation of the machine, whose chief parts and principal actions we have thus passed under review. Now to our own minds, the existence of such a provision, or series of provisions, in a single animal or group of animals,—however elaborate the design might appear, and however perfect might be its operation,—would have little or no theological importance. It *might* be regarded as an instance of successful construction, without imputing *design* or *intentional adaptation of means to ends.* The inference that would be founded upon it, by a person on the outlook for examples of such design, might be met by the objection that we see only *one* out of *many* possible combinations of organs; that supposing these to have been made without any design, according to the ordinary laws of chances *some one* might prove to be fitted for a continued existence on the surface of the globe whilst all the rest perished long since; and that consequently such an isolated example *proved* nothing whatever as to the capacity of the original Designer. Such an objection, it seems to us, could not be easily rebutted; for every one is familiar with cases in which pure *accident* has accomplished a purpose better than any *design*. Thus, when a painter had been labouring unsuccessfully for many hours to produce

the appearance of foam at a horse's mouth, and in a fit of vexation and despair threw his sponge, charged with a variety of colours, against the picture, the very effect was produced which he had been in vain exhausting his powers to attain, and this by the merest combination of chances, which might never occur again. The sponge *happened* to be charged with colours, in the precise amounts and proportions requisite to produce the effect; and it *happened* to be flung by the painter in such a manner as to alight on the precise spot where these colours should be deposited, and to deposit just the amount of colour which gave the desired appearance. It would be scarcely possible for any one, struck with the exactness of the representation, but ignorant of the means by which it had been brought about, to avoid the conclusion that the most elaborate design had been exercised in producing the effect; especially when he considered that this was but a small portion of a work, on which he *knew* that a designing mind had been employed. And yet he would be completely mistaken. The same liability to error, as it appears to us, pervades every theological argument erected upon *single* adaptations of means to ends in the organized creation; and this more especially, since we have no right to assume that a designing mind has been exerted about it at all,—that such a mind has been at work upon any part, being the very thing to be proved.

Now we will suppose that our painter, struck with the result of this casualty, had adopted the method in other cases, and had executed a number of other pictures, in all of which (after many trials and failures, it may be) the same effect was successfully repeated. It is obvious that any person who witnessed this result would be much more entitled to suppose it *intentional*, than he would be from the single case first cited. And further, we will suppose that, instead of repeating precisely the *same* effect, the painter so varied the colours in his sponge and the manner in which he threw it, as to produce variations in the effect, which should be adapted to represent correctly the different appearances that would be appropriate to a *variety* of animals; it is obvious that an observer who knew nothing of the method, and only witnessed the result, would be still more justified in regarding it as proceeding from a *designed* adaptation of means to ends, and would indeed feel it almost impossible to resist the conclusion that it was so, even when informed of the *casual* nature of the first or single adaptation.

Just so, as it seems to us, must the most complete sceptic, if not so prejudiced as to be *determined* to resist conviction, yield to the cumulative argument from design, whilst he may question the supposed proof drawn from isolated cases. Thus, to revert to the subject on which we just now touched; whilst the existence of the most perfect adaptation between the mechanical and chemical operations of a single organized body, in the function of respiration, *proves* nothing in regard to the intention or design of this adaptation, the evidence accumulates when we observe the same adaptation existing, the same plan followed out, in other groups of living beings; and it attains its greatest force, when we contemplate the variation of the methods which are employed in different groups to compass the same end, these variations being in every instance adapted most exactly to the general wants of the system. Thus, whilst the description we have given of the respiratory apparatus applies chiefly to mammals, we

find that apparatus extended in birds through the entire system, and so arranged that, whilst really constructed upon a lower type, it shall impart a greater amount of aeration to the blood, and at the same time confer buoyancy upon the body. In reptiles, on the other hand, a still further degradation of type,—shown in the small amount of subdivision of the lungs,—is accompanied by a very low amount of aeration; which is conformable to the feeble circulation, sluggish habits, and low temperature of that group. Tracing the air-breathing apparatus, again, through the articulated series, we find a gradual transition from the concentrated lung-like sacculi of the spider tribe,—whose lining membrane is folded like the leaves of a book, so as to expose a large amount of surface in a small space,—to the extended tracheal system of insects, penetrating into every part of the body, supplying the deficiency of circulatory power by the universal diffusion of the air that it conveys, and ministering to an amount of aeration which, like the muscular energy of these animals, surpasses in proportional degree that of all other tribes; and thence, through the more limited tracheal apparatus of the myriapods, to the simple air-sacs of the terrestrial worms, which present the lowest form of organs for atmospheric respiration. We might follow out the leading forms of the apparatus for aquatic respiration,—commencing with fishes, descending through the mollusca and aquatic articulata to the radiated classes,—in the same manner; and might show in detail the beautiful uniformity of the general plan, combined with an endless variety of modifications, adapted to the general habits and conditions of the animal. And we might then revert to geology for proof that we do not find these adaptations, because the animals that present them *happen* to be fitted to live and to continue their race in the circumstances in which we find them; for were it so, we ought to find for one such casual instance of adaptation an almost infinite number of failures: whilst the fact is, that the remains of extinct animals which the progress of research brings to light, all exhibit the same plan, with modifications adapted to the circumstances of their existence; and that, so far from these having come into existence by anything like that "fortuitous concourse of atoms" which was once a favorite method of accounting for the existing state of things, and having then immediately perished because incapable of continuing their existence,—we have ample evidence that they lived and died, eat, drank, and propagated their kind, through countless successions of ages, until some change in the condition of their dwelling-place rendered it no longer fit for their habitation, and subjected them to the general law of extinction, which had already cleared the way, by its operation on previous races, for *their* appearance on the scene.

This is an outline of the course of argument which, as it seems to us, is required to build up the science of natural theology upon its true basis. We would not discard the ordinary inferences drawn from isolated cases, for they are well fitted to make an impression on minds, which cannot appreciate the grander and more general views we have indicated, and which do not perceive the slenderness of the support on which they rest; and they aid, by their accumulation, in forming the extended and secure foundation which is required for solidity. It must be obvious, we think, that when phenomena of any kind are viewed as the necessary results of

general laws, or, to speak in language which we deem more correct, as consistent parts of a uniform plan,—their mutual harmony and adaptation becomes much more striking, than when it is regarded as expressly contrived in each individual case, in accordance with the ends to be attained, as they present themselves. For the one act implies a comprehensive acquaintance with *all* the results of the plan laid down, and a mutual adaptation of the different parts of the plan, in the first instance; whilst from the other might be inferred a limited conception of those results, requiring to be corrected when the machine should be put in motion.

Now, we cannot but think that those who planned and executed the Bridgewater Treatises have in general erred in dwelling too much upon the *particular*, and too little upon the *general;* in accumulating individual instances of design, and in directing but little attention in comparison towards the general plan, of which those instances are but isolated examples. Thus we have a whole treatise devoted to the Mechanism of the Hand, by an anatomist who, with all his accomplishments, was unable to comprehend the philosophy of his science, and who went out of his way to sneer at those who are engaged in its development. So again we have the subject of Animal and Vegetable Physiology separated from Geology; thereby putting it out of the power of either writer to give a comprehensive view of the organized creation, and of what is known of the plan on which it has been developed; since in such a view, the *past* and the *present* must be equally embraced.

Although the subjects of Dr. Prout's Treatise appear to be somewhat heterogeneous, yet they lead not unnaturally into each other; and we are disposed to think that the plan of treatment is on the whole more adapted to its purpose, than is that of most of the other Treatises. After the introductory chapters, to which we have already referred, Dr. Prout proceeds to those most general and fundamental ideas on which the science of chemistry rests; and gives a luminous view of the present state of our knowledge of the "molecular actions of matter," connected by an ingenious hypothesis of his own. The facts, however, are kept sufficiently clear from the hypothesis, for any intelligent reader to separate them with facility; and although we do not find that hypothesis altogether satisfactory, we are disposed to think that the author has done wisely in introducing it, as showing the mode in which general principles may be applied. No argument illustrative of design is founded upon the supposed molecular arrangements he has given; his purpose being rather to show that the *invisible* operations of chemistry are at least as wonderful as the *visible* operations of mechanism, and are, like them, capable of being reduced under simple and determinate principles. "The foregoing attempt," he says in conclusion, "to connect physical molecular phenomena is acknowledged to be very imperfect. Enough, however, has been said to draw the attention of those who are interested in the subject. Another age will follow out these or some nearly allied principles; and the physical phenomena of the microcosm will thus become as much a matter of mathematical inquiry and proof, as the physical phenomena of macrocosm or universe now are." (p. 163.) This division of the work closes with an admirable chapter, comprising general reflections and arguments founded on the preceding details; from this we shall select some passages in

illustration. After noticing, as an example of the adjustment between the conditions of the different materials of our globe, the mutual adaptation of the properties of water and the temperature of the earth, Dr. Prout thus continues:

"If we do not admit of this adjustment, we must suppose that the whole has been the result of chance, or of some other unintelligent principle; and if water had been the only case in which such adaptations were apparent, the supposition of chance might perhaps be received, at least it would be difficult to prove the contrary. But when we see similar happy adjustments in every object around us,—in the different elements of the air we breathe, in the soil we tread upon, in all the varieties of rocks composing the solid crust of our globe; not one of which could have been more happily contrived for the purposes they fulfil, nor indeed be scarcely conceived to exist otherwise than as they are, without destruction to the whole of the present arrangements,—when we see all these things and duly reflect on them, it becomes absolutely impossible to suppose that so much happy adjustment, so much apparent intelligence, so much, in short, of what the veriest sceptic, under other circumstances, would have allowed to be evidences of design, can be evidences of anything else than design; or have resulted from any unintelligent cause whatever. Hence we are irresistibly impelled to the only rational conclusions the premises appear to admit of, viz., that all these happy adjustments and adaptations which we see in nature, are really and truly what they appear to be,—so many evidences of design; and, consequently, that the whole have sprung from the will of an intelligent and omnipotent Creator." (p. 167.)

As the mutual balancing of the perturbations of the solar system, producing the stability or equilibrium of the whole, affords the most comprehensive foundation for the argument from design in the arrangement of the planetary bodies, so do we think that Dr. Prout appeals to what the reflecting mind must admit to be the most comprehensive form of his portion of the argument, when he rests it on the equilibrium of the several distinct agencies of nature:

"Amidst all the endless diversity of property, and all the changes constantly going on in the world around us, we cannot avoid being struck with the general tendency of the whole, to a *state of repose* or *equilibrium*. Moreover, this tendency to equilibrium is not confined to the ponderable elements, but prevails also, in the same remarkable degree, among the imponderable agencies, heat and light, which, as we have seen, cannot be anywhere long retained in a state of excess, on account of their natural disposition to acquire a certain state of equilibrium, depending generally upon the place of the earth in the solar system. Now, the formation of this state of equilibrium and its preservation, may be considered as the results of those wonderful adjustments among the qualities and quantities of bodies above alluded to; the qualities being such as to neutralize each other's activity, while the quantities are so apportioned as to leave one or two only predominant. It is further to be observed that this state of equilibrium is not absolutely *fixed;* such an unyielding condition would be not less incompatible with the present order of things, than a condition of unlimited change. All things therefore are so adjusted, that slight deviations or oscillations about the neutral point of rest or equilibrium take place, and are even necessary, as the world is at present constituted; though these changes are bounded within very narrow limits, and greater deviations would instantly prove fatal to the whole." (p. 174.)

"Throughout nature, the exigences and incongruities necessarily arising from the arrangements we have been considering, have given occasion for the display of the most astonishing wisdom and power. And instead of that jarring and clashing, which might have been expected from so many conflicting elements, the qualities and quantities of those elements have, upon the whole, been so

wonderfully adjusted to each other, that they neutralize and balance each other's evils; and the general result has been that all have finally settled down together, into that harmonious state of equilibrium before alluded to, so admirably adapted for the existence of organic life." (p. 180.)

We have said that we consider the subject of meteorology as not unnaturally connecting itself with that of chemistry, in such a general survey as that here taken by Dr. Prout; and the transition from one to the other becomes still more obvious, when it is made through the views we have just quoted. For whilst it is the business of the geologist to trace out the history of these various alternating periods of disturbance and of equilibrium or quiescence, which have contributed to produce the present condition of the globe,—and to demonstrate that to these convulsions and changes we owe all that boundless variety of sea and land, of mountain and plain, of hill and valley; all that endless admixture of rocks, of strata, and of soils, so essential to the existence of the present order of things; without which the world would have been a mass of crystals, or one dreary monotonous void, altogether unfit for the support of the present races of organized beings, and precluding the existence of man,—it belongs to the meteorologist more especially to consider the globe in its *present* condition of equilibrium, and the means by which this state of equilibrium is maintained; in particular, to point out the influences of heat, light, and electricity, and of the energies allied to them; to study the laws of the distribution and change of these wonderful agents, in the production of climate; to trace, in short, the effects of heat, light, and electricity, on the earth, the ocean, and the atmosphere; and all the infinite variety of phenomena dependent upon them. This is the method in which Dr. Prout has considered the subject; and we can recommend the chapters on meteorology as containing an admirable summary of those general operations of chemical agents upon the surface of our globe, which are concerned in producing the endless varieties of climate, and in thereby adapting it to the residence of almost innumerable tribes of living beings; the existence of a large number of which would be incompatible alike with any greater uniformity, and with any wider diversity, in its conditions.

The concluding book is not so much a limited account of the function of digestion, which its title seems to imply, as a general sketch of the chemistry of organization, and of the various processes by which alimentary substances are assimilated to, and become component parts of, a living body. At the time of the first publication of this Treatise, this sketch was decidedly in advance of the general state of knowledge on the subject; and many of the views contained in it were as novel as they now are familiar. The author may justly claim the credit of having contributed largely to the direction of inquiry towards these objects; and when the labours of Dumas and Liebig and their followers, are quoted as having built up the present fabric of organic chemistry, the foundation which had been laid by our countryman should not be forgotten. And it would be well if his clearly-drawn distinctions between what chemistry *may*, and what it *cannot*, effect for physiology, were more constantly kept in view. To those chemists who imagine that the employment of the apparatus for ultimate analysis is to revolutionize the science of physiology, we would

give it as a problem to be solved,—what light can be thrown by chemistry upon that simplest and most fundamental of all vital operations, the growth of a *cell* from a germ, its gradual development, its production of the germs of new cells in its interior, its operation upon the surrounding elements, so as to combine them into the materials for its own fabric and for the germs of its successors, whilst at the same time it fills its cavity with compounds of a totally new and distinct character, and finally its death, by which its contents are set free,—the cell-germs to go through the same process in their turn, and the other substances to serve, it may be, as materials for some purpose, to all appearance totally unconnected, in the economy of nature? The chemist may ascertain the number of atoms of carbon, hydrogen, oxygen, and nitrogen, which are combined in the substance of the cell-wall, in the reproductive germs, and in the contained products of cell-action. He may ascertain the sources of these in the carbonic acid, the ammonia, and the moisture, which the cell derives from the atmosphere. And when he has done all this, what more can he effect? Or how does this information give any clue to the mysterious properties of the primordial cell-germ,—properties by which it not only operates itself, but gives rise to an unlimited extent of similar operation; properties which are so far peculiar to itself, that no combinations which the chemist can effect can ever exhibit the like, and which are derivable only from a pre-existent being of the same character; properties which are at least as far removed from those which give rise to the physical and chemical operations of matter, as those are from each other? Surely there can be no reasonable expectation that physics and chemistry can ever give any account of these actions, which are rightly denominated vital, as being peculiar to *living* bodies; and the utmost that can be expected in regard to them is that, when the laws of molecular agency are better understood, they may be included with others in some higher and more general expression common to all.

Having said so much in recommendation of Dr. Prout's Treatise, we feel called on to express our complete discordance with his views in one important particular; and it is rather singular that the full development of these views, in a form which almost gives them (to our minds at least) the character of a *reductio ad absurdum*, is to be found only in the present edition, a mere sketch of them being all that was contained in the preceding. In the third chapter, entitled "Of Matter and of Physical Agents in general, and of their mutual relations," we meet with the following passages:

"That matter and the power of moving matter exist, both within our own corporeal frames as well as in the world around us, we cannot doubt, without doubting our own existence. By evidence equally irresistible we are impelled to the belief that matter, and power of moving matter, though everywhere conjointly existent and exerted, must be essentially different. In other words, that matter does not move in virtue of any property necessarily belonging to it as matter, but that matter was first created and set in motion by the Deity when he formed the universe; and that the motion then originally imparted to it is still retained, and will continue to be retained by matter, 'till time shall be no more.'" (p. 22.)

"Of the positive or abstract nature of matter and of power, any more than of the positive and abstract nature of the Deity himself, we know absolutely nothing. . . . . Taking for granted that matter and power exist, and are different, we assume their relationship to each other to be that of *passion* and *agency*. According to this assumption, matter is entirely passive; while the forces by which it is

moved are, as it were, personified, i. e., supposed to be wielded by subordinate and delegated existences, capable of exerting physical force according to certain laws, and within certain limits, prescribed by the Deity; of whose will, therefore, they are the immediate executors. In other words, to use the language of Paley, this assumption regards physical agency as exerted by 'subordinate beings, to whom the Deity, after having laid down certain laws and furnished certain materials, has delegated the task of drawing forth a creation.'" (p. 23.)

Dr. Prout is perfectly correct in designating this notion as an *assumption;* and its baseless character becomes so evident upon the slightest consideration, that we must confess our astonishment that it should have been adopted and sustained by a mind like our author's. We believe that it is now universally admitted by the profoundest logicians and physical philosophers of our day, that *we can form no idea of matter except through its properties;* and that the *forces* or *powers*, by which the various material phenomena are produced, are nothing else than *those properties in action.* Thus our most simple and general idea of matter depends upon its *resistance* to our touch; and that resistance is due to nothing else than the operation of the mutual attraction, subsisting among the particles of the substance touched,—and varying in its relative amount in substances of different kinds, or in the same substance according as it exists in the solid, fluid, or gaseous state. Now this mutual attraction of its particles, more or less balanced by a mutual repulsion, is essential to our very idea of matter; so completely so, in fact, that it has been argued by one of the profoundest philosophers of our day, with a degree of vigour which must impress if it does not convince, that we gain nothing in our explanation of the molecular actions of matter, by regarding the centres of the spheres of attraction and repulsion as being occupied by material particles or atoms; but that if we regard these centres as mathematical points, we shall be at least as well able to explain those phenomena of attraction and repulsion which give rise to our fundamental notions of matter.* According to this view, then, *matter* is nothing but *force.* We do not profess to adopt it; but we unhesitatingly affirm that we can form no idea of matter but by its forces, and that the assumption that *matter* and *force* are distinct, and are respectively characterized by *passion* and *agency,* is not only unnecessary but highly illogical. The idea, too, of "delegated existences," appears to us to involve so many difficulties, as to be completely untenable; and we cannot but regard it as altogether a fiction of the imagination. If all matter, with its properties, originally came into existence at the Creator's *fiat,* why should we suppose any "delegated existence" to have the charge of regulating its subsequent operations, which are nothing else than the original properties in action, or the *continued expressions* of that same Will which at first called the universe into being? To suppose a "delegated existence" necessary to superintend particular classes of operations, is, in our apprehension, to bring down the attributes of the Deity to a human standard, by limiting the exercise of Omniscience and Omnipotence to the general superintendence, and committing the details to subordinates.

But it is when this doctrine is applied to the organized creation, that its objectionable character seems to us most palpable; for the "delegated existences," here termed "organic agents," are regarded by Dr. Prout as possessing the rank of "*independent intelligent agents,* superior

* See Prof. Faraday on Electric Conduction and the Nature of Matter, in Taylor's Philosophical Magazine, 1844.

to, and possessing the power of directing and controlling, the common forces of matter, so as to cause these forces to associate material elements into the organized, instead of the crystallized condition. (p. 399.) We cannot but think that he is quite mistaken in affirming that this hypothesis, "variously modified, is not only the most ancient, but with some reserve and under different disguises, is that which is most generally adopted by physiologists to the present time." For although the notion of some kind of existence has been very prevalent under various names—the last, and the one in general use, being that of "vital principle"—we know of few if any philosophers, who have been bold enough to affirm that the "organic agent" is "a conscious being possessing knowledge, will, and power." (p. 399.) In fact Dr. Prout himself in his former editions, and in his Gulstonian Lectures, characterized the "organic agent" as "an ultimate principle, endowed by the Creator with *a faculty little short of intelligence;*" and it is only in the present edition, that he has gone to his present extreme. The inconsistencies of his former hypotheses were most ably exposed by Mr. Robertson in his 'Critical Remarks on certain recently-published Opinions concerning Life and Mind,' (London, 1836;) and we extract from that publication a passage which anticipates the progress of Dr. Prout's views. "Did Dr. Prout unequivocally allow that the organic agent possesses intelligence, the inquirer would be easily satisfied; for from analogy he would be led to regard this agent as an order of being most amazing in the variety and compass of its powers, surpassing the human intellect in an infinitely greater degree than the latter surpasses the intellect of the worm or zoophyte. And the longer he pursued the comparison between the powers and resources of his own mind and those of this agent, the more irresistibly would he feel the conviction stealing upon him, that Dr. Prout's 'organic agent' is no other than that great Being, who alone, in the language of Scripture, is 'wonderful in counsel and excellent in working.'" It is, indeed, fully admitted by Dr. Prout, that the active intelligence, which can alone explain organization, is vested in the great Source of all knowledge and of all power; but he considers that the idea of mediate agency is in general most in accordance with natural operations. Now, we have already pointed out, that the hypothesis is alike cumbersome and illogical in regard to ordinary physical phenomena; and we cannot see on what firmer basis it rests in regard to those of a vital character. Fully agreeing with Dr. Prout "that matter and material forces when properly directed are fully adequate to form organized bodies;" and that "material forces fulfil the will of the Creator in organic processes, without any knowledge or will of their own," (p. 401); we must confess our inability to see any ground for the assumption, or to detect what is gained by the hypothesis, that the will of the Creator is not directly exerted upon matter in the construction of organized bodies, but employs a mediate agent. Dr. Prout maintains that "the hypothesis of mediate agency is not in any way derogatory to the Deity, but the reverse; inasmuch as the creation of an intelligent agent capable of accomplishing his purposes conveys to us a more exalted notion of his power, than the accomplishment of his purpose by himself." We cannot say that this appears to us in the same light; and we shall illustrate our view of it by a simile. Suppose a general to be in command of a large army, every component individual of which is perfectly under his control. If he had the means of so operating *immediately* upon the

minds of every soldier, as to cause him to execute his will without any intervening command, and if he himself possessed the knowledge and skill requsite not only to guide the general evolutions of the *masses* composing the army, but to direct the movements of every *man*, in the most advantageous manner,—would he trouble himself with the intermediate machinery of subordinate generals, colonels, captains, lieutenants, sergeants, and corporals, in order to convey his will and direct the movements of the troops? Or if he should do so, still retaining his absolute power over every individual action, would the function of his officers be anything else than that of mere mouth-pieces? As an army is at present constituted, there is large room for the operation of intelligence in the subordinates or delegated powers; because it is upon them that the carrying-out of the will of the commander into its details entirely rests; and they have not only to choose the best means of doing this, but to modify and accommodate it to circumstances, sometimes adding to it, sometimes (it may be) even acting in opposition to it. But this is the necessary result of the imperfection of the *human* commander, whose knowledge and power are alike limited, and who is compelled to act through the medium of subordinates. To imagine that anything is gained by the interposition of "intelligent agents" between the Deity and the materials on which he operates, is either to set limits to his knowledge and his power, or to give to those agents an office purely nominal. For if they have any of that freedom of choice in their actions, which is possessed to a certain extent by the officers of an army, then the Almighty intelligence is *not* directing the operations of matter in their detail; whilst if that detailed direction has a real existence, they are merely like so many instruments through which the Creator acts, and there is no room for the operation of intelligence in them. Or if it should be argued that they are left free to choose, in ignorance of the will of the Creator, who has nevertheless predetermined what they *shall* choose (which seems to be the position of man,) we answer that this supposition is completely at variance with the fixity and determinateness of the phenomena of organization; which, when properly surveyed, give to us an idea of *law*, or in other words of being the working-out of a definite and uniform plan, fully as much as do those of physics and chemistry. Whereever we see the operations of a *limited intelligence*, as in man and some of the higher animals, there we witness an indefiniteness and want of fixity, to which the operations of the simplest organism could not be safely left.

We shall refrain from urging any further objections to the doctrine in question; not, however, from want of argument, but for want of space; but we shall extract the two passages in which it is carried out to its fullest extent, and leave it to our readers to digest them, if they can. To us they present the hypothesis in a form so utterly unphilosophical, as scarcely to require further disproof.

"Admitting then that the Deity acts through the intervention of delegated existences, and that neither matter nor material forces, nor any assumed *vital* forces can be supposed to possess intelligence; we are driven to the assumption, that this intelligence is vested in the immaterial conscious beings; in other words, that one or more agents possessing knowledge, will, and power, exist in every organized body, from the lowest to the highest. In its lowest grade, the knowledge and operations of such agent may be supposed to be confined to the properties of the material elements adapted for organization, which it has the knowledge to select and the power to form, into an apparatus enabling it to organize other matters, and effect ulterior purposes. Where the operations of this primary

organic agent terminate, those of another and higher organic agent may be supposed to begin; which by carrying the general process of organization a step further, adapts the organized material for the operations of a third and superior agent. Thus each new agent may be supposed to possess more or less control over all the agents below itself, and to have the power of appropriating their services, till at length in the combined operations of the whole series of agents at the top of the scale, we reach the perfection of organic existence. (p. 403.)

We cannot understand this passage in any other way, than as implying that there are numerous grades, and an almost infinite individual multiplication, of "organic agents," in every one of the higher organisms. For, as a single cell may be a living being in itself, and may maintain an independent existence, it must necessarily possess its own organic agent; and, by parity of reasoning, since every cell in a complex organism performs its functions in virtue of its own inherent powers, it must be alike furnished with its organic agent—consequently there must be as many distinct intelligent agents in the body of a man, as there are individual cells. This, indeed, we may infer to be Dr. Prout's view, from the following passage:

"The ranks and grades among organic agents are doubtless as numerous as the ranks and grades among organized beings; but organic agents have been, and as far as our present views are concerned, may be classed under three grades. *Organic* agents strictly so called, the immediate fabricators of plants and animals: *animal* organic agents; and the intellectual agent peculiar to man; which properly speaking ought not to be classed with organic agents; since it cannot be shown that the intellectual agent in man has anything to do with the structure of his corporeal frame. In man, as we have said, all these three agents coexist. The organic agent or agents strictly so called, the immediate fabricators of his body, whose intelligent agency consists in wielding material forces, and, by their aid, of organizing matter into the primary condition of irritable cells. Various agents of higher rank, having the capacity of controlling organic agents, and of modelling the primary irritable cells into masses and *forms*, suited to their purposes. And lastly, the intellectual agent, who, capable to a certain extent of influencing all below itself, employs the organized machinery constructed for its use, and suited to its exigences, by the inferior agents, and thus becomes the organized being, man." (p. 406.)

We must confess our inability to discern the essential difference between this figment, and the mythical notions of the ancients respecting their presiding deities or semi-deities, which we presume that Dr. Prout would condemn as absurd. And we think it would not be very difficult to erect just as good an argument in defence of *their* existence, as that which Dr. Prout has put forth as the foundation of his system of organic agents. Thus, why should we not suppose an "intelligent agent" concerned in the selection and arrangement of the particles of silver in a certain metallic solution, so as to develop the *arbor dianæ;* or why should not suppose that Mercury is exercising a *choice* in behalf of the particles of his metal, when they leave one combination and pass into another, by the exercise of what is commonly termed *elective affinity?*

It is but justice, however, to Dr. Prout that we should add—what, indeed our readers may have already inferred from the recent development of this doctrine—that it is essentially independent of the rest of the work, and in no way interferes with its philosophical or theological value, except as constituting an ugly excrescence, from which we should be very glad to see it freed.

ART. VIII.

1. *Traité des Maladies des Articulations; accompagné d'un Atlas de* 16 *Planches.* Par A. BONNET, Professeur de Clinique chirurgicale à l'Ecole de Médecine de Lyon, &c. *Paris*, 1845.

A Treatise on Diseases of the Joints; with an Atlas of 16 *Plates.* By A. BONNET, Professor of Clinical Surgery in the School of Medicine of Lyons, &c. *Paris*, 1845. 2 vols. 8vo, pp. 1229.

2. *Recherches pour servir à l'Histoire des Tumeurs Blanches.* Par M. RICHET, Prosecteur de la Faculté, &c. (*Annales de la Chirurg. Français et Etrangère*, Mai, Juin, 1844.)

Researches intended to elucidate the History of White Swellings. By M. RICHET, Prosector to the Faculty of Medicine of Paris. *Paris*, 1844. pp. 146.

3. *De la Résection de la Tête du Fémur.* Par M. BONINO. (*Ann. de Chirurg. Franç. et Etrang.* Avril, Mai, 1844.)

On Resection of the Head of the Femur. By M. BONINO. pp. 38.

4. *De la Coxalgie.* Par M. le Docteur J. G. MAISONNEUVE, Chirurgien du Bureau central des Hôspitaux civils de Paris. (*Ann. de Chirurg. Franç. et Etrang.* Dec. 1844. Janv. Fev. 1845.)

On Coxalgia. By J. G. MAISONNEUVE, M.D., Surgeon to the Central Bureau of the Civil Hospitals of Paris.

5. *Recherches sur le Mal Vertébral de Pott. Mémoire couronné par le Conseil des Hôpitaux.* Par le Docteur TAVIGNOT. (*L'Expérience*, Juin, Juillet, Août, 1844.)

Researches on Pott's Disease of the Spine. By DR. TAVIGNOT; *being a Memoir to which a Prize was awarded by the Council of the Hospitals of Paris.*

6. *Maladie des Articulations Costo-chondrales et Costo-vertébrales, avec ou sans Ramollissement Tuberculeux et Nécrose des Os du Rachis.* Par A. TOULMOUCHE, D.M., Professeur de Pathologie externe à l'Ecole préparatoire de Médecine à Rennes. (*Gazette Médicale de Paris*, Janvier, 1845.)

On Disease of the Articulations of the Ribs, with or without Tubercular Softening and Necrosis of the Vertebræ. By DR. A. TOULMOUCHE, Professor of External Pathology in the Preparatory School of Medicine at Rennes.

M. BONNET is already well known by numerous contributions to surgical literature in various journals, and by his work on Section of the Tendons and Muscles of the orbit; he now appears in a more ambitious character as the author of two large volumes on a most important class of diseases, the fruits—he informs us—of six years' diligent and special investigation, aided by the ample opportunities for observation which he enjoyed as surgeon-in-chief to the Hôtel-Dieu of Lyons. Both the character and position of the author, and the importance of some of the positions he advances, call for a somewhat extended notice of his work: but we shall not of course, attempt a systematic analysis of its entire contents. We shall limit our attention almost entirely to those points on which Bonnet's

views claim to be, or really are, more or less novel and peculiar; and we shall take occasion to advert to whatever seems most worthy of notice in the several memoirs whose titles are above transcribed.

Few authors now-a-days think it necessary to offer any excuse for adding to the already unwieldy bulk of medical literature, but M. Bonnet explains at great length his motives for writing the present work, and his explanation may be reduced simply to this:—That no complete treatise on diseases of the joints has hitherto appeared. It is true that Brodie has published what M. Bonnet rather superciliously describes as "a collection of memoirs on some very limited points of pathological anatomy, and of diagnosis, under the name of a treatise on Diseases of the Joints;" but we have no work "qui embrasse l'ensemble des arthropathies;" and it is this deficiency which our author professes to supply. All preceding writers have, he maintains, taken a one-sided view of the subject. One set of authors, belonging to the school of vitalism, have chiefly investigated the relations of diseases of the joints with general affections of the system, and another, the pathological anatomists, have almost exclusively examined their local causes, lesions, and treatment. M. Bonnet's aim has been to combine the somewhat exclusive tendencies of those two schools, "to apply the idea so frequently spoken of, but so seldom followed out, of not separating medicine and surgery." (p. 197.)

With the view of considering diseases of the joints under every aspect, M. Bonnet divides the work into three parts. The first part is devoted to diseases of the joints in general, and is divided into four chapters:—*Pathological Anatomy*, *Etiology*, *Diagnosis*, and *Treatment*. The second treats of the several kinds of diseases to which joints are liable. And in the third are considered the special characters and treatment of those several diseases as they occur in each particular joint. This arrangement, whatever may be its advantages, has the inconvenience of leading to frequent repetitions, and as frequently severing matter which would be much better continuously embodied.

We may here observe that M. Bonnet limits himself to the consideration of what he terms "vital disease,"—"organic alterations of the joints." "Traumatic lesions," however, "such as sprains, contusions, and wounds, are studied, but chiefly in their relations to the pain, inflammation, and abcesses which they may cause." (vol. xi. p. 1.) The history of fractures and dislocations of the joints, and of those deformities which are not consecutive to diseases of the articulations, such as club-foot, congenital luxations and lateral curvature of the spine, is omitted to avoid extending the work to three volumes.

I. Pathological anatomy of the joints. M. Bonnet states that on this subject his *dissections* have not led him to the discovery of anything new: "If," he says, "I have added anything to the pathological anatomy of the joints, it is less by the ordinary means of dissection than by employing methods of research imperfectly or seldom applied . . . . . with this view I have had recourse to the chemical analysis of the diseased products, to the artificial production on the dead body of various physical lesions, and to the comparative study

of diseases of the joints in men and in animals." (p. xi.) We shall therefore only notice those points on which M. Bonnet claims to have thrown new light, except when reference to other matters is necessary to introduce or explain our author's views.

Chemical analysis. The views to which M. Bonnet has been led by the chemical analysis of the products of disease may be thus stated: The elementary lesions which occur in the structures of the joints, whether in the bones or in the soft parts, are divisible into two classes: one comprising such products, *e. g.* pus and tubercle, as are completely insusceptible of organization, and which, being therefore only capable of cure by elimination, tend, in the vast majority of instances, to make their way to the surface, and determine the successive absorption of the textures in which they are deposited; the presence of these products is consequently unfavorable, and generally indicates an unmanageable state of disease. To this class we need not further allude. The second class includes those products that are capable of being organized: they are "*fungosities*," and *lardaceous*, *fibrous*, *cartilaginous*, and *bony tissue*, which all essentially consist of *plastic lymph* in one or other of the phases of its development. Under ordinary circumstances, plastic lymph, at first unorganized, is soon penetrated with vessels, and ultimately becomes cellular or fibrous, or even cartilaginous or bony; but it by no means uniformly runs through its regular periods of organization, which on the contrary may, under the influence of various disturbing causes, be more or less permanently arrested at an early stage of its development. Thus, plastic lymph, when once organized, is subject to the diseases of the natural textures; for example, it may become infiltrated with pus or with tubercle; its organization then sustains a true arrest of development, and it remains in a state of which the granulations of an ulcer, and "*fungosities*" of joints are examples; a similar effect may be produced by a purely local cause, as the presence of a foreign body; in illustration of which M. Bonnet refers to the granulations in necrosis or of an issue, which persist indefinitely until the dead bone or issue peas are removed, when they pass to their third stage of cicatrization or conversion into fibrous tissue. In proof of these views, M. Bonnet appeals to the chemical analysis of *fungosities* and of *lardaceous tissue*. *Fungosities* he has found by chemical analysis to "be formed of serum and of fibrin, permeated by vessels; in a word, they consist of plastic lymph at that stage which I have considered as the second period of its organization." (vol. ii, p. 19.) *Lardaceous* tissue, which, together with *fungosities* and pus, forms most of the alterations known as white swelling, including the disease described by Brodie as a "morbid change of structure of the synovial membrane" is proved by chemical examination to consist of fibrous and cellular tissue, infiltrated with serum and with fibrin; if the former preponderate, it is soft; if the latter, it is white and compact; occasionally it is permeated by an unusual number of vessels, and is then less dense, and more or less red. *Lardaceous* tissue may be considered as intermediate between *fungosities* and *fibrous* tissue, and thence, both in theory and in fact, its presence indicates a better state of the constitution than the existence of *fungosities* alone does, and when the tumour contains both, the more the former preponderates the more favorable is

the case. It is unnecessary to notice M. Bonnet's remarks respecting adventitious bone and cartilage; and as regards *fibrous* tissue, we need merely state that M. Bonnet regards it as the indispensable condition of recovery from the above-mentioned lesions, and that is found in every case which either tends to a cure, or has actually been cured, excepting where *fungosities* are developed in bones, and osseous deposit has occurred in the organized lymph.

It might seem natural here to inquire, what are the circumstances which modify the products of disease in affections of the joints? why in some instances products incapable of organization are generated? and still more, why it is that when an organizable product is effused, its organization is in some cases arrested, and in others completed? But M. Bonnet's views on those points will be better considered when we come to his chapter on etiology.

Morbid changes. Such are the conclusions which M. Bonnet has drawn from the chemical analysis of the products of disease. He does not attempt to trace or describe the anatomical characters of the morbid alterations in the various stages of their progress, and indeed he is of opinion that this mode of research has been nearly exhausted by previous inquirers. M. Richet, however, in the memoir mentioned at the head of this article, endeavours to trace minutely throughout their successive periods the pathological alterations caused in the joints by inflammation, and thence to establish the following conclusions: 1st. That neither cartilage of incrustation, or extra or intra-articular fibrous tissue are susceptible of primary alteration. 2d. That consequently disease of a joint can only commence in the synovial membrane, or in the bone, excepting in the rare cases in which it begins in the soft parts external to the joint. And, 3dly. That the diseases termed white swelling are always the result of *synovitis* or osteitis, except in the comparatively very few instances in which they are consequent on tubercular degeneration. (pp. 174-5.) The last of those conclusions, though differently expressed, substantially agrees with M. Bonnet's views, and in other respects also we shall find the two writers pretty nearly agreed, although their inquiries were so very differently conducted.

Synovitis. Though dissections of joints affected with *chronic synovitis*, in its advanced period, are numerous, opportunities of examining the anatomical alterations of *acute synovitis* in all its stages are rare, and M. Richet therefore bases his description on numerous experiments performed on animals, and on some cases of traumatic arthritis, dissected at various stages of the progress of the inflammation. Within from four to six hours after having freely exposed the synovial membrane of a dog, sanguineous congestion commences in the sub-synovial cellular tissue, rather than in the membrane itself. In about ten hours the membrane loses its polish, but M. Richet never found it become preternaturally *dry*. During the second day the redness becomes more superficial, and less uniform, being disseminated in patches, and the surface of the membrane is covered with a sero-sanguineous oozing, which gradually gets thicker, and by the third is replaced by thin pus: the surrounding tissues are now highly congested, the synovial membrane is uniformly red, and very minute granulations appear on its surface. The following days all those

appearances become more marked, and from the fifth to the twelfth day, a pseudo-membranous exudation forms on, and becomes intimately adherent to the granulations above mentioned: and at a still later period the synovial membrane assumes a fungous appearance, and forms a prominent chemosis round the cartilages, which retain their natural whiteness. In the case of a man who died of purulent arthritis on the twenty-fifth day after the removal of a foreign body from the knee, numerous soft fungous granulations formed a prominent ridge round, and encroached on, but did not adhere to the cartilages, which were intact. On the sixty-third day, after pure alcohol had been injected into the joint of a dog, the chemosed synovial membrane, covered with the pseudo-membranous exudations almost completely covered, but did not adhere to the cartilage, which had sustained no alteration, except a slight loss of polish, and an obvious diminution of its thickness. Such were the appearances observed up to about the sixtieth day. The changes that occur subsequently, M. Richet describes from pathological specimens, taken from the human subject; for though a white swelling, when amputated, has always existed more than two months, yet when the disease has commenced in the bone, as the synovial membrane is secondarily affected, opportunities occur of examining it in almost every stage of inflammation, which in such cases is not unfrequently chronic from the commencement. When inflammation of the synovial membrane, then, is very protracted, the granulations on its surface become more developed, and greatly increase its thickness, which is also augmented by œdematous infiltration, externally in œdema, which generally soon passes into the induration termed *lardaceous*. The natural structure of the membrane is now often completely lost and confounded with the new deposition on both its surfaces, but sometimes the adventitious structure can be removed, and the synovial membrane demonstrated. As already stated, the synovial membrane at first forms a prominent ring round the cartilages, but gradually it, or rather a pseudo-membranous prolongation from it, encroaches on, and more or less completely covers their surface, to which at first it is not adherent, but soon becomes so, feebly at first, after a time more firmly. The cartilages, during this process, become thin, and if they are eroded, the synovial membrane implants parasitic roots on the denuded bone. The cartilage being then included between the bone, (become highly vascular,) and the synovial false membrane, is slowly absorbed. Generally, when the disorganization has gone a certain length, pus or other fluid is effused into the joint. But in some the synovial membrane is greatly thickened and altered with very little fluid in the joint: this is the state termed *fungus articulorum*, the disease described by Brodie as a peculiar morbid state of the synovial membrane, which M. Richet maintains, from having traced up the successive alterations in various cases, is simply an advanced stage of very protracted chronic synovitis. When *fungosities* are once formed, they do not seem capable of retrogression; if the disease is arrested, they become firmer, and their cure consists in becoming truly indurated. (pp. 31-43.)

Osteitis. The anatomical characters of osteitis, especially in its early stages, are not very accurately determined, because an opportunity of examining it is not of frequent occurrence; we shall, therefore, give

an abstract of M. Richet's observations, which relate entirely to chronic osteitis of the articular extremities of the long bones, an affection apparently often connected with the scrofulous diathesis, and thence, perhaps, often regarded as a scrofulous affection of the bone.

We must first refer to a point in the anatomy of the long bones, to which M. Richet attaches much importance, viz. the free communication that exists between the cancellous texture of both extremities of a long bone through the medium of the medullary canal. In proof of this communication, M. Richet adduces two experiments,—1st, If a recent long bone is suspended vertically, after the removal of a portion of the compact layer of its depending extremity, the oily matter will exude entirely even from the cancellous texture of the upper extremity; 2dly, Water injected into the upper extremity of a femoral bone similarly prepared, escapes below in a few seconds. This fact, M. Richet considers, not only explains the easy propagation of disease from one part of a bone to either extremity, and the pain often experienced along and at the extremity of the bone opposite to that which is diseased, (p. 11,) but also gives a valuable diagnostic sign, inasmuch as when such pain does occur, he concludes that the bone must be the seat of the disease. (p. 153.) Now, though inflammation is the disease to which bones are most liable, and their spongy extremities are most frequently affected with it because of their great vascularity, yet the progress of inflammation in bone is so slow, that we can seldom examine it at its commencement, at least in the part originally affected; but in consequence of the facility with which it is occasionally propagated from one to the other extremity of a long bone, M. Richet has several times been able to examine the early stage of inflammation at one end, secondary to a more advanced stage at the other. In such cases, the bones, clothed with a thickened periosteum, seemed red and vascular, and, on tearing off that membrane, minute drops of blood, reappearing several times when wiped off, exuded from the surface of the bone, which sometimes, but not constantly, is decidedly enlarged, even at this early period. On sawing the bone longitudinally, it is found to be evidently softened, the cut surface is of a wine-red colour, and the cells, which are enlarged and filled with blood, mixed with oily matter, sometimes yield under the finger, sometimes are preternaturally condensed. At a more advanced stage, the foregoing appearances are exaggerated, and the compact tissue presents numerous minute perforations for the passage of vessels into the bone, on whose surface periosteal secretions may now begin to be deposited, whence, as well as from the expansion of the cells, its bulk is augmented. M. Richet especially insists on the enlargement of the bone, which has been denied by several English authors, but which he has repeatedly verified by dissection. He conceives also that it can be infallibly detected in the superficial joints during life by pressing on opposite points of the condyles of the femur, for example, firmly with the finger and thumb, until the œdema of the soft part is removed; the diameter of the bone can then be taken, with callipers placed in the depressions left by the fingers, and compared with that of the opposite bone. M. Richet considers that the enlargement so detected is a certain proof of the bone being diseased, and may also aid in determining whether it is primarily or secondarily

affected, as the enlargement is greater, and occurs more rapidly in the former than in the latter case. (p. 154.) Without in the least disputing the accuracy of M. Richet's dissections, we think the mode of mensuration he recommends eminently calculated to lead us to an erroneous conclusion, and we think his confidence in its accuracy has led him to regard enlargement of the bone as more frequent than it really is: that it does occur, and that not very rarely, has been long perfectly known; but we owe to M. Richet a more minute account of the mode of its development.

Proceeding with the further changes effected by inflammation in bone, we find that the redness, at first uniform, at length becomes concentrated at certain distinct points, varying from the size of a pea to that of a small nut, in some of which coagulated blood is found effused into the cells. At this period the compact layer of bone interposed between the cancellated texture and the cartilage of incrustation, becomes thin and perforated, giving passage to dark blood, which detaches the cartilage already preternaturally thin, and commencing to be pierced with holes beginning on its attached surface; but frequently the separation of the cartilage is effected by granulations (*fungosities*) growing from the diseased cells, and then, on removing the cartilage, the compact layer of bone comes away with it, and the granulations appear as a red fungous, uniformly flattened mass, because of the resistence offered to their development by the compact texture of the bone. That the granulations by which the cartilage is detached spring from the spongy cells, and not from a layer of cellular tissue, or of synovial membrane, interposed between the cartilage and the bone, as MM. Gerdy and Blandin, together with Mr. Liston, respectively maintain, is shown by the fact, that on tearing off the cartilage its attached surface is always rough from portions of bone adhering to it; the granulations, therefore, not being denuded until a film of bone is removed with the cartilage, must be situated beneath and not between the two structures. Concrete pus next forms in, or round, the isolated depôts of blood already mentioned, pronounced by M. Cruveilhier, in the specimens shown to him by M. Richet, to be true abscesses of bone; and the cells, in those situations, either become necrosed, or are absorbed, leaving a cavity of corresponding size; occasionally the sanguineous infiltration is not collected in circumscribed deposits, but is uniformly diffused; a large portion of the bone may then be necrosed, and either isolated by new bone, or the sequestrum may project into the joint. When the cartilage is perforated, the joint becomes diseased, and the inflamed synovial membrane undergoes the alterations already described. Finally, in some rare cases, the granulations of the bone detach the cartilage *en masse*, and it is found lying in the joint reduced to a very thin film. It is in the advanced stage of osteitis that the inflammation frequently extends to the other extremity of the bone, through the medullary canal, which is found intensely red throughout its entire length, though externally the shaft of the bone exhibits no indication of disease whatever. Osteitis also occurs as an affection secondary to synovitis, and the changes are essentially the same, but slower in their progress, and there is generally an early and considerable superficial deposit of bone near the joint. (pp. 130-151.) M.

Richet attempts no explanation of the fact that bone is sometimes softened, and sometimes rendered denser by inflammation; in other words, that the parietes of the spongy cells are sometimes thinned, and sometimes hypertrophied. M. Bonnet accounts for this on the general principle already explained. Plastic lymph in bone, as in other textures, tends to become fully organized, and here the last stage of organization is conversion into bone: but here, as elsewhere, its development may be arrested, and if it be completely so, it remains in the state of *fungosities*, which cause the softening and absorption of the tissues they are in contact with, and this happens in patients whose constitutions are greatly deteriorated; but when a certain degree of vigour and reaction exists, the arrest of development is only partial, more or less of the effused lymph becomes ossified, and the bone is proportionably condensed. The same considerations, M. Bonnet thinks, clear up the much-disputed question whether tubercles cause softening or induration of the bone, in which they are deposited: when the bone is hardened, it is not attributable to the deposition of tubercle, but to the contemporaneous secretion of plastic lymph, which has gone through its regular stages of organization. (vol. i, pp. 34-6.)

We may here just remark that M. Richet's description of osteitis includes the disease described by Brodie as "a scrofulous disease of the joints."

Diseases of cartilage. M. Richet next considers the pathological anatomy of the cartilages of incrustation. Both he and M. Bonnet adopt the opinion that cartilage is not vascular, and is not covered by the synonial membrane. We shall not detail M. Richet's arguments to show that cartilage is, to use his phraseology, or rather that of M. Velpeau, "organic, but unorganized," as they are only those previously urged by many whom he does mention, and by Toynbee, whom he does not mention. Suffice it to say, that M. Richet considers cartilage analogous in structure to the epidermis and the nails, and that it derives a parasitic nutrition by imbibition or endosmose from the bone to which it is intimately and *immediately* attached, and from the synovia with which its free surface is bathed. In evidence of the facility with which cartilage imbibes synovia, M. Richet refers to the fact, observed by Bichat, of it and the synovial fluid having been found yellow in jaundice; and to experiments, in which he injected various coloured fluids into joints of animals, and shortly after (within ten minutes in one experiment) found the cartilage uniformly stained throughout its entire thickness, while the other textures were only superficially coloured; and that the liquid part of the blood is imbibed by cartilage from the bone is shown, M. Richet considers, by the separation and erosion of the cartilage in disease of the articular extremities of the bones, which he attributes to this element of their nutrition being perverted or cut off. Consistently with these views, M. Richet denies that cartilage is susceptible of inflammation, or of active participation in the diseases of the adjacent tissues.

The only direct proof that cartilage is susceptible of inflammation, would be to demonstrate its vascularity; and it is singular, M. Richet says, that long as the matter has been in dispute, no preparation showing vessels in cartilage has yet been produced. Vessels belonging to the

membranous prolongation of the inflamed synovial membrane, have been erroneously considered as evidence of cartilage becoming vascular on the surface; and the punctated redness caused by minute perforations of cartilage being filled with blood from the cancellated texture of diseased bone, has been mistaken for vessels penetrating the cartilage from the bone. But M. Richet, in addition to this negative argument, urges that cartilage never becomes vascular under circumstances in which it must be expected to do so, were the occurrence possible. By no description of physical or chemical injury could he excite a trace of vascularity in the cartilages of animals. In man also accidental solutions of continuity of cartilage remain unaltered, and without any attempt at repair, for he recently found in a patient who died a month after having fractured the lower extremity of the radius, the fracture perfectly united, and all the adjacent textures highly vascular, but the cartilage, which had been divided, as the fracture ran into the joint, showed no vascularity or other alteration; the edges of the division were like that of a nail cut with a scissors. When cartilage is exposed, as M. Richet once saw after disarticulation of the knee, and as has occurred in several other cases, it neither becomes red nor inflamed, but is detached—pushed off as it were—by granulations from the bone. Again, portions of cartilage are not unfrequently met with in joints in a very advanced state of disorganization, retaining all the chemical and physical characters, but somewhat rough and thin, showing that their absorption had commenced, but not manifesting the slightest trace of vascularity. And, finally, in cancerous disease of a joint all the structures become involved, excepting the cartilages only, a fact long since observed by J. L. Petit, and of which M. Richet gives a new example.

As M. Richet denies any active participation of cartilage in disease of the joints, it remains for him to account for those changes which they undeniably do sustain; and these he also refers to perversion of their nutrition, or to mechanical influences. And seeing that every alteration of cartilage, even its total disappearance, *may* occur in joints, not indeed healthy, but certainly manifesting no sign of serious disease, he urges that when the same alterations occur in white swelling, they must be a consequence not the cause of the disease.

In joints, otherwise apparently healthy, and which performed their function naturally, and were free from pain during life, the cartilage may present various alterations. The slightest change seems to be *loss of elasticity*, first noticed by Delpech, which is usually accompanied by, and is, perhaps, the first stage of *softening*, in which the cartilage has lost its polish, and presents the appearance of parallel fibres implanted perpendicularly on the bone, and adhering through the medium of a firm transparent, gelatinous substance, or without any lateral connexion, and in either case flexible under the finger. M. Richet has repeatedly seen this condition of cartilage in old subjects, and has always found it coincide with a diminished quantity of synovia in the joints; and, coupling those two circumstances, he refers the change to the nutrition of cartilage by the synovia, and from the bone becoming less active. We consider it as somewhat analogous to the senile alterations, and fall of the teeth and of the hair. Partial or total absorption of the cartilage, is

perhaps, the third stage of this alteration; at all events it may occur without symptoms, and in joints in other respects healthy, at least apparently so; of which M. Richet saw a remarkable example in the ankle and knee-joints of an old man, a patient of M. Velpeau's, who walked freely a few days before his death, and during life never complained of any affection of the joints. Indeed, eburnation of the articular surface of a bone with absorption of the cartilage, is not unusual in old persons, and sometimes is attended with trifling inconvenience; and Girard, (Archiv gén. de Méd. 1824, t. iv, p. 195,) and Dupuy, (Id. 1835, t. ix, p. 276), have ascertained that the same condition exists in the neck of the astragalus of most old draught horses, without disenabling them to work.

Whenever cartilage is rough, uneven, or chiseled, there is always, M. Richet observes, acute or chronic disease of the joint, and those alterations are due to wearing with absorption, acting chiefly on certain points, and always caused by perverted or defective nutrition consequent on disease of the bone, or the presence of an abnormal liquid in the joint; for as cartilage lives at the expense of the bone and the synovial fluid, if they are diseased, it suffers also. In like manner, *erosion* of cartilage (for M. Richet rejects the term *ulceration* as applied to that structure) is always a secondary affection, sometimes depending on disease of the synovial membrane, but much more frequently, M. Richet thinks, on disease of the bone. In the latter case the erosion is almost always accompanied with more or less inflammation of the synovial membrane and effusion into the joint; and such are the cases, M. Richet says, which have been mistaken for primary ulceration of cartilage; but, he adds, the bone is *always enlarged*, and if sawn longitudinally *through the centre* of the erosion, a vascular inflamed point will be found corresponding to each erosion, while the bone in the interspace may retain its natural texture and colour. "Nevertheless," M. Richet exclaims—

"Many surgeons, Brodie and others, maintain that those losses of substance are the result of primary inflammation of the cartilage, of ulceration which subsequently extending to the adjacent structures, give rise to white swelling." (p. 168.)

This position, M. Richet says, Brodie founds on several imperfect, and therefore valueless, observations in which the only alterations found on dissection are stated to be "ulceration" of the cartilages, with enlargement ("*gonflement*") of the bone, and our author regrets that Brodie did not take a hint from this "*gonflement*," and examine the interior of the bone, which he would have infallibly found presenting signs of an advanced stage of inflammation. M. Richet also appeals to the absence of pus, in some cases as an evidence that erosion of cartilage is not ulceration, an objection which, he says, Brodie has endeavoured to anticipate by a petitio principii,—by assuming that ulceration of cartilage differs in this respect from ulceration of soft parts.—In fine, M. Richet concludes,

"1st. That cartilage of incrustation may be more or less altered in its structure.

"2dly. That its alterations are never inflammatory, and that consequently the terms *inflammation* and *ulceration* of cartilage should be rejected and replaced by the phrases *softening*, *wearing*, ("usure,") and *erosion*, &c.

"3dly. That the alterations are very seldom primary, and rarely occur, except the synovia or the bones have sustained modifications, which, by perverting the nutrition of the cartilage, leave them subject to the action of chemical or physical influences." (p. 174.)

In stating M. Bonnet's and M. Richet's views we have made scarcely a comment on them, and the length to which we have gone will prevent our making as many as we had intended; a few are, however, necessary. In the first place, we must observe that M. Richet, though he criticises Brodie's cases and doctrines, is either very imperfectly acquainted with, or very careless in his exposition. Brodie expressly says that ulceration of cartilage may arise from several causes, of which he mentions as the chief, disease of the synovial membrane, chronic inflammation, and scrofulous alteration of the bone, and "a morbid condition of the cartilage itself;" yet M. Richet writes as though Brodie considered ulceration of cartilage to be always a primary affection of that structure. M. Richet also reproaches Brodie with not having examined the bones sufficiently, and indeed saying nothing about them, except that they were *enlarged;* now, it is remarkable that there is no mention of the bones being enlarged in *any one* of the thirteen cases recorded in Brodie's chapter "on the ulceration of the articular cartilages;" but if M. Richet had read those cases, he would have found evidence that Brodie did examine the bones, and he would also have found evidence confirmatory, so far as it goes, of his own views. Of the thirteen cases eight only are given as examples of primary, and in *all* of them, except the first, (case 26,) it is expressly said the denuded surface of the bone was diseased; (carious or ulcerated being the terms used;) it is no doubt added that the bones retained their natural texture and hardness, except indeed in the 38th case, in which a considerable extent of the cancellous structure is stated to have been diseased, whence it is to be inferred (but it is an inference only) that in the other cases the cancellous structure was unaltered. We do not indeed quite understand this account; caries is a condition in which the bone presents increased vascularity, extending some distance, beyond the carious part, together with softening and suppuration; yet in several of the cases, e.g. the 28th, we are told that the caries was unaccompanied with the formation of pus; besides, caries particularly affects the cancellated structure of bone, but here it is stated to be confined to the thin layer of compact tissue on the articular extremity of the bone, without the vascularity, and the rarefaction, and softening of that tissue essential to its existence. It is difficult of course to draw any conclusion from those cases beyond the negative, that they certainly do not prove the primary ulceration of cartilages. On the contrary, they point directly the other way.

But if M. Richet has misunderstood one English writer, he has completely overlooked another; and yet his description of the invasion of cartilage by the synovial membrane, or pseudo-membranous prolongations from it, might seem to be in a great measure borrowed from Mr. Key. We cannot indeed affirm that such is the case, for M. Richet has evidently seen what he describes, and may have had no knowledge of Mr. Key's labours; whether this be the case or not, M. Richet's account is valuable,

as tracing the progress of the alterations of the synovial membrane in its various stages. Key and M. Richet both agree that cartilage may be absorbed through the instrumentality of the synovial membrane, and the chief difference between them is, that Mr. Key thinks this is effected in chronic synovitis, by the gradual development of the synovial membrane itself, and in acute cases by a false membrane, effused from, and superadded to, the synovial membrane; whereas M. Richet has, in every case, whether acute or chronic, seen the encroachments on the cartilage effected by the formation of a false membrane. The views of Mr. Key and of M. Richet respecting cartilage and its alterations, are somewhat analogous, but yet different. Key considers that the organization of cartilage is very low, M. Richet denies that it is organized at all. Mr. Key says its primary alteration is a breaking up of its tissue, and M. Richet says pretty much the same thing; but Mr. Key considers that this primary breaking up is a vital action originating in the cartilage itself, and seems to imply, without directly saying so, that this vital action is inflammation, while M. Richet attributes the alteration to the nutrition of the cartilage being impaired or cut off by the imperfect supply or obstruction of the fluids that feed it. Mr. Key thinks that disorganization of cartilage, occurring independently of disease in the other structures of the joint, must lead to suppuration, from its disintegrated particles mixing with the synovia, and irritating and causing inflammation of the synovial membrane; while M. Richet maintains an opposite opinion. Mr. Key, indeed, subsequently modifies his statement, admitting it to be possible that the debris of cartilage may be reduced to an homogeneous fluid, mix with the synovia, and be absorbed. And that such is the case, M. Richet shows, as cartilage may be *softened*, reduced to a *fibrous state*, or even *totally disappear* in joints, which certainly cannot be called healthy, but which sometimes present little or no symptoms of derangement during life.

We have seen that M. Richet denies that cartilage ever becomes vascular, and how he explains the alleged examples of such an occurrence. We do not pretend to be able to determine this question. When such competent observers as Sir B. Brodie (case 50), and Mr. Mayo (Med.-Chir. Trs. vol. xix, pp. 63-4,) each adduces a case in which they affirm that cartilage of incrustation was distinctly seen to be sensibly vascular, and when Mr. Liston, (Med.-Chirurg. Trs. vol. xxiii, p. 92 et seq.) professes to have determined the point in the affirmative by microscopical examination of the injected vessels, it is difficult to deny the fact, either on theoretical grounds, or on the strength of negative researches, however numerous. In the present state of our knowledge, the weight of evidence goes to establish that ulceration of cartilage by the action of its own vessels, is, at the least, an exceedingly rare occurrence; and the direct proof of its becoming vascular in disease of the joints, has been obtained by but a few observers, and in a few cases only; while it is at the same time indisputable that cartilage is frequently removed by the action of the adjacent tissues, without itself having any active participation in the process. We shall hereafter have occasion to remark on some of M. Richet's views respecting the pathology of the bone.

Experiments on the Joints. M. Bonnet adds, as an appendix to the chapter on pathological anatomy, an account of the effects produced "*by the forcible injection of liquid into the joints,*" (p. 50,) effects which, though entirely artificial, greatly elucidate, he thinks, important points connected with the symptoms and treatment of diseases of the joints. We scarcely think those experiments so fruitful in these respects as their author does, but they are certainly sufficiently interesting and important to require concise but accurate notice.

In injecting the joints, M. Bonnet, in the case of the hip, the knee, and the shoulder, lessened the weight of the limb by amputating it a little below the joint, and threw in, through a perforation in one bone (which was fixed), an injection which became solid on cooling, and he thus obtained an exact cast of the cavity of the articulation at its maximum distension, or in the event of the capsule being ruptured, could trace the exact course of the effused fluid.

The general effects of forcibly injecting a joint, are, 1st, To make the bones assume a determinate position, always the same in the same joint whatever was its position previously. 2dly. To enlarge the cavity of the joint to its maximum capacity. 3dly. To separate the bones, and always interpose a layer of the injection between the entire extent of their articular surfaces, contrary to the opinion of Sabatier, Boyer, Larrey, &c. That liquid in a joint, say the hip, must simply distend the capsule, and accumulate round the bones without being interposed between them. 4thly. To render the joints motionless till a portion of the fluid escapes. 5thly. When the fluid is pushed too forcibly, to rupture the capsule of the joint where it is thinnest and least supported.

The cause of the motion communicated to, and of the position assumed by, the bones of a joint when it is injected, is the interposition of the injected liquid between the articular surfaces and the unequal resistance of the ligaments; for the interposed fluid tends to separate the bones, and if they are kept at a given degree of approximation at one point, and free to separate at others, the point at which further separation is prevented is a pivot on which they move. The anatomical character of a joint, therefore, determines the direction of its motion when it is injected. In the orbicular joints, if the capsule is denser and less distensible at any one part than elsewhere, that part is the centre of motion. In the ginglymoid joints the two lateral ligaments simultaneously resist the separation of the bones, and therefore if the extent of the articulating surfaces is the same before and behind the lateral ligaments, the bones will assume the straight position, but if there is a greater extent of the articulating surfaces, either before or behind the lateral ligaments, the pressure of the liquid will be greater on the more extensive surface, and consequently the joint will be flexed in the direction opposite to that surface. (vol. i, pp. 50-64.) Thus the *knee*, when injected, assumes the semiflexed position at an angle with the femur somewhat greater than a right one; the lateral ligaments being situated nearer the back than the front of the joint. (vol. ii, p. 152.) In the case of the *hip*, the femur is flexed so as to make an angle of about sixty degrees with the abdomen, and is at the same time abducted and rotated outwards, because the

capsule, being scarcely extensible anteriorly, and externally, and considerably so inferiorly and internally, the head of the femur can be separated much further from the acetabulum in the latter situation; but the motion in which the inner and lower part of the head of the femur is separated more than its outer and anterior part from the acetabulum, is just that during which the shaft of the bone is carried forwards and outward, or, in other words, is flexed and abducted. The rotation of the femur outwards arises from the direction of the fibres of the capsular ligament, being oblique from above downwards, and from without inwards, and when the capsule is distended, they tend to become straight, and in so doing exert a traction on the bone which rotates it outwards. Moreover, the shape of the articulating surface of the femur is such, that it cannot be flexed and abducted without a tendency to rotation outward. (vol. ii, p. 266.) In the *ankle*, the foot is *very slightly* extended on the leg. In the *shoulder* the humerus is abducted at an angle of about thirty-five degrees, with the side of the thorax, and carried forward in an arc of a circle of about fifteen degrees, because the capsule is least distensible, and shorter at its outer and upper part. In the *elbow* the fore-arm is flexed to nearly a right angle with the humerus, in a position intermediate between pronation and supination. In the *wrist* the hand is always brought straight with the fore-arm.

As forced injection brings a joint into a determinate position, we might expect that those experiments would explain the position assumed by joints in diseases accompanied with effusion into their cavities; but it is only when the effusion is very rapid, as well as abundant, that it can produce this physical effect of forced injection; and even then the effect would not be entirely physical, for in the position in question the capacity of the joint is at its maximum, and consequently the tension of the fluid on the capsule the least possible, but this is precisely the posture the patient would instinctively select to diminish pain. Whenever effusion into a joint occurs somewhat slowly, the capsule yields to the progressive distension, and the liquid not being interposed between, and therefore not tending to separate the bones, they are not forced into the position which leaves the greatest space between them. M. Bonnet somewhat superfluously applies those experiments as grounds for insisting on the necessity of perfect rest in penetrating wounds of joints. As the capacity of a joint varies with its position, it must be alternately increased and diminished during motion; and when increased, as part of its parietes are rigid, there is a tendency to a vacuum; and, when the joint is open, air passes in, (the prejudicial influence attributed to which by M. Bonnet, we shall hereafter consider;) when a joint, therefore, is wounded, we should keep it perfectly motionless in that position in which its capacity is least; and, by a fortunate coincidence, that position is the one in which an anchylosed limb is the most useful, being extension in the hip, knee, and wrist, and flexion in the ankle and elbow. (vol. i, pp. 262-8.)

Position of the limbs in diseased joints. M. Bonnet discusses the causes and effects of a faulty position of the limbs in diseases of the joints in the chapter on "*Etiology*," but this is a more convenient and seems also a more suitable place to consider this subject, to which

M. Bonnet attaches the greatest importance. He considers that a faulty position of the limb exerts the most powerful influence on the progress of every disease of a joint, and that the re-establishment of a correct position is one of the [often the most] powerful remedial agents at our command. In a few exceptional cases, a bad position of the limb may not aggravate the evil, but, as a general rule, it augments inflammation and pain, by exerting a violent and continued tension on the synovial membrane, the ligaments, and the soft parts generally, and leads to spontaneous luxations, which are simply the result of a vicious posture of a joint whose ligaments are weakened or destroyed. (vol. i, p. 89.)

The causes of the position assumed by a limb in disease of a joint, are, 1st. The physical effects of an accumulation of liquid in the synovial cavity. 2dly. The weight of the limb, and the pressure of external objects, as the bed-clothes, &c. on it. 3dly. The necessity experienced by the patient of keeping the affected joint as motionless as possible. 4thly. The active or passive contraction of the muscles. The first of these causes has been already alluded to, the others shall be presently noticed when illustrating the mechanism of the deformity in some of the individual joints.

Any position of a joint is injurious,—1st, which causes continued distension of the soft parts on one side of the joint,—for the synovial membrane, ligaments, &c. are uniformly found most diseased on the side so distended; 2dly, which determines considerable and continued pressure on any part of the bones of the joint,—for the bones and cartilages are more extensively absorbed and ulcerated where such pressure exists; and, 3dly, which tends to produce spontaneous luxations, (vol. i, pp. 78-85.) The influence of such positions will be best estimated by referring to some particular joint.

In disease of the *knee* the patient may lie on his back with the limb extended and lying on its posterior surface, but usually it is rotated outwards by its own weight, a rotation increased by the weight of the bed-clothes when once commenced. But this posture is unsteady, and is therefore seldom adopted; the patient seeks a more fixed position, and for this purpose turns more or less on either side, and at the same time flexes the leg on the thigh, and the thigh on the pelvis, in order to give the limb an extended base of support. The same thing indeed is commonly done in health when lying on the side, with the lower extremities extended; it is difficult to keep the body motionless without an effort, which ceases to be necessary when the limbs are flexed. If the patient lie on the diseased side, the knee is everted; if he lies on the sound side, it is inverted. When the flexion is once thus established, it tends to cause displacement in the following manner:—The leg, when extended on the thigh, enjoys neither lateral nor rotatory motion; but if it is flexed on the thigh at any angle of from forty to one hundred degrees, it is easily rotated both outwards and inwards; because in the extended position the whole of the articulating surface of the tibia is in contact with that of the femur, and the tibia cannot rotate on its axis without one of its condyles being carried backwards against the posterior ligament, which resists the motion; but in the semi-flexed posture, the centre only of the tibia is in contact with the femur, and the posterior ligament

being relaxed, offers no resistance to rotation; spontaneous luxation of the tibia, therefore, especially that accompanied with rotation, is favoured by the semi-flexed posture. But greatly the more serious mischief results from the pressure generally exerted on the joint when in this position. If, indeed, the limb lies on its *entire* outer surface, there is neither distension of the ligaments, &c. nor tendency to spontaneous luxation. But lateral decubitus is seldom complete; and, when the limb is everted, it usually bears chiefly on the outer edge of the tarsus, and of the lower part of the leg, while the knee is at the same time imperfectly supported. The tibia consequently tends to make with the femur an angle salient externally; and, therefore, the external lateral ligament, and the soft parts generally of the outside of the joint, are necessarily distended. Furthermore, the lower part of the leg is carried by its own pressure on the bed forward and inwards, whence its upper portion is impelled in the reverse direction, or backwards and outwards. All those causes, therefore, concur to produce a partial luxation backwards, outwards, and with rotation outwards. When the semi-flexed limb is *inverted*, the effects of the position are different. The limb is then supported on the entire inner edge of the foot, and there is then no tendency to luxation with rotation, or to luxation backwards. The lower part of the leg is carried directly outward, whence the tibia tends to make with the femur an angle salient internally, and re-entrant externally, and the internal lateral ligament, &c. being thus put on the stretch, it might be expected that the tibia would tend to become luxated directly inward. But M. Bonnet has not observed this in three cases, which he has seen since his attention was particularly directed to the relation between spontaneous luxations and the position of the limb, in which the leg rested on the internal border of the foot, there was no luxation, but the tibia and the femur were absorbed externally where they forcibly pressed against each other, and a considerable hollow existed in this situation when the limb was straightened. (vol. ii, pp. 156-62.)

In diseases of the *hip*, the thigh is never extended on the pelvis, as it is when we stand in a perfectly vertical posture. The femur is always flexed on the pelvis, usually at an angle of about 150 degrees, and may or may not also deviate to either side. Hence the thigh may assume three positions in relation to the pelvis. 1st, Flexion without lateral deviation, which is extremely rare; 2dly, Flexion with abduction, which is always coupled with rotation outwards, and occurs frequently; 3dly, Flexion, with abduction, which is the most frequent, and always coincides with rotation inwards. These are the only positions that occur in diseases of the hip, but the flexion of the thigh may sometimes be easily overlooked; thus, when the patient lies on the diseased side, the inferior extremities may be extended on the bed, but the pelvis is then flexed on the femur; in other words, the axis of the pelvis, and the axis of the femur, form an angle open anteriorly, just as when the pelvis is fixed, and the thigh flexed on it.

As to the cause of the flexion of the femur, it is sometimes at least partly referrible to the cavity of the joint being most capacious in this position, whence the diminution of pain consequent on the relaxation of the capsule of the joint occasionally causes it to be selected; but M. Bonnet

thinks this position is assumed chiefly because it is always the natural position of the thigh when at rest in the recumbent position. When lying on the back, the trunk is raised by pillows, and therefore flexed on the thigh, and in decubitus on the side, the thigh is flexed to give a sufficient base of support to the body. But why is the thigh, when flexed, sometimes everted, sometimes inverted, and sometimes neither? M. Bonnet attributes these differences chiefly to the position in which the patient habitually lies. During dorsal decubitus, if the ham is supported by a pillow, the thigh may be simply flexed directly forwards; but if the limb is not supported behind, or if the patient lies on the affected side, it is everted; on the contrary, it is inverted by its own weight when the patient lies on the sound side. But if the mode in which the patient lies alone determined the inclination of the thigh outwards or inwards, adduction should only occur when the patient lay habitually on the sound side, but it occurs also when the patient lies on the diseased side; this is sometimes caused by the relation of the femur to the pelvis having been previously established while the patient lay in another position; but sometimes it arises from this :—as has been already stated, rapid and copious effusion into the hip-joint abducts and rotates the thigh outwards; but when the capsule becomes perforated, the disease has often made much progress at the inner part of the joint and among the soft parts external to it, and the patient inverts the thigh to relax the soft parts in that situation. This explanation, M. Bonnet says, tallies perfectly with facts. Usually at the commencement of disease of the hip joint, the femur is flexed, abducted and rotated outwards, and at a more advanced stage is flexed, adducted, and rotated inwards.

As to the *effects* of those positions, *slight* direct flexion of the femur is the most favorable posture, for then the capsule is nowhere distended, nor does the head of the femur press unequally on the cotyloid cavity; neither is there any tendency to luxation; and anchylosis, if it occurs, does so in the most favorable position. Considerable flexion is, however, injurious, as the head of the femur then distends the capsule posteriorly; and in the event of anchylosis, when the patient attempts to walk, the hips are projected back, and the trunk arched forward, as during progression the thigh must be nearly vertical, and the trunk must assume a position corresponding to the deformity of the limb. In flexion, with abduction, and rotation outwards of the femur, the evils just mentioned are aggravated, the capsule is distended internally and anteriorly, whence there is a tendency to dislocation into the foramen ovale, if the flexion approaches a right angle, or on the pelvis if the flexion is less, events, however, which very rarely occur. In flexion of the thigh, with adduction and rotation inwards, the head of the femur bears against the posterior and upper part of the capsule of the joint, and of the cotyloid cavity, and spontaneous luxation is greatly facilitated by the deficiency of the brim of that cavity in that situation. (vol. ii, pp. 267-76.)

It is unnecessary to go through M. Bonnet's account of the mechanism and effects of position in the other joints, as we shall hereafter state his practical deductions from his views with respect to each joint. We shall for the present only add that M. Bonnet lays down, as an invariable rule of practice, that "whatever is the disease of a joint which we have to treat,

if the diseased limb is in a vicious posture, it should be reduced to a favorable one." (vol. i, p. 119.) This rule is not confined to chronic diseases, but is applied to every disease and every stage of every disease. Thus, in an acute inflammation of a joint (say the knee), he objects to the practice usually followed of leaving the limb in the position selected by the patient, and not attempting to straighten it till the inflammation has subsided, and on the contrary, inculcates that the limb should be at once, 1st, brought into a favorable position, and 2dly, then kept immoveable in that position by means of a suitable apparatus. These are the two great indications, according to M. Bonnet, to be fulfilled in the surgical treatment of diseases of the joints. In acute inflammation, a vicious position of the limb keeps the soft parts of the joint on the stretch; by straightening the limb, this painful distension is removed, and the inflammation and pain are allayed. The cases he gives, he says,

"Will demonstrate better than any reasoning the justness of his principles. They will leave no doubt of this truth, that when the most energetic antiphlogistics have completely failed in an inflammation of the knee, and the malady is becoming aggravated, straightening the limb and then keeping it immoveable by means of a suitable apparatus, is sufficient to arrest the progress of the disease, to cause cessation of the pain, and progressive amelioration." (vol. ii, p. 203.)

M. Bonnet acknowledges that this method occasions severe pain during the entire time necessary for straightening the limb; a period which is but short if the disease has not lasted for more than a month, but extends to two or three days if the malady has existed for two or three months. But once the limb is straightened and rendered motionless, the amelioration is immediately experienced. In upwards of twenty cases, he met two only which were not amended; those were cases of inflammation of the wrist, which he kept pronated, and he has since ascertained that the failure arose from having adopted this position, as the wrist should be kept in a position intermediate between pronation and supination.

The effects of protracted immobility of joints will be here properly noticed, both because of the pathological changes induced thereby, and because M. Bonnet founds thereon a rule of practice which he considers of very great value and importance.

It has long been admitted that protracted rest is capable of producing stiffness and some other alterations of the joints; but though the subject has been alluded to by J. L. Petit, Hunter, Boyer, Cruveilhier, Velpeau, Kunholtz, Malgaigne, and Vidal de Cassis, it has been more especially investigated by M. Teissier, whose researches go to establish that protracted immobility may cause—1st, effusion of blood and of serum into the cavity of a joint; 2d, vascularity and formation of false membranes on the synovial membrane; 3d, alterations of the cartilages; 4th, anchylosis.

Cloquet and Sanson both mention 'a local scurvy,' produced by long-continued rest during the treatment of fracture, especially the violet spots that not unfrequently appear on the skin. M. Teissier was the first to notice sero-sanguineous effusion into the joints under the same circumstances. Having for a considerable period taken every opportunity of examining all the joints of limbs kept long motionless, whether during the treatment of fracture or by paralysis, he almost constantly found all the joints, even those most remote from the injury, containing bloody serum, or even

blood almost unmixed, and in one instance clotted. Similar effusion also often existed in all the soft parts external to the joint. This effusion always coexisted with vascularity, especially of the fringed folds of the synovial membrane, which were swollen and red. False membranes were occasionally, but not often, found along with the foregoing alterations, and in every instance in which M. Teissier did find them they were already penetrated by vessels, and adhered to the cartilages. The cartilages sometimes presented an uniform or a punctated redness. When the cartilages were not eroded, the redness occurred as ecchymosed maculæ; when, on the contrary, they were eroded, it was punctated, and in one instance resembled arborescent vascularity, but this, M. Bonnet says, is the result merely of the cartilage imbibing the blood; for though redness by imbibition is uniform when the cartilage retains its natural structure, its surface becomes villous when the imbibition is punctated. Anchylosis is of course a remote result of perfect rest, and, so far as is known, is established by the false membranes adhering, and becoming cellular or fibrous. M. Bonnet gives several cases in which, after fracture of the shaft or of the neck of the femur, the foregoing alterations (excepting anchylosis) were found on dissection, not only in the knee but in the ankle. And M. Velpeau has published two cases which prove the possibility of anchylosis by cellular or fibrous union being caused by long-continued rest. The circumstances that favour the occurrence of those changes in joints when kept motionless are: 1st, the duration of immobility; 2d, old age; 3d, the whole body being kept at rest; 4th, a feeble or cachectic constitution. Whence they are less likely to occur in young, healthy, well-fed persons, and in those fractures, as of the upper extremities, which do not require confinement to bed, or do not take long to consolidate. M. Bonnet thinks it impossible, in the present state of our knowledge, to explain the occurrence of these alterations. From their nature they might be considered inflammatory, and supposed to depend on extension of inflammatory action from a fracture, or on some injury received at the time of the accident; but they occur in the joints most remote from the fracture, and also have been observed where the limb was motionless in consequence of paralysis. Moreover, in cases where the neck of the femur was fractured, the knee and ankle were considerably affected, while the hip was very little so; a result explained by the difficulty of maintaining the hip motionless, while the knee and the ankle are easily kept so. Finally, the alterations arising from immobility are not purely inflammatory; the characters of the sanguineous effusion are analogous to those of scurvy. M. J. Guerin attempts to account for these alterations thus: we have seen that the capacity of a joint varies during motion; in certain motions, therefore, there is a tendency to a vacuum produced in the joint, whence a suction is exerted on its parietes, which excites the exhalation of fluid; and thus he attributes to atmospheric pressure an important influence on the mechanism of serous exhalation. But when a joint is at rest, there is an equilibrium between the external and internal pressure, and therefore no suction and consequent suppression of the synovia; and M. Guerin thinks that the exhalation of the synovia being arrested, the fluids stagnate in the vessels, which leads to congestion and other morbid changes. This explanation, M. Bonnet objects, supposes that the synovia is diminished in quantity; but

the very contrary is the fact. (vol. i, pp. 67-78, and vol. ii, pp. 133-6.) We may here anticipate so far as to say that M. Bonnet deduces from the researches of M. Teissier, from the experiments of M. Guerin on the influence of atmospheric pressure on the exhalation of synovia, and from his own clinical experience, that motion exerts a most powerful influence in modifying the function and morbid condition of a joint; and that a joint should not be kept perfectly motionless for a considerable period in any disease, except in acute inflammations attended with considerable pain, in wounds of joints, and in those chronic cases in which a cure can only be expected from the establishment of anchylosis. (vol. i, p. 132.) But how are we to determine (suppose in a case of acute inflammation becoming chronic,) when immobility is to be replaced by motion? M. Bonnet gives no rule on this point beyond expressing an opinion that, unless some contra-indication exists, rest should not be prolonged beyond six weeks; but he quotes, without expressing any opinion as to its value, the following method for obviating this difficulty, proposed by M. Malgaigne in the Journal de Chirurg. (Oct. 1844):

"A certain method of diagnosis is pressure on the diseased joint, but not on every part of it indifferently, for experience shows that the pain has a place of election in each joint. Thus, in the shoulder, pressure posteriorly sometimes, but rarely, occasions pain; the place of election is anteriorly; elsewhere pressure may be made with impunity. In the elbow the place of election is over the head of the radius; in the hip, at the posterior part of the head of the femur, behind the great trochanter. If the pressure on the head of the humerus, anteriorly and posteriorly, on the anterior and posterior part of the head of the femur, and on the head of the radius, causes no pain, we may pronounce that arthalgia of the shoulder, of the hip, or of the elbow, has terminated, that the period for perfect rest has terminated, and that danger from motion has ceased; and then we should fearlessly encourage the patient to bear with fortitude the pain which he must endure on the very threshold of the second period of the treatment." (Vol. i, pp. 422-3.)

Experiments on mechanical lesions of the joints. Some account of M. Bonnet's experiments on the injuries resulting from forced motions of the limbs may naturally find a place here. It would, however, be superfluous to give a summary of all his experiments,—which, by the way, are detailed at most wearisome length,—both because many of the results are unimportant, and also because they chiefly bear on the history of fracture and dislocations, subjects, except so far as these experiments go, excluded from M. Bonnet's work. We shall, however, in order to give a general idea of M. Bonnet's researches, refer to a few points which may be interesting, either because of their novelty, or their possible practical application.

Fractures of the malleoli of the ankle, and of the inferior extremity of the radius, from violent motion impressed on the foot, or on the hand, are perfectly well known, but it is not so generally understood that analogous motions of the knee, the elbow, the shoulder, and the hip, may also cause fractures of the shafts, or of the articular extremities of the bones.

In the adult, when the leg is rotated violently on the thigh, as a general rule the knee remains intact, and the tibia is fractured about its centre, and the fibula close to its superior articulation; those fractures

are always very oblique, but no relation could be traced between the direction of the obliquity and of the rotation. The tibia is first fractured, and the fibula occasionally escapes, from the motion allowed of in its superior articulation, and from its elasticity permitting of a certain extent of torsion. The fractures of the fibula also may readily escape detection, the fragments being held together by the periosteum, and by the fibrous and aponeurotic expansions which cover the bone at the situation of the fracture. In children the results are usually the same, but in two out of eight experiments, the femur, and not the tibia, was fractured. In one case the femur was twisted just above the condyles, so that the external surface of the shaft corresponded to the anterior surface of the condyles. In the second case there was a very oblique fracture of the lower third of the thigh; the periosteum was only partially torn, and intervened between the fragments when an attempt was made to cross them. We may here mention that he scarcely ever, in his experiments on young subjects, detached the epiphysis, the bone was almost constantly broken immediately above the epiphysal cartilages; the deformity was very slight, and the fragments were not completely separated, being in a manner locked by numerous asperities, and always retained by some untorn portion of periosteum; sometimes indeed the periosteum was intact, and had to be divided to expose the fracture.

Generally, as already said, the knee is uninjured when the leg is forcibly rotated on the thigh, but in one experiment the tibia was partially luxated on the femur, and the appearances in this case explain, M. Bonnet thinks, the accident described by Sir A. Cooper and Mr. Key as dislocation of the semilunar cartilage of the knee. In this experiment the foot was suddenly rotated outwards while the leg was bent to a right angle with the femur, and immediately a peculiar sensation of something having slipped (*d'un soubresaut particulier*) was felt. The foot remained everted and the leg flexed at an angle of about 45 degrees with the thigh; the internal condyle of the tibia projected a little inward and forward beyond the internal condyle of the femur, and the head of the fibula was carried backwards and inwards. On extending the leg gently on the thigh, another slight slip was felt and the bones resumed their natural position; dissection showed that none of the ligaments or of the muscles were torn. M. Bonnet raising the patella to inspect the cavity of the joint again rotated the leg outwards, and saw that the slip or jolt was occasioned by the internal condyle of the femur passing behind the semi-lunar cartilage which was thus thrust forward on the internal articulating surface of the tibia, the capsule of the joint remaining unruptured; the external condyle of the femur was carried very slightly forward but remained in contact with the external semi-lunar cartilage. The joints of the subject experimented on, were, M. Bonnet says, very lax. The appearances in this experiment agree very well with the account of the cases termed dislocations of the semilunar cartilage, as regards the way in which the displacement was occasioned and the mode of remedying it, but differ in the very obvious deformity observed in M. Bonnet's experiment; still we think the experiment throws considerable light on the affection described by Key and Cooper, which, many surgeons suggest, were not examples of displacement of the semi-lunar cartilage but probably cases

of foreign bodies in the knee. M. Bonnet, we have seen, considers that the affection in question, is a partial dislocation of the condyle of the femur, permitted by relaxation of the ligaments of the joint—a view already taken of the affection by Malgaigne; but if the attachments of the semilunar cartilages, both to the tibia and to the capsule of the joint, are very lax, so as to allow them to slide on the tibia, (which is proved by dissection to occur,) then the cartilage may be pushed forwards by the condyle of the femur, instead of the condyle of the femur slipping behind it, as occurred in M. Bonnet's experiment.

To enter into a further account of M. Bonnet's experiments on mechanical lesions of the joints, would lead us too far from the proper subjects treated of in his work; but other occasions will occur on which we can notice them, so far as is necessary, in connexion with fractures and dislocations, to which, as has been already mentioned, they principally refer; and as it would be impossible to examine those parts of M. Bonnet's work which relate to the etiology, symptoms, and treatment of diseases of the joints within the limits now at our command, we shall conclude for the present, and resume the subject in our next Number.

ART. IX.

Untersuchungen über Entstehung des Krankheitsgenius, &c. &c. Von DR. MARTIN GEIGEL, &c. *Würtzburg,* 1840.

Researches into the Origin of the Morbid State, &c. By Dr. M. GEIGEL, Physician at Würtzburg. 8vo, pp. 480.

THE systematic collection of facts, the task undertaken by Dr. Geigel, is little less advantageous to science than the discovery of them, especially to medical science, in which the relations of a new fact are almost infinitely numerous. The discovery of some general principle in physiology leads necessarily to a completely new arrangement of the whole cycle of medical studies, just as in war a successful movement is followed by a complete change in the position of the corps d'armée. It is fortunate that labourers are never wanting thus to rearrange details, and group the tried experience of the past around the novelties of the present. The development of the modern solido-humoral pathology is the result of a rearrangement like that here alluded to. The humoral pathology based in a great measure upon experience gave way, in consequence of a natural reaction, to that founded upon general and pathological anatomy. The latter branches of medical science having in their turn been cultivated as far as possible by the simpler means of research, the microscope and a more refined chemical analysis were pressed into the service of the inquirer. A new impulse was thus given to physiology and pathology, and two new lines of research marked out, namely, microscopic anatomy and pathological chemistry, the bearings of which upon medical theory and practice are now beginning to show themselves distinctly, and to point to a fusion of what was valuable in the two theoretical systems mentioned.

The factors of the opposite organic conditions, says Dr. Geigel, are the nervous system and the blood. They have a reciprocal action, and consequently neither an exclusively solid nor exclusively humoral pathology can exhibit the

results of such action. If we would estimate the morbid action of the nervous system on the blood, we must seek out the material changes in the latter; but we cannot forget on the other hand that the morbid condition of the blood may be the primary cause of these changes. Dr. Geigel's plan includes the consideration of this mutual action and reaction of the blood and nervous system. He first considers the functions of the ganglionic system commencing with an analogy between them, as seen in the fetus and in plants. The influence of the sympathetic on the motion of the blood in the arteries and veins, and on the functions of the absorbents, on digestion, and on the evolution of animal heat are considered. Under a second head the excitants of the ganglionic system are reviewed, and especially the influence which the gases contained in the blood exercise upon them. The following passage will give the reader an idea of Dr. Geigel's views and method.

"Since we know that the blood cannot be vitalized without a nervous system, since we know also that the action of the nervous system cannot be developed without the blood, and, consequently that neither can exhibit life, or develop vital phenomena without the other; further, since we see that all matters received into the blood act variously, partly on the blood, partly on the nervous system, so matters must absolutely exist in the blood, which shall sometimes exalt, sometimes depress the functions of the nervous system, or produce both results according to the relations which they bear to the blood when received into it. These matters according to my views are the gases contained in the blood. The blood contains, according to the researches of chemists, oxygen, hydrogen, nitrogen, and carbon, the latter generally in combination with oxygen or carbonic acid." (p. 55.)

Dr. Geigel then details experiments showing the relative quantities of these present in arterial and venous blood respectively, and their operations on the nervous system. He infers that carbon paralyses the ganglionic system, but carbonic acid excites the susceptibility of the sensitive nerves. Nitrogen excites the sensitive system, while hydrogen paralyses it, or at least diminishes the irritability of the brain and nerves of sense, acting on the cerebral system as carbon acts on the ganglionic. Oxygen, however, is the primary excitant of the ganglionic system, increasing its activity and irritability. Nitrogen combined with oxygen, as the nitrous oxide, or with hydrogen as ammonia, increases and maintains the activity of the sensitive nerves.

Dr. Geigel then considers the excretory organs as the channels through which these various gases are removed in a solid form from the blood, and then discusses the action of the brain on the ganglionic system, and on the organs of the body generally. In the latter section a variety of physiological details are entered into which we cannot even enumerate.

In the section on the influence of the ganglia on the brain, Dr. Geigel develops a theory of temperaments. The gases in the blood perform a leading part in marking the different temperaments. If oxygen be present in a quantity above par, it determines the sanguineo-choleric temperament; if carbon, we have the melancholic or hypochondriacal. In the next section we have the pathology of fever, and inflammation, discriminating pulmonary inflammation, as it is connected with arterial or venous blood. As the function of the lungs has an important bearing on the changes in the blood, the physiology of sanguification, respiration, and circulation, is minutely considered, especially with reference to the

gases and the influence of the nervous system. The succeeding section is devoted to a practical application of the physiological views there developed. Dr. Geigel recommends the use of the electrometer and multiplicator in forming a diagnosis as to the nature of the blood in a patient, and consequently in learning the nature of his disease and the treatment suitable thereto. He thinks these instruments more important than the pleximeter and stethoscope; the data afforded by the latter are only mechanical, those by the former are dynamical. If for example, in a case of rheumatic inflammation of the lungs, the electrometer shows the individual to have negative electricity, or no electricity at all, we know that the crasis of the blood is venous, and as the absorbent power of the veins and lymphatics are diminished under such a condition of the blood, the inflammation must be treated with those remedies which excite the absorbent powers, as nitre, narcotics, mucilaginous medicines. All this, we need not say, is purely hypothetical.

There is a curious section on the laws of absorption from the atmosphere in which the influence on the blood of atmospheric pressure, temperature, purity, and dryness of the atmosphere, the influence of climates and seasons in modifying the composition of the blood and the functions of the nervous and vascular systems, are reviewed. Several pages are then devoted to the pathological applications of the laws thus illustrated, and the theory of epidemics, as cholera and puerperal fever, is discussed. The cholera is, according to our author, a disease dependent on increased venousness, and some space is devoted to a consideration of the epidemic constitution of the years preceding and following the outbreak of that disease. Dr. Geigel also shows the connexion of earthquakes, comets, and other astral and telluric influences on the constitution of the blood, giving it the arterial or venous character as the causes just mentioned, which develop the one or the other, prevailed. The locality and dates of the earthquakes observed in various parts of the world during the twenty-one years subsequent to 1816, are given at length from Professor Schön's researches, (of Wurtzburg.) This connexion of cholera with comets and earthquakes has been noticed by several English writers on the disease; as, for example, by Mr. Orton, whose publication may be considered almost as a standard work.

The remaining third of Dr. Geigel's volume is devoted to a consideration of the pathology and treatment of all the acknowledged epidemics, as ague, hooping-cough, sweating sickness, yellow fever, putrid fever, plague, ganglionic typhus, cholera, &c., scorbutus and rheumatic fever, each finding a place in the list. The hypotheses of our author are applied fully to all these diseases. The cause, for example, of intermittent fever, consists in a predominant supply of hydrogen in the blood. Altogether the book is a curious production, presenting some interesting views, and displaying very considerable research on the part of the author; but displaying also the most dogged and persevering hobby-horsicality we ever witnessed. The blood, blood-gases, and nervous system in all its divisions occupy every page from the first to the last.

ART. X.

Fruits and Farinacea the proper Food of Man; being an attempt to prove from History, Anatomy, Physiology, and Chemistry, that the original, natural, and best Diet of Man is derived from the Vegetable Kingdom. By JOHN SMITH.—*London,* 1845. 8vo, pp. 418.

THE present age is prolific in popular medical philosophy. Empiricism is not confined to barefaced quackery. There are metaphysicians amongst the mesmerizers and hygienists of a class far superior to the Morisonian. The lay philosophers we allude to have all-grasping minds. Whatever be their peculiar pursuit, they go deep into things. Mesmerism was practised by the early Christians; a fruit and farinaceous diet was the law of paradise. Mesmerism is destined to make manifest the mysterious connexion between matter and spirit; the general adoption of a fruit and farinaceous diet will usher in and characterize the millenium, and enable Great Britain and Ireland to support one hundred and ninety-five millions of inhabitants; that is, if we adopt the estimate and views of John Smith, the Pythagorean of Malton.

Although these extreme opinions are absurd enough, we by no means assert that the inquiries which have led to them are not to be commended. On the contrary, we hail the spirit of medical inquiry amongst laymen as one of the good signs of the times. It is they who will interpenetrate society with medical wisdom; they are the true interpreters and expounders of the esoteric lore of the medical profession, and it is they that will be no trifling instruments for elevating the profession in society.

Mr. Smith is not a mere theoretical writer: he has a claim to be considered a practical philosopher. His attention was directed to the investigation of human diet about ten years ago, after reading an Essay on the manifestations of mind to a small literary society. In that essay he attempted to trace the phenomena of sensation from the lowest to the highest forms of animated being, (one of the characteristic grasps we have just alluded to) and the question occurred to him, "Is man justified in slaughtering animals for his food, seeing that, by a beautifully organized structure, they are rendered exquisitely sensible both of pleasure and pain?" We give the result of this questioning in the author's own words:

> "As the subject appeared to me one of great interest, I determined to investigate it as fully as my time, talents, and opportunities would permit; and resolved to adopt practically whatsoever should appear to be the plain dictates of nature. Suffice it to say, that after carefully consulting the writings of Moses, traditionary records, comparative anatomy, physiology, chemistry, general history, and private experience, I arrived at the firm conviction that the flesh of animals is not only unnecessary, but decidedly prejudicial to man's health and well-being. I therefore at once discontinued it as an article of diet, and, notwithstanding the expressed fears and remonstrances of my friends, I persevered; and was soon rewarded with better health and more real enjoyment than I had experienced during many previous years.
>
> "Having derived incalculable advantages from a strict adherence to a fruit and farinaceous diet, and being fully satisfied (after a long and patient investigation of evidence), that it is a food well adapted to *all* constitutions, in *all* climates fit for the residence of man, I can no longer resist the importunity of my friends to make known to the public the result of my inquiries." (Preface, p. xi.)

Thus the public obtained the benefit of John Smith's reading and experience anent a fruit and farinaceous diet; and by this means it fell out that John Smith came under our critical notice. We will hastily follow him through his leading arguments.

It is first argued that fruits and farinacea are the *original* food of man: firstly, because the Mosaic history expressly states that "God said, Behold I have given you every herb bearing seed which is upon the face of all the earth, and every tree in the which is the fruit of a tree yielding seed; to you it shall be for meat." Secondly, because in the works of Greek and Latin writers there are frequent allusions to a period in which man lived in a state of innocence and happiness on the delicious fruits of the earth:

> ———————— "void of care and crime,
> The soft creation slept away their time.
> The teeming earth, yet guiltless of the plough,
> And unprovoked, did fruitful stores allow:
> Content with food which nature freely bred,
> On wildings and on strawberries they fed;
> Cornels and bramble-berries gave the rest,
> And falling acorns furnished out a feast."

This from Ovid, but similar testimony as to the dietetics of the original race of man, and also as to the amazing longevity of these frugivorous people, "is also afforded by Manetho, who wrote the Egyptian History; Berosus, who collected the Chaldean Monuments; Mochus, Hestiæus, Hieronymus the Egyptian, and those who composed the Phœnician History; also by Hecatæus, Hellanicus, Acusilaus, Ephorus, Nicolaus, Diodorus Siculus, Herodotus, Strabo, and Jerome of Egypt."

How long a fruit diet was persisted in, is a matter of question by our author. He thinks, however, that as God had commanded man to be fruitful and multiply and replenish the earth, he would, in obeying this command, be frequently placed in circumstances such that a supply of fruits and farinacea could not be obtained. As man was to have dominion over all animals in all climates, our author thinks it "consistent with all correct views of divine government to expect that he would receive such an organization from the Divine hand, as would render him capable of subsisting on the greatest variety of food—the productions of all climates, with full liberty to use all such as he might be induced by his instincts or reasoning faculties to adopt, as circumstances might require. The flesh of animals, therefore, could not be excepted; for in many climates no other food could be procured." These are Mr. Smith's own words, and yet after expressly stating that man has an organization suitable to the greatest variety of food, he very naively adds, "but we are not thence to infer that the digestive organs of man are the best adapted to an animal or even mixed diet."!

The arguments in the three succeeding chapters as to the original food of man, will be best given in the author's own summary:

"I have now completed my investigations respecting the original diet of man; and have, I trust, satisfactorily proved that the flesh of animals did not contribute to his support. The language of Scripture seems to me particularly clear and decisive on this point, showing that fruit and other vegetables were appropriated to the use of man. His original innocence and moral perfection speak the same

language; for the thought of creating pain and misery, by slaughtering an animal in the midst of pleasure and enjoyment, could arise in no breast whereon the image of the Creator was faithfully sealed, except in the case of dire necessity. The testimony of profane antiquity also is in favour of a simple vegetable diet, among the first ages of mankind. The senses of sight, smell, and taste, the instincts expressly designed by the Creator for directing each animal to its appropriate food, loudly proclaim man to have been originally frugivorous; while the absence of fire and other results of discovery would entirely preclude the first human inhabitants of this globe from feasting upon the flesh and blood of slaughtered animals." (p. 44.)

We need scarcely observe that the testimony of profane antiquity is no testimony at all; and that the facts of holy Scripture are not scientific data, and were not written to advance scientific truth. The *philosophy* of the origin of man is as yet involved in impenetrable obscurity, and there seems little probability that science will so advance that the facts recorded by the sacred writer will be satisfactorily explained and elucidated by mere scientific inquiries and speculations. We know certainly that the races of men mentioned in Scripture, and referred to by ancient writers, were Asiatics,—inhabitants of a warm climate,—to whom a fruit and farinaceous diet was alike the most acceptable, the most agreeable, and the best. If then the arguments drawn from these sources be at all valid, (which we however deny altogether,) they are applicable only to the dietetics of inhabitants within the tropics.

The second part is devoted to a series of dissertations, tending to show that fruits and farinacea are the *natural* food of man. These are founded on physiological data. The whole of this part consists of fallacies more or less glaring, which arise out of the indeterminate meaning attached to the term *natural*. This is nowhere defined, but our author and other writers of his class evidently refer to the mythological period termed the golden age, in which men lived in a state of nature. They were (to use a cant phrase,) "robed in innocence and purity;" that is to say, they went about stark naked, like the aborigines of Australia, or Terra del Fuego. They had no houses, but slept beneath "the shade of the forest glade," or perhaps lived like monkeys in the trees from which they derived their sustenance. They had no laws, and of course no law-courts; no wars, no manufactures, no amusements such as civilized man indulges in, no literature, no science. In short, they lived so unlike man as we know man, that they closely resembled the beasts of the field and of the forest. The Indians of California present probably a good example of the "style of living" in the golden age. Their lodges are somewhat like low haycocks, being composed of a framework of sticks thatched with bulrushes; their food consists principally of mussels from the river and of acorns from the forest. Both men and women are to be seen employed shelling, pounding, and baking them into bread; but Commander Wilkes, of the United States' navy, to whom we owe this information, remarks, that the pig-like filthiness with which the process is conducted excites disgust. Man in a state of nature is man in the lowest stage of his development; man in a state of civilization is man in the highest stage of development. From whence then should we take the true type of the genus *homo*? Must we look for it amongst the savages of Australia, or among the nobility and gentry of Great Britain? The folly of such views

is amply shown in the following passage, which our author adopts from a kindred writer:

"What is the natural dietetic character of man, according to the real and true evidence of comparative anatomy? In considering this question it is important that we should remember that, whatever may be true concerning the natural dietetic character of man, there is neither now on earth, nor has there been for many centuries, any portion of the human race, so far as we know, which have lived in all respects so perfectly in a state of nature, or in a state to which the constitutional nature of man is most perfectly adapted, as to afford an opportunity to study the true natural history of man, and learn his natural dietetic character from his natural dietetic habits: and therefore, so far as this question is anatomically considered, man must in strict propriety be regarded as an extinct species; because, though man is actually a living species of animal, yet the species, as a whole, have become so artificial in their dietetic habits, that it is impossible to derive from those habits any evidence which can justly be considered unquestionable, in relation to the natural dietetic character of man." (p. 56.)

The conclusion at which our author arrives, is that at which it might have been foretold that he would have arrived, from the premises previously stated. It is sufficiently humiliating:

"Comparative anatomy, therefore, warrants us in concluding that the alimentary organs of the orang are the true type with which to compare those of man, in order to ascertain his true dietetic character. Now, as the orang-outang and most species of monkeys, when in a pure state of nature, and when left free to choose their own food, and to follow their undepraved instincts, are wholly frugivorous—subsisting exclusively on fruits, nuts, and other esculent farinaceous vegetables,—we are perfectly justified by all the laws of correct reasoning in concluding, that the natural food of man is not of that mixed nature which many physiologists would have us believe." (p. 78.)

Professor Owen has, we believe, discovered a striking resemblance between the teeth of the Australian aborigines and the higher simiæ. We know of no race of men so much in "a state of nature" as the blacks of Australia, and we hand over the fact to our author as an additional proof of his views. Indeed, it appears from quotations made by Mr. Smith, that some of the simiæ, like the Australian naturals, cannot resist the temptation of roast beef and other unnatural luxuries.

The authority of celebrated authors is next adduced in support of the phytophagous doctrine, and it is for this reason that our previous remarks were made. Linnæus, Daubenton, who speaks of man in a "state of *pure* nature;" Gassendi, who refers to "the primeval and spotless constitution of our nature;" Sir Everard Home, who thought "while mankind remained in a state of innocence," their only food was of a vegetable origin; are quoted in succession with no other result on our minds, than the conclusion that they all fell in with the old popular mythology of mankind. Even Cuvier speaks of the 'natural' food of man; thus assuming the previous existence of a condition of the human race in which seed and fruit diet was the ordinary diet, and that man was created to use such diet only.

Mr. Smith is well aware of the ordinary objections that can be and are raised against his views, but he seems marvellously ignorant of the utter fallacy lurking under the expressions "natural," and "state of nature." Man differs from all others in being eminently progressive; his natural condition is one of progress; his natural dietetics are founded on a chang-

ing power of adaptability to the new circumstances in which his progressive development places him. This adaptability is certainly fully recognized by our author, but its practical results are lost by bending facts to his theory:

"Adapted by nature for feeding upon neither flesh nor herbage, he is (notwithstanding) created with an adaptability to either or both, as climate or circumstances may render necessary; but we are not justified in inferring that he enjoys, by this deviation from nature, that full share of health, pleasure, and longevity, which would be secured by a strict adherence to his more natural diet. If, therefore, we would judge correctly of organs and their functions, we must carefully distinguish between *adaptation* and *adaptability;* and must not hastily conclude that, because an animal can exist and be comparatively well upon a certain kind of diet, it was designed to live on that diet as its best and most natural food. Each animal has been organized upon fixed principles, and each organ has its determinate function and special adaptation; but an all-wise Creator has provided against emergencies, by conferring on each organ—particularly if connected with existence or with organic life—a considerable latitude, by which it can (to a certain extent) vary its functions without destroying its power, or so far impairing the constitution as suddenly to destroy life." (p. 89)

Here is at once the double assumption that there has been in man a deviation from nature, and that each animal has been organized upon fixed principles of adaptation; whereas, on the one hand we have no scientific knowledge whatever of man's "state of nature," and on the other the tribes, genera, and species of animals pass into each other so frequently, and often so imperceptibly, that adaptability is in fact the law, and appears to determine the generic and specific characteristics. It is by this law that surrounding circumstances act upon the senses and the organs necessary to secure food. At the call of certain common instincts and sensations, the powers of the animal are put into vigorous operation, under circumstances different from those by which itself or its parents had been surrounded. The repeated exercise of these powers develops the organs suitable to their exercise; what was at first strange and displeasing becomes habitual and pleasing, and at last the nature of the animal is adapted to the circumstances. As the circumstances of man with regard to climate, opportunity of obtaining food, clothing, &c., vary greatly, so do his 'nature' and his corporeal form vary; and consequently there is no fixed type or condition invariably applicable to all men.

But even the comparative development of the senses scarcely presents the grounds for Mr. Smith's argument, which he takes up. Having, for example, demonstrated from a consideration of the masticatory organs of the simiæ, that their food is that which is most adapted to the masticatory organs of man, we find him contradicting himself in the consideration of the senses. In man, Mr. Smith observes, we find this sense [the olfactory] placed in a closer relation with fruit than with any other article of diet—a mere assumption, by the by,—and yet in the possession of the maxillary sinus, he is much more allied to the goat and other ruminants than to the simiæ, in whom it is nearly obliterated.

The last argument Mr. Smith introduces in favour of vegetable diet as the natural food of man, is founded on his sensitive and moral feelings. Every manifestation of pain and suffering in a sensitive being, he observes, must at all times awaken the sympathies of the human heart, except in

those who are constitutionally obdurate, or whose feelings have been blunted by repeated acts or scenes of cruelty and misery.

"Can we suppose, then, that the Deity would have implanted in the human breast such an aversion to the taking of life, such a horror of shedding blood, and such a heart-sickness on witnessing it; such a hatred of cruelty, and such a sympathy with creatures writhing with pain, if he had intended us to feed upon the flesh of slaughtered animals? Would he not rather have formed us cruel and ferocious, like all carnivorous animals, which seem to derive pleasure from witnessing the sufferings of their victims? Or has the All-wise Creator departed from that harmony of design, so conspicuous in all his works, and rendered necessary for man's support a food, the procuring of which shall do violence to the best and kindliest feelings of his nature; shall be continually weakening and tending to exterminate the attributes of benevolence, mercy, and love; and gradually defacing the image in which God created him? Could he intend that the human race should eat their food with compunction, that every morsel should be purchased with a pang, and every meal empoisoned with remorse? No! Consistency runs through all the works and designs of God!" (p. 114.)

The designs of God have made it requisite that all animals shall destroy each other. Even the herbivorous trample down, crush, and masticate hundreds of insects, while whole nations of men are hunters and fishermen. Indeed, man is a hunting animal; the love of the chase is one of his strongest instincts. So much for the facts on which Mr. Smith grounds his argument. On the other hand, carnivorous animals display warm sympathy for pain and suffering. All persons conversant with dogs know that even the expression of pain on the countenance of those with whom they associate is sufficient to awaken their sympathy, while the cries of suffering will distress the faithful animals exceedingly. In short, the whole argument is merely a display of an effeminancy of sentiment, which would be contemptible were it not well-meaning.

Our Pythagorean having, as he imagines, proved "that vegetables were the original, and are now (as well as in former ages) the natural food of man," he next attempts to demonstrate that they are the best food of man. His arguments are derived from various sources. Firstly, he devotes a chapter to the consideration of the chemistry of dietetics, with a view to show that vegetables contain all the elements and qualities necessary for the complete nutrition of man; that they are easy of digestion, and that "they are superior to animal food or a mixed diet for maintaining all the vital processes; for producing the *mens sana in corpore sano*, in the greatest perfection and for the longest period." The whole chapter shows considerable and praiseworthy research, but the conclusion is lame. It is this:

"Organic chemistry, however, has not yet been brought to such perfection as will enable us to mete out man's food by its laws. We have yet much to learn in this respect; and a short notice of the subject is introduced here only to show that from the vegetable kingdom may be selected, for human food, such articles as will bear a comparison with a mixed diet so far as our present knowledge will permit us to judge; and that the light already thrown upon the matter by chemistry, is sufficient to prove that fruits, grain, roots, and other esculent vegetables, if used in a natural, unrefined, and unconcentrated state, contain every principle necessary for the nourishment of man." (p. 163.)

The next chapter is occupied by proofs that the experience of nations and individuals is in favour of a vegetable diet as the best for man.

Pythagoras heads a list comprising the names of Plutarch, Porphyry, Haller, Ritson, Cheyne, Lambe, Newton, (not Sir Isaac, but one who wrote a work entitled 'Return to Nature,') Shelley, Hufeland, Sir Richard Phillips, sundry Americans, St. Matthew, the three most ancient orders of priests, the Brahmins, Magi, and Druids; and also the Athenian prince Triptolemus, who established the 'Eleusinian mysteries.' The maxims and practice of these persons are set forth at length in the next chapter in proof that the use of fruits and farinacea is conducive to health. Numerous nations in every clime and continent are also referred to in detail, as living almost or altogether on vegetables, and yet being strong, active, and remarkable for their longevity. Our author's style of argument and mode of treating his subject may be gathered from the following extract:

"Examples might be multiplied, from all parts of the world, of people living entirely upon vegetable food, and enjoying perfect health and bodily vigour: but perhaps none are more striking than those we have in close proximity to us. 'The chairmen, porters, and coalheavers, the strongest men in the British dominions, are said to be (the greater part of them) from the lowest rank of people in Ireland, which are generally fed with the potato. No food can afford a more decisive proof of its nourishing quality, or of its being peculiarly suitable to the health of the human constitution.' This remark has been amply confirmed by the recent experiments of Professor Forbes, on the weight, height, and strength of above eight hundred individuals,—his tables clearly showing that the Irish are more developed than the Scotch at a given age, and the English less. The Rev. Howard Malcolm, of Boston, who has travelled extensively in Europe, Asia, and America, says: 'The finest specimens of the human body I ever beheld I saw in Ireland, and they had never tasted animal food.' Many English farmers, who have been in the habit of employing the natives of the emerald isle, bear testimony to the fact, that those who are steady and refrain from spirituous liquors, are indefatigable, and are capable of performing a much greater amount of agricultural labour, on their simple meal of potatoes and butter-milk, than the English labourer, though feeding on abundance of flesh-meat." (p. 222.)

A chapter is headed "Influence of azotized food in the production of certain diseases." The comment is not, however, suitable to the text, as in fact Mr. Smith's arguments are really directed against an *intemperate* use of animal food. Of the baneful effect of such intemperance there can be no doubt whatever. The following paragraph contains so much common sense, that one cannot but feel a little surprise at the frequent aberrations from a sound judgment manifested by the author:

"Without entering further into the nature of disease or its causes, we may show: 1. That too stimulating a diet, or one that is unnatural in quality or quantity, is a very general cause of functional disorder. 2. That an abstemious diet of fruit, grain, and other farinaceous vegetables is, in general, the surest means of restoration to health. Let it however be clearly understood, that improper food is not considered the only means of introducing disease; inattention to exercise, pure air, cleanliness, the cutaneous and other excretions, together with a number of acquired and unnatural habits, may be equally effective in destroying health; and a man who lives temperately upon a mixed diet of animal and vegetable food, and is at the same time regular in other sanitary habits, will enjoy a far greater share of health, and be less liable to epidemic diseases, than the man who adheres to a vegetable diet but neglects all other physiological laws." (p. 239.)

There can be no doubt that the intemperate use of flesh meat induces

(as our author argues) gouty and urinary diseases. It is certain also that there are individuals (as, for example, with a highly arthritic constitution, or who have already been gluttonous) for whom a strictly vegetable diet, may be prescribed with the best result. We think it, however, scarcely warrantable to infer, that a mixed animal diet in moderation induces disease, or to propose that all mankind shall adopt a strictly vegetable diet, because examples of this kind have occasionally occurred. It seems to us much more reasonable to infer, that the exceptions prove the rule. Nor does it follow, that because the flesh of diseased animals is injurious, that flesh must necessarily not be eaten. Fruits and farinaceous vegetables may also be poisonous when diseased. Whole districts sometimes suffer in Germany and other parts of the continent, where rye bread is the ordinary food of the people, from the mixture of ergotted rye. Mr. Smith would, we imagine, think it monstrously unfair to interdict the use of rye bread on this account; and yet this is precisely one of the arguments on which he rejects the use of animal food.

Another reason why animal food should be interdicted is, that it conduces to caries of the teeth. As an example of Mr. Smith's loose mode of argument we quote the following:—

"An intelligent sea-captain, who had visited most parts of our globe, informed Mr. Graham that he found those people who used hot liquids, and hot food, and smoked tobacco and other narcotic substances, always had black and much decayed teeth; but that in the islands of the Pacific, and other parts, where the people seldom or never take anything hot into their mouths, use little or no animal food, and are very simple, plain, and natural in their diet; they had very regular teeth—white, clean, and free from decay. In Mexico the higher classes consume great quantities of animal food, generally eating it three times a day; and they are noted for the early decay of their teeth, and for nervous complaints; whereas the Indians, residing in the same locality, but who live on vegetable produce, are remarkable for their fine white teeth, for their mild expression of countenance, and for their general good health." (p. 273.)

The fact is, that the higher classes in Mexico, of both sexes, *are inveterate smokers*, a reason quite sufficient of itself for the bad state of their teeth. Young Mexican ladies esteem it as much à la mode to smoke cigars as do our English dandies.

A chapter details numerous examples of the beneficial effects of vegetable food on invalids, and will be read with interest. The case of Dr. Lambe, one of the Elects of the College of Physicians, is worthy record:

"In a letter dated April 16, 1825, (?) Dr. Lambe writes as follows:—"From the age of nineteen to thirty-five, I was constantly suffering from the usual symptoms of dyspepsia, which toward the latter period were accompanied by a constant and oppressive pain about the stomach. At the age of thirty-five I had an attack of enteritis, which was severe enough to require two venesections; after this I never went out in the damp of the evening without feeling some tenderness over the abdomen. Under these circumstances, together with a general feebleness of health, I determined to try the effect of substituting distilled water for common water as my drink. The effect of this change was a thorough relief of the dyspeptic pains, and abdominal tenderness. In the ensuing three years, a headache, from which I had occasionally suffered earlier in life, returned so frequently and so severely, as to induce me to take active measures for its relief. I then determined to abstain from animal food, as well as from

the use of common water. The intensity of the paroxysms was instantly relieved; yet they recurred in a mitigated form, for at least thirty years. I have been engaged in the active duties of my profession until the middle of last year, which was the eightieth year of my life. Since then, from a partial failure in my sight, I have retired into the country, where, making allowance for my time of life, I enjoy a good share of health." (p. 281.)

We believe Mr. Smith's printer has here substituted 1825 for 1845; as Dr. Lambe's age is certainly nearer 80 than 100. But even if the date were correct, the example of such a Pythagorean centenarian would avail more if we did not occasionally find inveterate smokers and drinkers attain to a longevity not much inferior to this. It is a remarkable circumstance too, developed by the recent sanitary inquiries, that persons attain to an extreme old age, who have been immersed amongst agencies extremely noxious to health; some having been exposed to these all their life; some taking up their residence in the unhealthy localities after middle life.

Mr. Smith next argues that a vegetable diet is protective against epidemics. His facts are of the same one-sided character as those we have previously criticised, or are stated with a carelessness that is scarcely excusable. In opposition to his assertions, we may observe that whole tribes of Indians in Mexico have been swept off by epidemics. The exemption of the negroes of the West Indies from yellow fever is confidently attributed to their use of a vegetable diet! The same remarks apply to the facts adduced in proof that a vegetable diet is conducive to personal symmetry; to the acuteness and perfection of the organs of special sense; and to real sensual pleasures and enjoyment. The case of Casper Hauser, who was fed in a dark dungeon on bread and water, is quoted as an example of the perfection of the senses induced by a vegetable diet; as if the confinement in darkness had no effect in developing the acuteness of vision, &c. observed in that individual. So also in the case of nations remarkable for their physical conformation, no account is taken of the race from which they have sprung, or of their habits. On the other hand, many of the North-American tribes, living almost exclusively on flesh, are remarkable for the fine symmetry of their persons; yet our author takes special care not to allude to them.

The recommendation of a vegetable diet would not be complete, unless it were accompanied by proofs that such a diet was favorable to the government of the passions and propensities, and to the development of man's moral power. These proofs Mr. Smith boldly undertakes to give, and they are like those that have gone before:

"In general," he observes, "those nations and individuals who indulge much in flesh-meat, are more licentious, ferocious, and cruel, than those who subsist on a less stimulating diet; and men noted for barbarity and violence, have an irresistible penchant for animal food, while those who are blessed with milder dispositions, and more benevolent feelings, seem instinctively to adopt a vegetable diet."

This easy assumption of false facts is really amusing. The southern French and Spaniards are by no means remarkable for their gentleness: the latter form probably the most bloodthirsty nation in Europe, yet little flesh do they eat in comparison with the inhabitants of the British isles, more especially the English. In fact, Mr. Smith makes small ac-

count of climate, or race, or laws. The Hindoos are gentle, because they abstain from flesh, the carnivora are fierce because they eat flesh, not because they *must* eat flesh, and so on with a hundred examples.

The sixteenth chapter, headed "Diet considered in its relation to population and the moral progress of man," is the most curious in the book. The following extract will show its bearing:

"In the United Kingdom of Great Britain and Ireland there are at present about twenty-eight millions of inhabitants, and about double that number of acres of land in cultivation; consequently, two acres for each individual. Were all living on a full animal diet, the land could only supply food for five millions six hundred thousand inhabitants; on the greatest delicacies of fruits, grain, and roots, one hundred and twelve millions; on grain and other vegetables (where, according to Lord Lauderdale, one acre will support four persons,) two hundred and twenty-four millions; on potatoes and common fruit, five hundred and sixty millions;—without including the extra produce from improved culture. Or let us suppose that, in Great Britain and Ireland, there are (in round numbers) eighty millions of acres, of which sixty millions are arable, or capable of being cultivated. Let half of these be appropriated to the production of the finest fruits, flowers, and timber; and to the support of cattle, sheep, and other animals, for the production of milk, wool, &c.; we shall then have thirty millions of acres for potatoes, wheat, and other grain. Let one half of this remnant be sown with wheat, and the remaining fifteen millions planted with potatoes: then—

15,000,000 of acres of wheat, at 3-qrs. per acre, will feed	45,000,000	inhabitants,
15,000,000 of acres of potatoes, at 10 persons per acre,	150,000,000	ditto
Total . .	195,000,000	

Which is equal to seven times the present population, and more than thirty times the number that the land would support on flesh alone, without taking into consideration the produce of the thirty millions of acres appropriated to fruit and other delicacies. Many useless trees now stand on hedge-rows by the side of common roads; if these were replaced by varieties of apple and pear trees, not only would they be more ornamental, but the owner and the hungry traveller would be supplied with many delightful repasts." (pp. 391-9.)

From this it appears that the euthanasia of the United Kingdom will be consequent on the general adoption of a vegetable diet: that when we have "returned to a state of nature," and live on fruits and farinacea, we shall be a warm snug family of one hundred and ninety-five millions, each living to be as old as the pythagorean centenarian of the College of Physicians. A slight allowance ought have been made for the ground their houses would occupy. If this argument be not sufficiently attractive, Mr. Smith holds out his terrors. Mankind will be suffocated at some future period, unless it take incontinently to fruits and farinacea.

"I may here briefly notice another reason for supposing that man will, in future ages, have recourse to a vegetable diet; though it refers to a period so distant, that it will be deemed worthy of little attention. It is a well ascertained fact, that while plants decompose the carbonic acid contained in the air, and liberate the oxygen, all animals (except the microscopic animalcules) consume the oxygen, and restore the carbonic acid to the atmosphere. Combustion also diminishes the oxygen, and increases the amount of carbonic acid. Now, in proportion as animals multiply, and vegetation decreases, the constitution of the atmosphere must be altered, and rendered less fit for the respiration of man. But it has been shown that, on vegetable food, man requires less oxygen than on animal diet; therefore, by increasing the growth of vegetables for his

food, and contracting the numbers of other animals, he preserves the purity of the atmosphere for an increasing human population, and for the continued existence of his species. An *exclusively* animal diet for man, however, is not advocated by any person; and the calculations are only introduced here for the sake of a comparison." (p. 395.)

Our descendants will certainly want all the fresh air they can get, for if neither wars nor disease interfere with the law of increase of the population for the next two hundred and fifty years, Mr. Smith observes that "eight hundred and ninety-six millions will undoubtedly be the population of Great Britain and Ireland," just about the whole population of the globe!

In conclusion, we have to remark that Mr. Smith believes the millenium is not an improbability, and concludes "that all will then resort to a fruit and farinaceous diet, which is also best adapted to all the wants of the human economy."

"No longer now
He slays the lamb that looks him in the face,
And horribly devours his mangled flesh"

Although we shall still enjoy a quarter of lamb, we wish Mr. Smith all health and happiness on his fare of fruit and farinacea. He writes like a modest, and well-meaning man, and we are sure his book will be useful to wine-bibbers, gluttons, and riotous eaters of flesh. If such wish to know their folly, and to learn wisdom, let them read Mr. Smith's amusing volume.

ART. XI.

Traité philosophique et clinique d'Ophthalmologie, basé sur les principes de la Thérapeutique Dynamique. Par M. F. ROGNETTA, Docteur en Médecine et en Chirurgie, &c. &c.

A Philosophical and Clinical Treatise on Ophthalmology, based on the principles of the Dynamic Therapeutics. By F. ROGNETTA, M.D. *Paris,* 1844. 8vo, pp. 724.

THIS is a second and enlarged edition of Dr. Rognetta's 'Cours d'Ophthalmologie,' published in 1839, and forms a complete systematic treatise on the disorders of the eye and its appendages, in which the author does not give merely the results of his own experience, but analyses and criticises the writings of his predecessors in the same field. Without losing sight of the grand object of such a work, the rendering it as practically useful as possible, Dr. Rognetta has by no means evaded the discussion of disputed points in pathology and practice. On the contrary, he presents us with a lively and interesting account of the doctrines of all the celebrated ophthalmologists of the present age; and when he states his own opinion, assigns his reason for their adoption with a clearness and frankness which are peculiarly pleasing. We have no hesitation in recommending the work, as being not only one of the most minute, but also one of the most readable books on eye-diseases which we have met with, containing a vast amount of information, collected with great pains, put together in an honest, manly manner, and written in a clear and agreeable style.

As for the claim which Dr. Rognetta sets up for his book, as being

"philosophical and clinical," we cannot say so much. The school of Rasori, to which the Doctor belongs, seem one and all to fancy themselves philosophers. We might let this pass; but to assume the title "philosophical" as peculiarly appropriate to their system, and as distinguishing it from all other medical systems, like the assumption of "physiological" by the school of Broussais, is more apt to make them appear ridiculous than to excite respect. Neither can we admit Dr. Rognetta's work as a clinical one, any more than other ophthalmological treatises in which numerous cases are detailed, but these chiefly borrowed from the writings of other authors, few of them personally and publicly treated by him, who, collecting them from various sources, illustrates by their aid the different topics which comes before him.

The chief peculiarity by which the work of Dr. Rognetta is distinguished is, that the author is a follower of the school of Rasori and Giacomini, from the pathological and therapeutical principles of which he professes never to swerve.

Our readers are sufficiently aware, that the doctrine of Rasori differs from the Brownonian chiefly in this, that while Brown considered every influence and substance in nature, capable of acting on the living frame, as a stimulant, Rasori supposes a numerous class of substances to act as direct sedatives, and they serve to depress the morbid excitement which may happen to exist in the organs of the body. Remedies, then, of this class Rasori calls *contro-stimulants*. Contrary to Brown, he regarded all diseases as sthenic; but from this doctrine Giacomini has departed, admitting diseases of debility, and in this he is followed by Rognetta, who refers (p.8,) to the ophthalmoid as examples of the sthenic class of diseases, in which "the morbid state of the living tissues is such that their vital force, their sensibility, their dynamism, in a word, is exalted," while as examples of the asthenic class he refers to certain of the amauroses, met with in those exposed to certain poisonous influences, such as the vapours of lead, mercury, &c., or debilitated by chronic hemorrhagies, the abuse of certain remedies, and the like,—causes which act upon the whole economy, and produce a morbid condition, which, like the former, is entirely functional or vital, and consists in a deficiency of stimulus.

By way of introduction, Dr. Rognetta gives a sketch of what may be termed *ophthalmic therapeutics*. As this is the most original part of his work, and contains numerous statements well deserving of attention, it will be chiefly to it that our remarks shall be directed.

He begins by observing, that since the time of Broussais, attention has been much concentrated on the study of the character of diseases. Hence the art of diagnosis has made immense progress, while the science which treats of those phenomena which follow the administration of medicines, and precede the restoration of health, has been almost completely neglected. A belief seemed to have gained ground, (but the remark must be understood as applying chiefly to France,) that the whole science of medicine consisted in pathology, and that therapeutics should follow pathological knowledge as an immediate corollary, which it would be sufficient to announce purely in a general way. To understand thoroughly the action of medicines was regarded as superfluous, and, with the exception of bleeding, abstinence, some few emollients, and certain substances regarded as

possessing a calming property, medical applications were abjured as irritants and incentives. To judge of the action of medicines fell, as an accessory province, into the hands of mere naturalists and chemists. The former Dr. Rognetta accuses of minding only the local effects of medicines, and excluding from consideration the influence which they exercise after their passage into the circulation; the latter, of neglecting the dynamic action of medicines, their action, that is to say, on the vital powers, and of too often supposing the living body to resemble the retorts and other apparatus of a chemical laboratory.

"To judge," exclaims he, "of the action of medicines, we must study their dynamic effects, according to the changes which they produce in the functions, and not according to their local effects, which can conduct only to false conclusions—a study new to France, imported from Italy by M. Mojon and myself, in our translation of the important work of M. Giacomini.* In a dynamic point of view, the knowledge of the therapeutical action of substances is altogther independent of chemistry, and belongs to the domain of physiological observation and of clinical experience. Chemistry has to do merely with the preparation of medicines, not with their action." (p. 14.)

Dr. Rognetta proceeds, in chapters fifth and sixth, to a review of the general and local medications used in the cure of the diseases of the eye. The former, he says, have been classed chiefly under three heads, viz. tonics, antiphlogistics, and revulsives; while others have been considered as specifics. This last class, as well as the three former, he merges into one or other of the two divisions under which he comprehends all ophthalmic medications, viz. hypersthenisants and hyposthenisants, in other words, stimulants and counter-stimulants, or excitants and sedatives.

1. Under the head of *tonics* or *excitants*, Dr. Rognetta throws out a number of objections to the vulgar use of these terms, and to their indiscriminate application not merely to wine, brandy, and the like, but also to the acids, bark, nux vomica, gentian, iron, and zinc, substances which have few common properties to bring them under one class.

"As the strength of the body depends on the integrity of the functions, whatever remedy re-establishes these in the healthy condition may be called a tonic. Bleeding will be a tonic, if it removes a pneumonia or an ophthalmia, and thus restores the lungs or the eye to its state of natural strength; and the same with tartar emetic, and a crowd of other antiphlogistic remedies. The patient is weak, it is remarked of one affected with scrofulous corneitis or chlorotic amaurosis; he requires tonics. He is weak, but why is he weak? Undoubtedly from the same cause that he is ill. Now, it is not by administering stimulants to him that his strength will be restored, but by re-establishing his organic functions; and if iron, or iodine, or bark attain this object, surely it is not by exciting the system.

"There are very few substances really capable of raising the rhythm of the functions, or rather the organic force, above the normal type, or above the level at which it may be at the time. They are alcoholics, opium, cinnamon, and the others. These are true excitants, but not tonics.

"The term *tonic* ought to be abandoned, or employed only in a wide sense, to signify a means capable of re-establishing the functions. Such a means is not always an excitant, nor are excitants always tonics. Thus, if alcohol, wine, or cinnamon be given to an ophthalmic patient, weak from scrofula, chlorosis, or intermittent fever, he will be rendered still weaker; the vital force, no doubt, or the organic sensibility will be raised above its former level, but, at the same time,

* Traité Philosophique et Expérimental de Matière Médicale et de Thérapeutique.

the normality of the functions, upon which strength depends, will not be restored." (pp. 17-19.)

Bitter substances are accounted tonics, as bark, coffee, hops, chicory, burdock, chamomille, bitter almonds, orange peel, angelica, &c., but the assertion, according to Dr. Rognetta, is altogether arbitrary and false. Far from being excitants, these substances are hyposthenisants, not because they are bitter, but because experience has shown this to be their dynamic action. Strange to tell, the only bitter substances the action of which is really excitant, such as opium, are not placed under this category. The reader must not, then, be surprised, when he finds the bitters prescribed by our author with a different intention from that which is usually assigned to them, or finds condemned those combinations of substances of opposite actions, the mixtures of which must become either totally inert, or of very equivocal efficacy; such as that of calomel with opium, opium with tartar emetic, bark infused in wine, camphor dissolved in alcohol, and the like.

Dr. Rognetta observes that it has been thought that the nature of the dynamic action of a medicine might be changed, by administering it in a larger or smaller dose; that calomel and the acids, for example, might be antiphlogistics in a small dose, and excitants in a large one, and *vice versa* for other medicines. This he regards as an error. So long as the composition of the medicine is not changed, its action cannot be fundamentally different—merely the degree of its energy will vary with the dose. The primitive intrinsic action is invariable also, whatever be the nature of the disease for which the drug is prescribed.

We think our readers will agree with us, that in his remarks on tonics, of which the above extracts and abstract afford a fair sample, there is more truth than novelty, and more puerility than truth. At the same time, the obvious conclusion to be drawn, we think, from the facts stated under this head by Dr. Rognetta, is in favour of more numerous and more minute subdivisions of the materia medica than those which he admits.

2. There are a few affections of the eye, observes Dr. Rognetta, which do not claim the aid of *hyposthenisant* or *antiphlogistic* medication. Its object is to diminish or destroy the morbid excess of organic force, and thus to re-establish the functions. It consists of several elements, whereof some are mechanical, and act indirectly upon the dynamism, that is to say, by removing mechanically a certain quantity of stimulus, of blood, heat, electricity, &c., by depletion, abstinence, bathing, cold applications, &c. The others are dynamics, and act directly by assimilation upon the functional force, or rather upon the ganglionic system which represents it; they lower the degree of this force, and if it be above the normal type, they bring it back to its natural point, and re-establish the functions. If their use is still continued, the vitality is brought still lower, and the symptoms are manifested of true asthenia. These last means offer the advantage of modifying directly the erethism of the organs, without taking away any of the substance; it is thus the counter-stimulants act, of which great use is made in the hypersthenic affections of the eye, (tartar emetic, nitrate of potass, calomel, belladonna, sulphate of magnesia, &c.) If there is a plethora, these means alone will be insufficient, for they combat only the dynamic condition, leaving the congestion, which, although now passive, is sufficient to prevent the resolution of the disease, and even to reproduce

the dynamic phenomena which the remedies had succeeded in dissipating. It is always, therefore, with evacuations of blood that the antiphlogistic treatment of acute affections of the eye should commence, if attended with plethora. A hypersthenic disease may exist without this last circumstance, and then bleeding would be useless, if not hurtful; such is the case in certain scrofulous, chlorotic, and syphilitic diseases of the eye.

In these circumstances, the disease resists bleeding, although its nature is one of excitation, and it often yields, as if by enchantment, under the influence of direct hyposthenisants, as iodine, iron, mercury, secale cornutum, nitrate of potass, &c. It is easy to understand, then, how bloodletting, which in general is useful in inflammatory diseases, may prove inefficacious in certain inflammations, without the aid of certain direct hyposthenisants. Purulent ophthalmia, retinitis, iritis, choroiditis, are examples. In such cases, it is a common practice to prescribe hyposthenisants in large doses immediately after bleeding, which does not prevent a recurrence several times to this remedy, if needs be. The remedies in question answer so much the better after bloodletting, that they keep up, prolong, and augment its good effects.

Such are our author's views respecting antiphlogistics, or hyposthenisants.

3. Theoretically, by *revulsion* is understood an artificial action on some point more or less remote from the part diseased, with the view of displacing or causing the disappearance of the disease. This action consists sometimes in an artificial disease, such as a burn with a red-hot iron, boiling water, the potential cautery, a blister, urtication, an eruptive ointment, &c.; sometimes in the use of medicines called evacuants, and of which the object is to promote secretions, or to draw off a quantity, more or less copious, of the animal fluids. The meaning of all this is, that we can, by the aid of those means, displace a disease—make it pass from the inside to the outside, from the upper regions of the body to the lower, from the lungs to the skin of the arm or leg, from the head to the intestines, from the eyes to the nape or the arms.

Dr. Rognetta remarks, that a slight reflection will show that, in principle, the mode of reasoning is no other than that of the humoral pathology. He rejects, then, the revulsive theory, and ridicules with justice the notion of revulsive bloodlettings, while he admits the utility of bloodletting as a mechanical means, diminishing the quantity of blood, and, as a dynamical, its indirect action being to diminish the stimulus of the circulation. Neither has he any greater respect for derivatives, such as cupping, the warm bath, rubefacients and vesicatories, as derivatives. Cupping acts exactly as venesection, the warm bath deprives the body of part of its caloric, while blisters act, he thinks, by being absorbed. Locally, he says, the action of a blister is insignificant. But the cantharides are absorbed, they act on the dynamism of the disordered organ, they hyposthenize it, and hence the antiphlogistic effect.—Dr. Rognetta holds, that an issue does not act by revulsion. But the continued loss of blood by means of the purulent discharge, weakens the system, and produces a hyposthenizing effect.

Purgatives, according to our author, act only by being absorbed, and benefit not so much by their effect as evacuants as by their directly sedative influence. That they will act by being absorbed, as when castor oil is rubbed into the skin, we do not deny, but we conceive it to be beyond a

doubt that they purge, not merely from their being taken up from the bowels and conveyed into the tide of the blood, but from their immediate action on the intestinal mucous membrane, and hence on the muscular coat.

4. Dr. Rognetta includes all medical applications made to the eyes themselves under the name of *collyria*, and staggers us at the outset by advancing the dogma, that all of them act only by being absorbed, exactly as medicines do when applied to the internal surface of the stomach. Some of them, however, he admits to act mechanically, or even chemically, such as cold water, compression, caustics. Always, and even in the cases now referred to, a dynamic effect is to be recognized; for potential caustics do not limit their effects to mere burning, but are partly absorbed, and act dynamically. Even cold water, while it subtracts heat, determines a dynamic effect. From these data, he draws the conclusion, that the efficaciousness of a collyrium is in proportion to its being absorbed, or passing into the circulation of the eye.

The doctor exclaims against combining a number of ingredients together in a collyrium, probably of opposite medicinal characters, or which upon being combined produce something opposite in the effects from what is intended. He instances sulphate of zinc and opium, acetate of lead and camphorated spirit of wine, acetic acid and spirit of wine, which being combinations of stimulants and sedatives, produce compounds whose actions are either null, or opposed to what is intended. We perfectly agree with Dr. Rognetta, that collyria should be as simple as possible. He arranges collyria under six heads: viz. 1, gaseous; 2, fluid; 3, soft or unctuous; 4, dry or pulverulent; 5, metallic, which contain either silver, mercury, copper, lead, zinc, alum, hydrocyanic acid, nitre, or common salt; 6, vegetable and animal. What hydrocyanic acid has to do among the metals we cannot divine.

Along with many useful hints in his discussion of these various local applications to the eyes, we encounter not a few prejudices and absurdities; such as, that a warm collyrium will do harm in the greater number of cases—that the application of fluid collyria by means of a camel-hair pencil is painful, from the mechanical effect of the instrument—and that a better way is to cover the finger with a bit of rag, dip it into the collyrium, carry it gently along the inside of the eyelids, and then leaving the rag between the eyelids and in contact with the eye!

With respect to the metallic collyria, he recommends them to be used in the fluid forms, and very weak, so that they may be the more readily absorbed. For instance, he limits the collyrium of nitrate of silver to three or four grains of the salt in an ounce of water, but recommends chiefly from one to three grains, the solution being used as a fomentation. In this degree of strength, he asserts it is absorbed by the eye, and by this means acts beneficially, not in mere conjunctivitis only, but in diseases of the choroid, and even of the retina. In chronic ophthalmia, his practice is, to agitate a stick of lunar caustic for a few instants in a basin of water, till it becomes a little turbid, and to let the patient use this as a lotion for the eyes and face with a sponge. He states this to be attended with great advantage. In like manner with regard to sulphate of copper in cases of chronic blepharitis, he pours a few drops of a concentrated solution of it into a basin of water, and with this directs the patient to bathe the eyelids

and the face, morning and evening, thus employing a sort of mineral water, the quantity of which makes up for the weakness of the saline ingredient. The absorption of it, the doctor thinks, is therefore easily effected, while if strong, it could not be tolerated, and could do no good. The weak solutions act upon all the vessels of the face, contributing to the vascularity of the eyelids and conjunctiva, and prove highly beneficial.

Dr. Rognetta, on the whole, seems well acquainted with chemistry; but sometimes makes a slip, as when he tells us, (p. 41,) that in blue ointment the mercury is oxidized. He might have learned long ago from Orfila, that it is merely mechanically subdivided, with scarcely a trace of oxide.

Corrosive sublimate, he recommends, not only in solution, but in salve, one part of the salt to be dissolved in distilled water and mixed with sixteen parts of fatty matter. This, we conceive, would be too irritating to be applied to the eye.

It is somewhat surprising, that Dr. Rognetta does not seem to be aware of the danger of using saturnine applications to the eyes.

He recommends highly as a collyrium, or drop for the eye, a mixture of equal parts of a saturated solution of alum, and a saturated solution of sulphate of zinc. This, it seems, has succeeded well in the hands of M. Clot-Bey, in the treatment of purulent ophthalmia in Egypt.

A bread and milk poultice, moistened on the surface with a strong solution of nitre, is highly recommended by Dr. Rognetta as a sedative in cases of ophthalmia, attended with pain and heat.

We regret he seems so little aware of the admirable effects of belladonna, employed in the form of a collyrium, doing little more than naming it along with the other vegetable substances. In our experience, few cases of scrofulous ophthalmia have resisted the careful use of a belladonna collyrium, and in many other varieties of inflammation of the eyes it proves scarcely less useful.

We have thus given a pretty full account of Dr. Rognetta's therapeutical principles, because, as we have already hinted, it is in these, much more than in his description of diseases, that his work differs from other ophthalmological treatises. Were we called upon to sum up in a few general conclusions the substance of his therapeutics, we fear one of these conclusions would be this, that in eye-practice, not less than in the treatment of other organs, many—many remedies, of the action of which we know nothing, are every day employed against diseases, the nature of which is equally unknown. This is a truth which, however humiliating, should never damp in the smallest degree the zeal of the honest and observant practitioner of the healing art. The *modus operandi* of medicines is almost wholly unknown—the fact of their being beneficial in many cases cannot be a matter of doubt to any man the eyes of whose understanding are opened, and blinded neither by prejudice nor self-conceit. To throw medicines aside because we cannot explain the mode in which they act, would be the height of folly, and almost as foolish is the attempt to arrange them within a very limited number of artificial divisions, such as those adopted by the author before us. We should much rather have no classification of remedies at all than that which arranges them into the two divisions of excitants and sedatives.—We should much rather call quina a specific, mercury a specific, sulphur a specific, iodine a specific, for the several diseases

which these remedies are known to cure, because we should thereby declare a fact, which we should not do by calling any of them an excitant or a sedative.

What is the grand practical result of the revolution in the classification of remedies, attempted by Giacomini? Almost nothing. We find Dr. Rognetta raising his voice against putting a little alcohol into a lead collyrium, because, forsooth, the alcohol is a stimulant, and would undo the sedative effect of the lead—or of combining opium with calomel, in iritis, because the hypersthenisant properties of the opium would counteract the hypersthenisant ones of the calomel. But this is all. The same remedies essentially are employed by our author as are used by other practitioners, or if his new therapeutics lead him to any practice very different from the common, it is to some such absurdity as attempting to cure iritis and other internal ophthalmiæ by atropism, (p. 242,) that is to say, by poisoning the patient with belladonna—a prank supremely useless, even when it does not prove dangerous.

The division of all diseases into sthenic and asthenic, and of all remedies into hypersthenisant and hyposthenisant, is absurd, and fit to take its place only in the childish brain of some quixotical water-curer, who, holding "the leading paramount indication in the treatment of any given disease, to be either to depress excited action, or to exalt depressed action," tells us, that "one single remedy, capable of being graduated in its doses, so as to exercise every degree of sedation or stimulation respectively, is calculated, under favorable circumstances, to operate against the whole host of maladies, and to supersede, or be backed against, the whole list of medicines,"* which remedy is water.

Barring his theoretical notions about the action of remedies, we can scarcely give Dr. Rognetta too much credit for his book, which, as we stated at the outset, is both well written, and rich in characteristic descriptions of disease, full of valuable facts, and rendered interesting by the reasonings of an acute and lively mind. With one short extract, explanatory of Dr. Rognetta's mode of regarding some of the most important disorders of the eye, we shall take our leave of him.

"The apparatus of vision may be considered as a direct emanation, a prolongation of the brain. In an anatomical point of view, the eye is, in fact, a sort of little brain. Like the encephalon, it presents a protecting osseous case (orbit), completed by a fibro-membranous apparatus (eyelids). Like the brain, it has a fibrous envelop (sclerotica), another which is vascular and serous (choroid), and lastly, an essential organic part, which is medullary or nervous (retina). Let us observe this portion of the cerebral substance, which is continued within the sheath of the optic nerve, and expands over the bottom of the eye; this considerable artery, which penetrates it (arteria centralis), and which imitates the basilar artery of the brain; this prodigious quantity of nerves which surround the eye, and the branches of which enter into its interior; and, lastly, this magnificent tree of arterial vessels, which bathe with blood the eye, the orbit, and all the neighbouring parts, as the carotid does the parts to which it is distributed; all this leads naturally to the conclusion, that one of the principal sources of the diseases of the eye is in the brain itself, or at least in the constituent elements of the eye, coming from the interior of the cranium. Thus it is that I see, in a great number of these diseases, affections depending on a certain morbid state of the encephalon, and I can every day congratulate myself on the practical results accruing from this manner of contemplating my subject." (p. 1.)

* Balbirnie's Philosophy of the Water-Cure, p. 100.

ART. XII.

A View of the Formation, Discipline, and Economy of Armies. By the late ROBERT JACKSON, M.D., Inspector-General of Army Hospitals. *The third edition, revised, with a Memoir of his Life and Services, drawn up from his own papers and the communications of his survivors. London,* 1845. 8vo, pp. 560.

THE volume before us consists of two parts, the first being a Memoir of Dr. Jackson; and the second, a new edition, his work on the Formation, Discipline, and Economy of Armies. We shall notice them in order.

ROBERT JACKSON was one of a large class of men of whom Scotland has just reason to be proud, and who themselves have yet greater reason to be proud of Scotland—men who, by the strength of their native intellect and the goodness of their moral qualities, have, under the fostering care of their country's institutions, raised themselves from poverty and low estate to a high, dignified, and influential position in society, where they were enabled to do infinitely more good to their fellow-men and their native land, and to enjoy more personal happiness than could possibly have been their fortune in the station in which they were born. It is only in this increased capacity of doing good to others, as well as of augmenting the stock of individual happiness, that we see any benefit in the raising or being raised from a low to an elevated station; as we cannot regard anything as intrinsically and absolutely superior or inferior in either one or the other, or in the occupiers thereof—feeling and acknowledging with the mightiest of Scotland's low-born great, that

> "The rank is but the guinea's stamp,
> The man's the gowd for a' that."

And it is only in the departments of literature and science, or in the professions based upon these, that the full worth of the Scottish character and the peculiar excellence of Scottish institutions can be truly manifested. In commerce and the arts, ordinary industry and prudence, together with a favorable concurrence of circumstances, may and do raise men in all countries from humble poverty to wealth and distinction, with very little aid from letters, and without any very extraordinary personal exertions. And this takes place nearly as often in England as in Scotland. But with a few very remarkable exceptions, we shall look in vain in the learned professions for Englishmen who have reached the highest posts and honours from the very lowest stations of society. The case is, however, very different with Scotsmen; as we believe it will be found that many of the most distinguished and estimable members of the learned professions, more especially of the medical profession, have sprung from the humblest grades of life, from the low but not unenlightened abodes of the small farmer, the artisan, or even the cottar. And the causes of the difference are sufficiently obvious. They lie, partly, in difference of national character—particularly in that noble ambition of every Scottish father to raise his son above himself—but chiefly in the difference of the educational establishments of the two countries. The existence in every parish, or at least in every small district, of a good school for teaching not merely the rudiments of ordinary knowledge but *the classics*, and at a

rate of expense consistent with the resources of the industrious in every rank of life, keeps up, throughout the length and breadth of SCOTLAND, as it were, perpetual lamps of wisdom, which serve, at once, as tests for distinguishing the capacities of all her youth, and as sources of illumination to the minds of the select few who are found worthy to tread in the higher paths of knowledge. How different the case is in England need not be here stated; nor is this the place to inquire what is the relative value of the respective systems of the two countries: but we may surely be allowed to regard the failure of well-meant but most inadequate measures to introduce a national system of instruction into England, as a proof that the importance of education is altogether unknown or misunderstood by those who have themselves most profited by education in that country.

I. Dr. Jackson was the son of a small Scotch farmer, and was born at Stone Byers, Lanarkshire, in 1750. He was educated at one of the parish schools above referred to. After leaving school he served a three-years' apprenticeship to a surgeon at Biggar, and in 1768 went to the university of Edinburgh to study medicine, where he attended the classes during three winter sessions; making a voyage to Greenland each summer on board a whaler, as the medical officer, with the laudable purpose of thereby assisting to provide the means for pursuing his studies. Having gone out as a passenger to Jamaica, in 1774, he became assistant to Dr. King at Savanna-la-Mar, where he first was introduced to military practice, having medical charge of a portion of H. M.'s 60th regiment. In this situation he remained about four years until, actuated by a love of change and enterprise, which seems to have adhered to him throughout life, he resolved to proceed to America to push his fortune there. "It could not be said that he was happy at Jamaica: the scene was much too limited for one of his adventurous and inquiring turn of mind." He accordingly embarked for New York, but having neglected to provide himself with a certificate of having paid all his debts, which was required by the laws of the island, he was landed at Point Morant, the eastern extremity of Jamaica. His finances not admitting of his hiring a horse, he was compelled to make his way on foot from Kingston to Savanna-la-Mar, 130 miles distant; an undertaking, in such a climate, of a very arduous nature. Having accomplished this journey, and provided himself with the necessary document, he again embarked for New York, where he arrived in good health, but with an empty purse, in 1778. Failing to procure a situation as mate at the general or naval hospitals there, he offered his services as a volunteer to the 71st Highlanders, encamped about seven miles from New York, which were accepted; and as he stated he had studied medicine, he was appointed to act as surgeon's mate, at least until a commission should fall vacant.

He continued to serve as hospital-mate with this regiment till he was taken prisoner at Cowpens, in January, 1781, under circumstances most creditable to himself. He was released on parole early in 1782, and returned by New York to England, and resided in London till peace was concluded between Great Britain and America in 1783. He started in May of that year on a pedestrian tour through France, Switzerland, Italy, and Germany, which

occupied seven months. On the 9th December, 1783, he was gazetted to an ensigncy in the 71st regiment; and in the January following he walked from London to Perth to join his old corps, which was then about to be disbanded. He must have been almost immediately afterwards placed on half-pay by reduction, which he continued to enjoy till promoted to be surgeon of the 'Buffs' in 1793. In 1784 he married, at Edinburgh, the daughter of Dr. Stevenson, "an accomplished woman of good fortune. Being by this union placed in easy circumstances, he was enabled to pursue his studies at Paris for a longer period than on the former occasion." In the end of the same year he went to Brussels, and thence to Leyden, where, in 1785, he took the degree of M.D., and shortly afterwards established himself in practice as a physician at Stockton-upon-Tees, Durham.

In 1793 he offered to serve again in the army as physician to the forces, but as he had never served as surgeon, his offer was not accepted. He was, however, appointed to the surgeoncy of the 3d regiment or 'Buffs,' with a promise from Mr. Hunter, who was then at the head of the department, that the physician's commission would be given him on the first proper opportunity. In 1794 he accompanied the regiment to Flanders, and in the end of that year received his promotion by order of the Duke of York, notwithstanding the opposition of Sir Lucas Pepys, the physician-general, who declined to carry out the arrangement of Mr. Hunter. Shortly afterwards he succeeded Dr. Kennedy as inspector of hospitals, and was thus placed in charge of the medical department of a division of the army. On the infantry being withdrawn from Bremen, he was ordered to England. In 1796 he accompanied the expedition to St. Domingo as second medical officer, Mr. Weir being the first, and served there till the evacuation of Port-au-Prince by the British troops in 1798. He thence embarked for America, and made a tour through the States with Dr. Borland, returning in the same year to England, and rejoining his family at Stockton. In 1799, at the desire of the Russian ambassador, he was placed in charge of a body of Russian troops stationed in Guernsey and Jersey, (who, after their service in North Holland, had arrived in a most sickly condition,) and obtained much credit for the able manner in which he conducted their hospitals.

In 1800, His Royal Highness the commander-in-chief nominated him to be "physician and head of the army-depôt hospital," (at the Isle of Wight.) In the latter end of 1801, the physician-general and surgeon-general preferred charges of malpractice against Dr. Jackson, which, on being investigated by a board, were declared to be groundless, and they received a merited reprimand. As Dr. Jackson did not not choose to hold intercourse with a person who had acted as he conceived Mr. Keate to have done, and as he must have corresponded with him had he remained at the depôt hospital, he tendered his resignation, which was accepted by the commander-in-chief, who, in his letter noticed in a very flattering manner the doctor's services. Shortly after this he published two works on the medical department of armies, which assisted to draw attention to the subject, and ultimately to bring about the abolition of the board as it then existed, and the adoption of the present system. These works were

entitled 'Remarks on the Constitution of the Medical Department of the British Army, with a detail of Hospital Management,' (8vo edition, 1803,) and 'A System of Arrangement and Discipline for the Medical Department of Armies,' (8vo edition, 1805.) In 1807 he was offered and accepted the situation of military secretary to General Simcoe, the commander-in-chief in India,—"a rare, if not a solitary instance of the selection in the Royal Army of a medical officer to such a situation." Unfortunately, General Simcoe did not live to proceed to his command.

The physician and surgeon-general of the army repeated, in 1809, their accusations of 1801, which had already been declared unfounded; but, on Dr. Jackson applying for redress by a court-martial, he was informed it could not be granted as he was on half-pay. In the excitement of the moment he took the law into his own hands, and caned Mr. Keate, the surgeon-general, for which he received six months' imprisonment in the Marshalsea, and was required at the expiration of that period to give security for his good behaviour for three years.

The medical board having been dissolved in 1810, Mr. Weir was appointed Director-General. Having had experience of Dr. Jackson's services in St. Domingo, he applied for and obtained leave to employ him as inspector of hospitals in the West Indies, where he served with credit till 1815. From this period he remained unemployed till the autumn of 1819, when he offered to proceed to Cadiz to investigate the question of the contagious or non-contagious nature of the epidemic fever prevailing in that place. Being prevented by political disturbances from reaching Cadiz, he proceeded—actuated very much by that love of change of scene which, as we have seen, characterized his younger days—to Constantinople, Smyrna, the Archipelago, and Athens, returning by Patras and Zante to Malta and Gibraltar. Hence he proceeded to Cadiz, where the epidemic was raging, and remained two months studying its characters. He afterwards went to Xeres, where the disease was more severe and more fatal, and on his return to England published the result of his observations. This was the last public service on which he was ever employed, although in 1827, being then seventy-six years old, he offered to proceed to Portugal with the British force sent out under Sir William Clinton. A few weeks after this he died of paralysis at Thursby, near Carlisle.

Such is a brief outline of the principal events in the life of one to whom the army owes much for the improvements he effected in the military hospitals, as regards the comfort and welfare of the soldier, and who, at the same time, introduced a system of economy into their management, which has saved large sums of money to the country. His biographer states that "his plan, substituted for the colonial contract alone, saved £80,000 to the British government."

One of the most prominent features in Dr. Jackson's character, was his untiring perseverance in endeavouring to improve the condition of the soldier, to preserve him in a state of health and efficiency, and to afford him the best assistance in the hour of sickness, notwithstanding the powerful opposition of interested parties. He was remarkable also for the practical nature of the improvements he proposed in the department,

and for the disinterested manner in which he was willing to sacrifice personal considerations for the good of the service. On more than one occasion he intimated his willingness to serve wherever he could be useful, "*and that it was a matter of indifference to him in what rank he served;*" and when he volunteered to accompany the expedition to Portugal in 1827, he offered to waive his rank, and do duty in the military hospitals under a junior.

Jackson appears to have agreed in opinion with Paracelsus, when he says "art does not go after any one; it is we who must follow her. I have somewhere heard that a physician must be a rambler, and it is just that which I like best. Diseases travel everywhere over the wide world, and the doctor who would know much must travel much. Whoever will indagate nature must trample her book with his feet." We cannot, however, agree with his biographer, that "the details of Dr. Jackson's wanderings on the continent may serve to encourage those who cannot purchase instruction at the great seminaries of learning, to seek it where they can, in the wide field of nature, where it costs little but the exertion of seeking, and it has the recommendation of being always genuine." We hold the advantages Dr. Jackson derived from his rambles to be chiefly attributable to great innate capacity, and a remarkable power of observation, aided probably to some extent by his previous education and habits. It would be unsafe to draw any such conclusion, therefore, from this instance;—it would, in fact, be arguing from the exception instead of the general rule.

We could have wished more care bestowed on the literary execution of this Memoir, which in some parts reminded us of the remark applied by King James, of facetious memory, to Lord Bacon's works, that "like the peace of God, they pass all understanding." A little more labour in the compilation would also have materially improved the work; there is a great want of dates, and when given they are not always correct. For instance, in narrating his tour in the beginning of 1783, the editor observes, "his pecuniary means for the undertaking were but low. As a hospital mate, he was entitled to no half-pay; but he had been for years an ensign in the 71st regiment," &c. Now he was not gazetted to an ensigncy in the 71st till 9th December of that year, after he had returned from the continent; an anachronism which ought to have been avoided.

The most serious fault, however, we have to find with the Memoir, and which detracts greatly from its value, is the omission of a review of the author's works. The editor states, (p. 116,) "This is not the place to speak of his professional publications, which we briefly dismiss by saying that they are characterized by vigour of thought, close observation of nature, and originality of views." Now, from this opinion we dissent, because we know of no place so suitable for a review of the opinions and publications of a literary or professional man, as a memoir of the kind now before us, especially when it is not compiled till a sufficient period has elapsed after his death to deaden those deep regrets we feel at the loss of one we respect or love, and thus to enable us calmly to exercise our judgment in the performance of the task. To the Memoir is ap-

pended a list of his works, from which, however, two have been omitted, viz. 'A Sketch (*analytical*) of the History and Cure of Contagious Fever,' (London, 8vo, 1819;) and 'Remarks on the Epidemic Yellow Fever, which has appeared at intervals on the south coast of Spain, since the year 1800,' (London, 8vo, 1821.)

II. The work on the 'Formation, Discipline, and Economy of Armies,' was published at Stockton in 1804. Dr. Jackson states the main object to be "to investigate the powers and qualities of the physical properties of man individually, according to classes and nations; to impress the utility, and to suggest the means of arranging the parts in the military fabric, according to the powers of exertion rather than the semblance of external figure; to animate the fabric so formed, by the strongest and most extensive principle which operates upon animal structure, and to preserve the fabric so arranged and animated, in a state of efficiency, by the observance of a correct system of economy." His hobby was, in short, to make efficient soldiers without much reference to human passions, or to the individual happiness of the men. Indeed, he sometimes seems to convey the idea that he thought men were created for the purpose of being made soldiers—intellectual instruments—attending to their physical wants for the purpose of sustaining and improving their physical powers, on the same principle that care is bestowed on cavalry horses; and regarding the cultivation of their intellectual or moral qualities in relation only to their military efficiency, and not as promoting their welfare, and ultimate happiness, as individuals. A second edition was brought out in 1824, in the preface to which he alludes to the probability of a charge of presumption being brought against the physician who ventures to give instruction on the mode of forming, training, and maintaining the instrument of war in discipline and efficiency. But he most justly remarks that "an estimate of materials is primary to the exertion of the military, as well as other fabric; and as medical men are, or ought to be, from the nature of their studies, better acquainted with the materials of which armies are composed, than men of other professions, the author is not disposed to admit the charge of encroachment,—not even to allow that he has exceeded the limit of his station in doing what he has done."

The work is divided into six parts. The first treats of the "estimate of qualities, and selection of military recruits," which he considers as affected by class or race, climate, locality, age, state of society, pursuits and occupations; and lastly, the powers and capacities as existing in the actual constitution of the individual. In this part he discusses the various qualities which peculiarly fit men for different arms of the service, and offers many excellent remarks on the classing of men according to their previous occupation and habits, and their physical powers, as ascertained by trial, and thus endeavouring to obtain correspondence of action rather than uniformity of appearance. Unfortunately, however, these views cannot be reduced to practice in the British army, recruited as it is by voluntary enlistment. In this respect, commanding officers are in much the same position as the army surgeon is in the selection of recruits,

regarding which Dr. Jackson justly observes, "the duty imposed in this case is not to select what is in every way good, but to reject what is absolutely unfit." The second part consists of a sketch of the military character of the nations which are, or have been, most eminent in war. Part third gives an outline of tactic, or rudiment of military training. Part fourth treats of the intellectual and moral motives of military action. These contain much interesting and instructive matter, but as they relate more to military than medical subjects, we pass on to part fifth, on "discipline and economy for the field and quarters," under which are considered the important subjects of diet, clothing, exercise, barracks, camps, and transport-ships." On these points the health of our troops materially depends, and they consequently deserve the attention both of the military and medical officer. "The medical history of armies," says our author, "holds out a dismal picture of human misery. Armies were crippled, almost annihilated, by artificial diseases in the late war, that is, by contagious fevers, proceeding from corrupted sources of recruiting, and gaining strength from ignorance of the principles which conduce to the preservation of health, or from indifference and negligence in applying them to the occasion. Such losses are melancholy, because they proceed from error. The errors are not always reprehensible, for they proceed from ignorance and misapplied care, as often perhaps as from indifference and neglect. Soldiers are selected from the healthy part of the community. Reason says they ought to be more healthy than the mass of the people; it is not so in fact. The cause of sickness does not consist in actual hardship, for that is rarely to a great extent, or where it does occur it rarely affects the health." Dr. Hunter has justly remarked, that "in military physick, the great improvements to be made are not so much in the cure as in the prevention of diseases." (Diseases of the Army in Jamaica in 1766.)

Dr. Jackson's remarks on the various topics treated in this Part are in general very judicious, and evince a thorough knowledge of his subject—a knowledge acquired under the diversified circumstances of service in the field, and in garrison, at home, and abroad, in tropical and in temperate colonies. On one or two points we differ in opinion from him, but his observations are all well calculated to suggest useful hints, and afford materials for reflection. Under the head of diet he discusses a point worthy of the consideration of the military authorities, recommending that "care should be taken, in laying the basis of primary education, that every one be competent to dress in a suitable manner the raw provisions of which the ration consists. If the principle according to which the application of heat acts on raw provisions, for the improvement of their taste and nutritive qualities, be explained to the young soldier, and rightly comprehended by him, he will know to vary the mode according to his means, and not complain that his meal is unsavory when his ration is good in kind and abundant in quantity; a case which often happens with those who are young and uneducated, and which happens with almost all British soldiers in their first campaign." It is well known that in the last war the French troops much excelled the British in this respect. On service this is not a matter of such small account as some may suppose, for by

enabling the men to have their food in a palatable form they are induced to eat their rations, when from great fatigue they might otherwise reject it if in an uninviting shape. "It may be said without offence that if the art of cookery be understood by the English nation, it is not generally practised by the English soldiery."

Our space will not permit us to review in detail the opinions of our author on the various circumstances likely to affect the health of soldiers, and the best means of maintaining them in a state of efficiency—nor is it necessary. The work before us deserves the careful perusal of every medical officer in the army, and might be advantageously studied by every commanding officer. As the success of any military operation must depend in a great measure on the health and vigour of the troops employed to carry it into execution, there can be no subject more deserving the attention of officers than the best means of warding off disease from them, and of keeping them healthy and efficient.

We have already observed that Dr. Jackson was remarkable for the practical character of his suggestions, and for his zealous endeavours to promote the comfort and welfare of the soldier. We cannot illustrate this better, or point out more forcibly the great credit due to him, than by enumerating briefly some of the suggestions he made in the first edition of his work in 1804, and showing how these have been gradually adopted in the army. As regards diet, he recommended the introduction of a breakfast mess, which is now the rule of the service; he wrote strongly against the issue of a spirit ration as destructive of health and morals, and injurious to discipline: this was finally abolished by Sir H. Hardinge in 1830. In dress and equipment, he recommended the reduction of the weight of the kit, which has been partially effected, but is still capable of improvement; the use of trousers instead of breeches and leggins; the abolition of the custom of dressing the hair with grease and flour, a custom now unknown, and of the use of pipeclay to clean the clothing. On one point connected with soldiers' clothing, there yet remains great room for improvement. "The common manufacturer," says Jackson, "exerts his genius to produce a commodity of the flimsiest interior texture with the best exterior surface. The contractor for soldiers' clothing exerts himself to go beyond the manufacturer for the common market, and he generally succeeds; for soldiers' clothing is inspected and approved by less competent judges than those who purchase for themselves. It thus happens, from the bad quality of contract clothing and contract shoes, English soldiers are sometimes ill clothed on actual service, and almost always ill shod . . . . a circumstance which, exclusive of better primary education for war, gives foreign soldiers great advantages over English, in protracted campaigns and harassing services carried on at a distance from the depôt of stores."

In discussing the influence of exercise on the health, he advises the encouragement of dancing, running, football, cricket, quoits, and those other manly and athletic games, the introduction of which into the army has since been strongly recommended by the Military Punishment Commission, and partially carried into effect. But while he thus desired to find employment for the body, he also inculcated the necessity of attend-

ing to the mental culture of the soldier, a subject which has been slowly but steadily making progress, and now receives the sanction of the highest military authorities.

We shall only notice one more suggestion, that of quartering the troops in the West Indies in the mountains, and employing the soldiers to erect hut barracks for themselves in these situations. This he strongly advocated on the plea of humanity, economy, and increased military efficiency. Upwards of a third of a century afterwards we find these views adopted by Government, and carried out in Jamaica by the very means he suggested, not however until the policy of the measure had been demonstrated by statistical investigation, and enforced by the loss of upwards of 12,000 soldiers, of whom it is probable two thirds *at least* might have been saved had the measure been adopted when he first proposed it. Although he did not live to see this or the abolition of the spirit ration, we can conceive the satisfaction he must have experienced at the steady progress the principles he had laid down were gradually making in the army, —a satisfaction in a great measure arising from the conviction that they were promoting that which had ever been the chief object of his exertions, the melioration of the condition of the soldier. We cannot conclude this article better than with the following quotation from the preface to the second edition:

"The subject of the present work has been under the author's consideration for upwards of twenty years. He has looked at it without prepossession, as desirous to ascertain the truth. He believes that many of the hints which have occurred to him would tend, if properly understood, to diminish the miseries which are common in military life; and in that belief he has put them together, and now presents them to the public, gratified if they do good; at any rate satisfied with himself, as acquitted of a duty which he conceives to belong to the station in which he has acted."

ART. XIV.

A practical Treatise on Inflammation, Ulceration, and Induration of the Neck of the Uterus: with Remarks on the value of Leucorrhœa and Prolapsus Uteri as Symptoms of Uterine Disease. By J. H. BENNET, M.D. &c. *London*, 1845. 8vo. pp. 212.

IN the study of uterine diseases we in England labour under disadvantages, which the French accoucheurs, particularly in hospital practice, are exempt from. He would be, indeed, a hardy pioneer, who would undertake to face the storm of outraged feelings which the full adoption of the French system of exploration of the os uteri, in either public or private practice in England, would excite. Even to the mind of an *English surgeon* there is something revolting in the nonchalant manner of exposing females in the Parisian wards. This feeling, if it is found among the members of the medical profession in this country, is of course reciprocated a hundredfold by the public. It may be that it is a weakness, a want of philosophy; whatever it may spring from, so it is, and, in consequence, our neighbours enter this path of inquiry with advantages which we possess not; and, as

long as this is the case, we must yield to them, in some degree, the palm of practical acquaintance with those facts which the use of the speculum alone can reveal.

Not that the facts so discoverable have, in our estimation, the weight which some attach to them. The use of the speculum in French practice is so universal that the practitioners of that country are apt to despise all other means of investigation except as subsidiary to this; and therefore naturally give the most prominent and constant place to that which really might often be dispensed with. But we are happy to find that the use of this important means of investigation is increasingly adopted in England; and though we cannot hope or desire to see our national character so modified, as to find forty females at a séance submitting to a procedure, which must outrage all feelings of delicacy or decorum, yet, with proper precautions, and with all possible regard to decency, we do fully believe that this most valuable means of diagnosis may be practised both in public hospitals and in private practice.

We do not hesitate to join fully in the assertion of the intelligent author of the little book before us, that we have never found insuperable obstacles to the use of the speculum, and we cannot but attribute the comparatively rare employment of it more to the want of will in the surgeon than in the patient. We would strongly impress upon our surgical brethren that the value of the speculum consists, not more in its facilities for diagnosis, than in the *application of remedies*, and we do not hesitate to say, that all local remedies must be much more readily and perfectly applied with the speculum than without it.

But we must go farther, and say, that if the use of the speculum should not be more general than it is, the practice of the *touch*, as being more ready of application, and less likely to excite a feeling of opposition in the patient, should in all doubtful cases be considered indispensable. We are well aware that this practice, particularly in the country, is lamentably neglected. Patients are allowed to go on, week after week, month after month, to sink into an ill-understood but confirmed state of disease, and to death, without the least proposal being made by the surgeon to examine locally the nature and character of the affection. The accuracy which may be attained by practice in the detection of shades of disease by the finger is quite inconceivable by those who have only rarely practised it; and we hold it to be the bounden duty of every practitioner; who is in the habit of treating the diseases of females, to neglect no opportunity of examination of the os uteri by the touch, in order that he may acquire and perfect that discriminating sense of which the finger is capable.

We have been led to preface our observations upon Dr. Bennet's treatise with these remarks upon the means of investigating the diseases of the os uteri, from the conviction that a stimulus is greatly needed to induce practitioners of one of the most important branches of the healing art, both to make the most use of that means of investigation which they possess, and to take every opportunity of completing its value by the use of the speculum.

The opportunities which our author possessed of investigating uterine disease in Paris were considerable, and he has certainly made the most of

them: with the results he makes us acquainted in his book; of which we feel it but fair to speak in terms of considerable praise, as the produce of much industry and accuracy of observation. But we could have dispensed with much unnecessary matter; and had the pruning and clipping been intrusted to our hands, before it was sent to the printers, we should have been inclined to have reduced its size to the really valuable portion of the matter. Notwithstanding, we augur well for the future success of the author, from the ability and industry shown in the present attempt, and wish him heartily well in the very important course of investigation which lies open to him.

The frequency of chronic inflammation of the neck of the uterus has often struck us as remarkable; and has in many instances been to us the clue to obscure diseases, the external manifestations of which are not at all to be trusted to. Cases of apparent spinal, lumbar, hypogastric, and even crural neuralgia, and a number of the anomalous cases of obstinate nervous diseases, may be traced, more or less directly, to a chronic state of inflammation of this part, with which the whole system most peculiarly sympathises.

We can further entirely sanction, by the result of our experience, the views of our author upon the frequency of the *symptoms* of prolapsus depending upon a chronic inflammation, and consequent hypertrophy of this portion of the female organ. We were long surprised at the amount of distress occasioned by symptoms of prolapsus uteri when on examination we have found but little descent, and the size of the *entire* uterus not much more than natural, though the cervix itself was hard and occasionally tender. We were at first so struck with this discovery, that we were inclined to doubt the reality of the relation. But observation has convinced us that the symptoms usually attributed to prolapsus are often produced simply by the engorgement and hypertrophy of this portion of the uterus; and that to relieve the symptoms they must be attacked through it, as the fons et origo mali. Nor do we consider that in these observations we are at all at variance with the authority of Dr. Ashwell, who, in his valuable work on the Diseases of Females, gives it as the result of his experience, that inflammation of the cervix is rare; as, from the statement of symptoms which he there gives, he is evidently speaking of acute inflammation of this part, whereas the cases which have fallen under our observation have all been of a chronic character, and probably never exhibited symptoms of an active nature. That the symptoms of prolapsus are by no means always proportionate in severity to the size attained by the uterus, as generally understood, or to the laxity of the vagina and round ligaments, we think few practitioners will maintain, who are in the habit of examining all their cases by the touch. That the existence of such a state of parts in women who have borne many children, and who have favoured the descent of the uterus by over exertion, will be productive of severe distress, we deny not; neither do we doubt that, in neglected cases of inflammation and hypertrophy of the cervix, the foundation of permanent enlargement and descent may be laid; but we do contend that there is a large number of cases which a superficial examination would set down as prolapsus, in which the sufferings are dependent upon other causes than those of prolapsus in general, and which

will not be benefited by the ordinary appliances for prolapsus, but on the contrary injured by them. We are the rather induced to press this opinion from the conviction that the use of pessaries and mechanical supports, as they are called, is far too universal and too indiscriminate in these cases. This mode of palliation is all well enough where the prolapsus is produced by a very relaxed state of the vagina, and increase of size in the uterus, from frequent childbearing. But when this cause has not been in operation, and where the symptoms of prolapsus being present at the same time, the vagina being natural, and the body of the uterus natural, or nearly so, in size, and when the cervix is indurated and hypertrophied, the symptoms of prolapsus will be aggravated rather than diminished by the pessary, and the symptoms can be relieved only by the treatment being directed to the correction of the morbid state of the cervix. Yet we have frequently known these cases treated by the use of the pessary, and, as might be expected—assuming that the view we have just given of their cause and pathology be correct—with injury rather than benefit.

The observations of Dr. Bennet on the pathology and treatment of ulcerations of the cervix, in the uterus which has never been pregnant, are practical and valuable, and deserve to be well borne in mind, in forming a prognosis. But this class of cases is just that in which we should be inclined to dissent from his continual use of the speculum. In those slight cases which simply indicate a superficial ulceration, or a slight hypertrophy of the cervix, we can see no possible necessity for its use, or for putting the patient's feelings to the unpleasant trial of the proposal to employ it, and, we must add, the doctor to the unpleasantness of recommending it. Certainly, the examination by the touch is all that the necessity of the case requires for the diagnosis; and the practised finger may here be safely relied on. At the same time we must allow, that if the application of the cautery, actual or potential, is required, the speculum is still essential for its application. But we further contend, that in these slight cases of which we speak, viz. those occurring in the uterus which has never been impregnated, other and milder forms of remedy will answer the end.

Dr. Bennet is sometimes visionary and too theoretical, as when he endeavours to prove the extreme frequency of laceration, followed by cicatrization and even ulceration, as the result of natural labours, (pp. 37-8.) We have so entire a confidence in that wonderful arrangement for the steady and natural, though vast change which takes place in the uterus, from the first deposit of the ovum to the last stage of the restoration of the maternal organs, that we have no hesitation in protesting against the adoption of an axiom founded on the idea that nature is unequal to the completion of her work.

We have seen no reason for believing that there is any laceration at all of the lining membrane or other structures of the uterus, in by far the majority of labours. But in fact we must warn Dr. Bennet's readers to bear continually in mind that his experience is principally drawn from hospital practice among the lowest class of prostitutes, and that he of course must be understood to be speaking always of that class: we do not therefore suppose that his observation of the opinion of M. Boys de Loury, that "ulceration of the cervix is common with pregnant women," is intended to have any further application than to the class in question, though the assertion is made without this qualification.

Upon these same grounds, as well as from the result of our own observations, we are disposed to demur to the conclusion that "the state of pregnancy predisposes to inflammation and ulceration of the cervix uteri." We here speak of the general run of cases, while it is evident that Dr. Bennet's conclusion can only be received as regarding the case of pregnancy in diseased syphilitic subjects. Had he qualified his assertion by this allowance, we had yielded direct assent, but then to the practitioner in midwifery the value of the dogma is materially diminished by this qualification.

When he enters into the province of cancerous ulcerations, our author tells us but little that is not universally received, and it would have been better modestly to have acknowledged this than to have assumed the character of guide through a *terra incognita* without any better pretensions to the character than many who have gone before. His diagnosis between simple, severe inflammatory ulceration and cancer fails entirely, as every attempt of the kind has failed, and must fail, by taking the character of an extreme case, and its features as those which are to be universally found, and in all stages of the disease. What we want as a diagnostic, is a universal and readily recognizable character which may indicate the genus in every locality and under every form and stage of growth. Dr. Hodgkin has gone further than any one else in accomplishing this most desirable object, and were the cystiform arrangement as readily recognizable during life as it is universal in its occurrence when examined after death, there would be little more to desire in the way of diagnosis, and cancer would be reduced to the same category under this head with all other ordinary diseases. This, however, is not the case, and we are still obliged to grope our way in the dark without the least spark of enlightenment from the past efforts of science. We are not without hopes that the forthcoming work of Dr. Walshe may remove some of our difficulties.

Dr. Bennet's cases of severe ulceration, both in the pregnant and unimpregnated state, are interesting; and his descriptions and cases of genuine chancre in the cervix uteri are well worth studying. (pp. 92 to 122.) On the subject of the treatment of obstinate ulceration and of hypertrophy of the cervix by deep cauterization, we shall only say that the plan is valuable if further trial gives the same amount of success which attends it, according to Dr. Bennet, in the Paris hospitals. We have not witnessed it, nor are we aware of its having been adopted in this country, but it may nevertheless be well worth the trial. In this practice our neighbours are bolder than we are, as indeed in surgery generally, which fact is strongly contrasted with what we should often call timid practice in medicine.

The deep cauterization is, according to Dr. Bennet, used with very general benefit and without any risk. In judicious and skilful hands it promises well in our estimation, and we should therefore strongly recommend those who are frequently in the habit of treating uterine diseases to turn their attention to a plan which gives great promise of benefiting permanently a most distressing class of cases.

We shall here append, in the form of extracts, the details of the principal local means of treatment recommended by Dr. Bennet, not only in these severer forms of disease, but in the slighter also: and we shall begin with the latter. Frenchified as the style will be found in these, they are

far from unfavorable specimens of the manner in which Dr. Bennet sometimes travesties his mother-tongue. But his long residence in a foreign country is a better excuse than most delinquents in this way have to plead.

I. "*The treatment of inflammation and ulceration of the uterine neck* [*neck of the uterus*] *in women who have not borne children.*"

A. *Without ulceration.* "The remedies usually resorted to in general vaginitis are here indicated. The most efficacious is, undoubtedly, the solid nitrate of silver, lightly drawn over the inflamed cervix, and over that part of the superior parietes of the vagina which generally participates in the inflammation.....

"After the mucous surface has thus been slightly cauterized, emollient or slightly astringent injections (acetate of lead or sulphate of zinc) must be used three or four times a day; complete rest must be enjoined; sexual congress strictly forbidden; and the general health attended to.....

"On the parts being again examined, in the course of a week or ten days, the inflammation will generally be found to have subsided, unless it coexist with blennorrhagia, in which case it will prove much more difficult to eradicate, and requires the same treatment as in the other regions of the sexual organs. Sometimes, nevertheless, it will be found advisable or necessary to reapply the nitrate of silver to the mucous surface a second or a third time. This occurs more especially when the injections have been improperly performed, as is usually the case if the patient is not instructed as to the way in which they should be used...

"To obtain the full effect of the injection, the patient should recline on the side of a bed, or of a lounging chair, elevating the pelvis, so as for the vagina to form an inclined plane, of which the cervix is the most dependent point. The vagina thus retains the injected fluid, like a vase; it penetrates gradually into every part, and remaining in contact with the inflamed tissues for a few minutes, exercises a decided influence on them." (pp. 138-41.)

B. *With ulceration.* "The basis of the treatment of inflammatory *ulceration* of the cervix uteri is, the cauterization of the ulcerated surface. If the inflammation has not penetrated to the deep tissues of the cervix, and there is no general inflammatory hypertrophy of the organ, *superficial cauterization*, combined with emollient and astringent injections, rest both of the organ and of the system, and attention to the general health, is all the treatment that is required, and will generally effect a cure in from four to six or eight weeks.....

"The progress of the inflammation and ulceration is, generally speaking, at once arrested by the cauterization. The congestion and redness of the cervix diminish visibly, the granulations become smaller and healthier, and the purulent secretion assumes the character of laudable pus, if it has not presented it before. When the cauterization is suspended, the ulceration will, however, often remain stationary, even if other measures (injections, &c.) are resorted to; and if left entirely to itself it is nearly certain to relapse, however advanced the healing process may be.....

"The agents used for the cauterization of the ulcerated cervix are various. The principal are the nitrate of silver and the acid nitrate of mercury. The latter is exclusively employed by many Parisian practitioners, among whom I may name M. Emery. It is a dissolution [solution] of deuto-nitrate of mercury in nitric acid ....

"When recourse is had to the nitrate of silver, its application may be repeated every fourth or fifth day, whereas the acid nitrate of mercury should not be applied oftener than once a week. The eschar formed by the former, being much more superficial than that formed by the latter, falls much sooner. In either case, whenever cauterization is resorted to, whatever be the agent, the blood or mucus which covers the cervix must be wiped off before the caustic is applied, otherwise the greater part of its power is lost in coagulating these fluids. This can be done conveniently by means of a little lint or sponge, held in a pair of long

forceps. Those which I use are thin, about ten inches in length, and made on purpose. The solid nitrate of silver, fixed in a long porte caustic, must be drawn two, three. or four times over the ulcerated surface, according to the effect wished to be obtained.

"In order to apply a fluid caustic, the following plan should be resorted to:—A small thin stick, about a foot in length, having been chosen, is formed into a brush, by inserting between its divided extremities a little wool, lint, or old linen, which is then fastened by a few turns of thread. These little brushes may be made *ex tempore*, and being of no value, can be thrown away when they have been used. The brush having been introduced into the acid, should be pressed against the rim of the bottle, in order that it may be merely moistened with the caustic, and then drawn over the ulcerated surface. A little water must then be injected into the speculum before withdrawing it, in order to absorb any superabundant acid.....

"In cauterizing the cervix, the speculum must be *firmly* applied to the parts, so as to protect the vagina from the action of the caustic." (pp. 144-50.)

II. "*The treatment of inflammation, ulceration, and induration of the uterine neck in women who have borne children.*

"In acute and recent cases of inflammation of the cervix in women who have borne children, the inflammatory nature of the hypertrophy is well marked by the intense redness and heat of the enlarged organ; but in chronic disease, these symptoms subside, leaving a passive hypertrophy, which appears to depend principally on a modification of the nutrition of the part. The redness is no longer vivid, and the heat is scarcely, if at all, greater than natural; indeed, the inflammation may be said to have ended in induration. Even in these cases—in which the disease has reached, as it were, one of its natural terminations—the ulceration of the surface of the organ often persists. This ulceration is kept up by the frictioning [!] of the hypertrophied organ against the parietes of the vagina.....

"The chronically enlarged cervix is extremely hard and resistant to the touch in its entire extent, and has not the latent elasticity which may still be observed in acute inflammatory hypertrophy. In either case, the size of the organ varies from that of a walnut to that of a large egg. The larger the volume, the more distressing are the symptoms produced by the prolapsus and retroversion of the cervix, so that the life of a woman who is labouring under hypertrophy of the cervix is often a burden to herself; she is never free from painful and disagreeable sensations.....

"As long as the uterus itself is acutely inflamed, no examination with the speculum should be attempted, as it would be very painful; but as soon as these inflammatory symptoms have subsided, it must be at once resorted to, as nothing will tend so much to allay the irritation of the uterine system as cauterization of the ulcerated surface. So far from cauterization in these cases, even when there is still acute inflammation of the cervix, and slight subacute inflammation of the uterus, endangering the life of the patient, as has been asserted, by exposing her to metritis, or metro-peritonitis, it invariably diminishes the inflammation both of the cervix and of the surrounding parts.....

"The measures which I recommended in inflammation and ulceration, unaccompanied by general induration, (viz., superficial cauterization, injections, rest, and a light diet,) will very often alone suffice both to cure the ulceration and to resolve the induration. . .

"This, however, only occurs when the nutrition of the engorged cervix has not been deeply modified by the subacute inflammation of which it has perhaps been the seat, for months or years Not unfrequently, especially in very chronic cases, the hypertrophy only diminishes to a certain extent under the above treatment, and even that very slowly, and then remains stationary, whether the ulceration heal or not.....

"Complete rest, which, in ulceration unaccompanied by induration, I stated to be only extremely desirable, is now indispensable. Indeed, if the patient can be

prevailed upon to keep her bed for a few weeks, it is much the best plan. When walking, standing, or even sitting, the enlarged cervix drags down the uterus; whereas, when the patient is lying down, this does not occur. If there is a good deal of hypogastric pain, large linseed poultices applied to the hypogastrium, and changed occasionally, will often give great relief. These poultices should be made thin, or otherwise their weight is painful. Tepid or cold hip-baths twice a day are useful adjuncts to the treatment.

"In these cases, cauterization of the ulcerated surface may generally be resorted to from the first, but the action of the nitrate of silver is too superficial, and the acid nitrate of mercury, or caustic potassa, should be preferred. Emollient or astringent injections should also be used. When the inflammation is confined to the neck, emollient injections will suffice; if the vagina is also inflamed, astringent injections are indicated.....

"The treatment may be confined to these measures for two or three weeks, during which time the influence of the medication followed must be narrowly watched. If the ulceration becomes less angry looking, if the granulations assume a healthier appearance, and if the hypertrophy of the neck appears rapidly to decrease, the treatment may be continued, as it will probably prove quite sufficient to effect a complete cure. But if this is not the case, if the amelioration which at first takes place ceases, or if the ulceration appears inclined to heal without the induration giving way, other measures must be resorted to.

"The most efficacious is the application of leeches directly to the uterine neck itself. They are extremely useful agents in subduing deep-seated chronic inflammation in this region. The following is the easiest way to apply them: after introducing an ordinary conical metal* speculum, wipe off the mucus which covers the surface of the cervix with a little lint or sponge, and then place the leeches in the interior of the instrument. Over the external orifice of the speculum spread a piece of linen, which depress with the finger into the speculum. In the concavity thus formed, place some lint or cotton, and then, with the forceps, push the whole towards the uterine neck. The linen carries the leeches before it, and presses them against the os uteri. On pulling out the linen and the lint, with which the speculum was plugged, in the course of about ten minutes it will nearly always be found that all the leeches have taken. They generally fill well in this situation, and the flow of blood is often considerable after they have fallen.....

"In order to modify effectually an engorged cervix, which has resisted all other modes of treatment, the indurated organ must be deeply cauterized, either with the Vienna paste, the plan adopted by M. Gendrin, or by the actual cautery, that followed by M. Jobert, (de Lamballe.) The eschar which forms, in either case, is much deeper than that which is created when the fluid caustics are used. The inflammation which accompanies its separation is also much more intense, and generally propagates itself into the entire cervix. The result is, that not only is the hypertrophied cervix diminished by the extent of the eschar which separates, but that the healthy inflammation set up in the chronically indurated tissues gradually melts them, as it were; so that often, on its subsiding, the hypertrophied cervix has regained its natural size.....

"If the Vienna paste is employed, the following is the plan pursued by M. Gendrin, which I likewise follow. I must first, however, state that the Vienna paste, which is much used in France to produce deep eschars, is formed of equal parts of quicklime and hydrate of potassa, reduced to a fine powder, and intimately mixed. This powder should be prepared only when wanted, and kept in a glass-stoppered bottle. To be used, it is made into a paste with a few drops of alcohol, and the paste is then spread over the part to be destroyed....

"When applied to the uterine cervix, a large and conical speculum must first be introduced, and the engorged cervix made to enter its orifice; or should the cervix be too voluminous, the speculum must be firmly pressed on the part

which it is intended to cauterize, great care being taken not to inclose between the rim of the speculum and the cervix a fold of the vagina. About as much of the paste as would cover a fourpenny-piece, a line in thickness, must be placed on a triangular piece of diachylon plaster, one end of which is inserted lightly in the cleft extremity of a small stick. The caustic paste is then carried by means of the stick to the cervix, and applied to the centre of the part comprised by the orifice of the speculum. With the long forceps, cotton is placed carefully all round the spot on which the caustic is applied, so as to completely protect the neighbouring parts; the stick having been withdrawn, the speculum is two-thirds filled with cotton or lint, which is firmly pressed against the uterine neck. The speculum is then extracted, the cotton which fills it being forcibly pushed back in the vagina with the forceps, as it is pulled away, so that the vagina remains thoroughly plugged. If all this is carefully done, it is impossible for the caustic to fuse, and to injure the parietes of the vagina. In about fifteen or twenty minutes, the cotton or lint must be gradually withdrawn by means of a bivalve speculum, and an eschar, of the size of a shilling, or rather larger, will be found where the caustic was applied. The vagina should then be washed out with a little tepid water, complete rest in bed enjoined, and emollient injections employed until the separation of the eschar, which takes place from the sixth to the eighth or tenth day." (pp. 138-78.)

We cannot part with our author without again expressing our high opinion of his little work, and recommending our brethren to possess themselves of it.

Art. XIV.

De Tenotomia Talipedibus applicata. Commentatio quam scripsit Chr. Weis.—*Havniæ*, 1844.

A Treatise on Tenotomy as applied to Club-foot. By C. Weis.—*Copenhagen*, 1844. 12mo, pp. 94.

This is a small, unpretending dissertation in less than a hundred pages, containing more practical information than some of the more bulky tomes written on the same subject. The author gives the result of his experience, to which, under the several heads of *treatment, age for the operation, period of origin and causes of club-foot, complications, prognosis, preparatory treatment, modes of operating,* and *unlucky incidents during the treatment,* he has prefixed the opinions of Stromeyer, Dieffenbach, Scousetten, Duval, Guerin, Little—in short, of all the principal modern writers on treatment of deformities. We have so fully considered the applications of tenotomy, and the general treatment of deformities, in some former Numbers, that it is unnecessary to follow the Danish author through every division of the subject. We shall confine our remarks to some of the more essential questions discussed by him.

The author considers it is not advisable to delay the operation until the age of 12 months, as some authors have recommended. We agree with him to the extent that wherever the operation is absolutely requisite, the earlier it is performed the better; but we wish the author had given us precise rules for determining, during the earliest period of life, what cases may be successfully treated by mechanical means, and those which indispensably demand operation. We fear the mechanical orthopædists of the present day still profess to cure all by instruments, whilst the surgical

orthopædists display a still greater zeal in performance of tenotomy in every case. We believe that if the cases of congenital club-foot be classed under three grades, all those of the first or slightest grade may unquestionably, within a reasonable time, be cured by well-applied mechanical treatment. Many of the second grade will also yield to this method, although the treatment will necessarily be more tedious, perhaps scarcely completed when the time arrives at which healthy children usually "walk alone,"—say at the age of 12 or 14 months. The majority of cases of the intermediate and the whole of those of the severer grades, can only be remedied within a reasonable time by operation. We should not venture our opinion in opposition to the evident leaning of the author more fully to appreciate the operative method, if we did not entertain the conviction, that, however essential tenotomy is in the above proportion of cases, it is improper in the remainder, for although the form of the member is by means of tenotomy more promptly remedied, the function of the divided muscles is often less completely restored. We have met with many instances of talipes successfully treated without tenotomy, in which the muscles of the calf attained the normal development, whereas, we believe that after tenotomy the muscles usually remain smaller than natural. We desire our views not to be misunderstood; we are too sensible of the value of tenotomy, properly applied, to disparage it; we simply offer an opinion based upon experience, that may possibly stay the hand of an over-zealous operator. Useless, i. e. improper or unnecessary tenotomy weakens the muscular powers of a limb which without operation might have attained normal development, whereas,—paradoxical as it may at first sight appear,—the proper application of section of tendons, by enabling the attendant to effect a cure of a deformity, proved by experience in similar cases to be otherwise irremediable, prevents the occurrence of atrophy, the ordinary concomitant of uncured club-foot; nay more, in proper cases, the operator has the satisfaction of witnessing a gradual augmentation of muscle.

The following remarks occur under the head of "complications."

"Whenever talipes is complicated with hemiplegia, paraplegia, or contraction of other articulations, as for example, of the hip, knee, wrist, or hand, or when strabismus coexists, the assemblage of symptoms indicates a common origin in the brain, or more probably in the spinal cord. The treatment of the secondary affections cannot be contemplated, whilst the morbid process in the central organ is in actual progress, but upon removal of the primary disease, the treatment of the contracted limbs, or the strabismus, may be undertaken. Very commonly, however, it is exceedingly difficult to discriminate the cases in which the lesion in the nervous centre is still in progress, displaying its existence by the secondary affections, from those cases in which the contractions independently exist. It is necessary for the solution of this difficulty, to seek for the peculiar symptoms of disease of brain or spinal chord, to ascertain whether the secondary phenomena have long remained stationary, i. e. whether the contraction or paralysis of muscles already affected, continues to increase, or progressively involves others. Notwithstanding the utmost diligence, it may even be no easy matter to determine whether the aggravation depends upon the primary disease, or is the result of the existing deformity." (p. 33.)

We agree with Dr. Weis on the importance of determining these matters before undertaking the treatment of deformities, but we cannot consider that with due diligence and experience the matter is so difficult as he con-

siders it to be. With the exception of some spasmodic contractions, which, like other convulsive diseases, such as tetanus, chorea, and cramps, are unaccompanied with evidence of physical lesion of the nervous tissue and febrile disturbance, deformities are for the most part well defined in their modes of origin. We can usually observe without difficulty the *incrementum* and *acme* of the disorder, as the older medical writers would have spoken. We can lay down a better rule for determining the period when the operation may with propriety be no longer delayed, namely, with very rare exceptions, never to divide tendons until satisfied that the resistance to proper movement of the articulations arises not simply from augmentation of contractility of the muscular fibres, but from structural or organic shortening of the fibres, (*retraction* of the French writers,) by which a mechanical obstacle to elongation is offered. We are convinced that tenotomy operates mechanically in the case of contractions, and not, as asserted by Stromeyer, dynamically; except in so far as the section must necessarily for a time, until close reunion of the divided parts takes place, dynamically weaken the muscular fibres, and when a wide separation of the divided parts permanently continues, the debilitating influence of the section must be permanent. Stromeyer has taught that tenotomy is antispasmodic in the therapeutic signification of the term.

Dr. Weis follows the original recommendation of Stromeyer, in causing his patients to wear the extension apparatus a few days before operation, by which means the patient becomes accustomed to the instrument, and the practitioner by observing its action can improve the adjustment without the inconvenience and loss of time occasioned by alterations after performance of the operation. This preliminary proceeding is not necessary to the practitioner experienced in treatment of deformities, but we are persuaded that if it had been followed by the inexperienced, fewer unsuccessful operations of tenotomy, and the direful sloughings from undue pressure, of which we have heard mention, and the consequent suffering and disappointment, would have been avoided. In the chapter on the tendons which require division, Dr. Weis strongly insists, in the treatment of T. varus, on the importance of dividing the muscles which act upon the front part of the foot and produce the powerful inversion which characterizes this affection, so as to overcome this part of the deformity before dividing the tendo achillis, and attempting to bind the foot. This plan, which originated with Dr. Little, has, no doubt, from its obvious division of the treatment of severe cases of varus into two stages greatly facilitating the result, suggested itself to every person familiar with the difficulties of these cases.

The author fully describes the modes of dividing the different tendons, a matter not difficult of execution, when those prominently or superficially situated are concerned.

We are glad to meet in the pages of our author the discussion of the methods of arriving at a perfect section of the posterior tibial tendon, the contraction of which so powerfully affects the form of the ankle. In members sparingly covered with adipose tissue, and in slight deformities, the posterior tibial tendon may with facility be attained, but in such cases the operation is seldom required. In infantile varus, on the contrary, especially when the affection reaches a high grade, or in the fat limb of a

healthy infant, the section of this tendon is not a perfectly simple matter on account of the comparative depth at which it is situated, and its proximity to the posterior tibial blood-vessels and nerve.

Stromeyer effects the division by inserting the point of a knife between the artery and tendon one and a half inch above the internal malleolus, the artery being secured from injury by application of the index-finger upon it. He then directs the edge of a knife against the inner side of the tibia, and severs the tendon. Dr. Weis prefers the method of Velpeau, who divides the tendon close to its insertion into the navicular bone. The limb is rested on the outer side, a sharp pointed knife is introduced at the distance of a few lines from the malleolus, a little beneath and posterior to the *anterior* tibial tendon. The point is then carried directly backwards, and attains the tendon about an inch below and in front of the extremity of the malleolus. "Injury to the plantar artery is with certainty avoided by taking care that the point of the instrument is not carried further into the sole than absolutely necessary for section of the tendon." Now, it is precisely this dependence of the safety of the artery, upon not carrying the point of the knife too far into the sole, which, from our experience, leads us to decide that Velpeau's operation is less secure as regards the artery than Stromeyer's, and also less certain of effecting the object of the operation—complete division of the posterior tibial tendon. It should be an important *rule* in tenotomy to cut in a direction *away from* arteries and nerves, and not, as in Velpeau's operation, *towards* them. We should not omit to mention the result of Dr. Weis's experience: he states that he has found Velpeau's method safe in the performance and certain in the results. Our author is unacquainted with the plan of operation, known here as that of Dr. Little, which consists in introducing, about one inch above and in front of the tendon (through a puncture made in the integuments and fascia with a common scalpel,) a blunt-pointed knife similar to Bouvier's tenotome. This is passed beneath the tendon, towards which the *surface* of the instrument is directed, the operator then turns the *edge* towards the tendon whilst an assistant endeavours to straighten the member. The tendon then cuts itself, as it were, upon the opposing edge. This operation is safe, as in case of injury to the posterior tibial artery the vessel will be completely severed, and not punctured as when a pointed instrument is used. Experience in subcutaneous section of this tendon and artery has proved that in this mode of operation no hemorrhage ensues, whereas after puncture of the vessel much trouble and anxiety has been occasioned.

The treatise contains representations of various suitable apparatus, an omission we have noticed in other works, the authors of which evidently attach too great importance to the éclat of the surgical operation, whilst the absence of details respecting mechanical treatment indicates their inattention to this portion of treatment, admitted upon all hands to be as essential to recovery as the operation itself. Many failures may doubtless be traced to this source.

We take our leave of Dr. Weis in thanking him for the copious information his book contains, and in expressing our admiration of the candid, modest manner in which his opinions are advocated.

Art. XV.

Beiträge zur Medizin, Chirurgie und Ophthalmologie. Von Chr. Conr. Wuth, Dr. med., chirurgiæ et artis obstetriciæ, praktischem Arzte etc. in Hannover. Mit Abbildungen. *Berlin*, 1844.

Contributions to Medicine, Surgery, and Ophthalmology. By C. C. Wuth, M.D. With two lithographic Plates. *Berlin*, 1844. 8vo, pp. 134.

The first forty-five pages of this work are occupied with an essay, entitled "General reflections on medicine," the chief drift of which is to show the necessity of being well acquainted with the healthy actions of the body, before attempting the study of diseases. The rest of the work contains a collection of interesting cases, partly medical, partly surgical, which have occurred to the author during a practice of many years' duration.

Although we cannot compliment Dr. Wuth on his style of writing, which is both rugged and prolix, the valuable facts contained in his work will well repay the pains of perusal, as may partly be judged from the following case, the only one on record, as far as we know, of a polypus being extirpated from the frontal sinus, and which we shall present to our readers in an abridged form. Its treatment does great credit to Dr. Wuth's skill:

Case.—"A boy, aged 10 years, was put under Dr. Wuth's care by his parents, with the remark, that for the disease of the eye, under which the child had already laboured nine years, they had sought the aid of many practitioners, who had used for it both internal and external remedies without relief. Their means were now exhausted, and the disease seemed to be incurable. During all this time the boy had suffered from severe pain in the head, and had enjoyed little or no rest, night or day. The left eye was so entirely pushed forwards out of the orbit, that it lay on a level with the back of the nose. Its lateral displacement projected it so much over the cheek-bone that, viewed in front, it hid the neighbouring side of the face. The displacement downwards brought the eye into a line with the point of the nose. For the last three years the eye had closed less and less completely, and the lids now covered it so imperfectly that the cornea, with a circumference of sclerotica four lines broad, remained constantly exposed,—whence a perpetual flow of tears. The strong orbicularis palpebrarum, whose annular fibres surrounding in concentric lines the protruded eye, seemed to press it still more out of the orbit. The extraordinary development of the muscle seemed to arise from its antagonistic action against the protruding swelling. A large, deep ulcer of the cornea threatened a speedy bursting, and total disorganization of the eyeball. A convolution of varicose veins of the conjunctiva covered the visible part of the sclerotica.

"The regions of the frontal and nasal bones were greatly protruded; while the eyeball had gradually quitted its natural place, in proportion as the orbit had become contracted, by the pressure exercised upon its constituent bones. The left side of the nose formed a flat surface along with the back of the nose, and a firm obstacle presented itself to the finger passed into the left nostril. From the stretching of the skin, the left eyebrow was separated widely from the right, and dragged downwards. The skin itself was thickened, and doughy to the touch, while at the outer-under part of the eyebrow was a small opening, from which, on pressing the surrounding region, a whitish mucous fluid walled out.

"As to the exciting cause, all that could be said was that nine years before, the child had a red-spotted eruption, with cough and severe headache (perhaps measles,) and that the present disease had for several years been increasing,

under almost constant pain in the head and many sleepless nights. Although the boy was much emaciated, and had a livid countenance, he seemed to possess a good original constitution.

"Dr. Wuth's diagnosis led him to a favorable prognosis. He showed the patient to the Hanover Medical Society. The views of the members on the nature of the disease were much divided. Some thought it medullary fungus, other exostosis, osteo-sarcoma, a tumour in the orbit, &c. Dr. Wuth gave it as his opinion that a large polypus occupied the frontal sinus, which he trusted he should be able to remove by an operation. To this he proceeded next day, by making first a vertical incision, two inches long, from the root of the nose upwards through the soft parts, and then a horizontal one, also two inches long, close above the eyebrow. He next dissected off the triangular flap thus formed, so far as to permit the frontal sinus to be trepanned. In the middle of the superciliary arch was a small hole in the bone, scarce a line in diameter, opening into the sinus, and explaining the source of the fluid already mentioned, which had kept the soft parts in an inflamed and fungous state. This state of the soft parts caused a considerable bleeding in the course of the two incisions. In consequence of the great dilatation of the sinus, it was necessary to make two openings into it with a small trephine, whereupon an immense quantity of polypi protruded, being connected together like grapes, and covered with a milk-white fluid. Dr. Wuth removed the greater part of the polypi with cutting instruments. After washing away the slimy, milk-white fluid, he remarked that the polypi internally were of a yellow colour and partly transparent, and that within each polypus there ran two or three vessels, giving off branches on all sides. The upper portion of the cavity contained mucous polypi, which felt partly soft, and yielded readily to pressure, but were partly firmer and of cellular texture, so that out of the cells a white, slimy fluid could be pressed. In the middle of the cavity the morbid product became denser and more opaque; posteriorly and towards the parietes of the cavity, it assumed a fibrous structure. The cavity terminated towards its inner and outer sides in small sinuses, or cellular spaces, in which the polypi lay firmly imbedded.

"Dr. Wuth next directed his attention to radically destroying the remainder of the disease, attached to the parietes of the cavity and contained in its cellular spaces, selecting such means as were not likely to act injuriously on the very thin osseous partition, separating the cavity from the brain, and which was felt to yield to gentle pressure with the finger. He felt with his finger, lightly applied, the pulsation of the brain, and was afraid lest so thin an osseous partition might yield to the movements of the brain, now that the counter-pressure was removed by emptying the cavity of its contents. He now bored through the osseous parts from the cavity of the nose to the frontal sinus, as the ethmoid cells, conchæ, &c., and introduced a silver canula, so that the fluid collected in the sinus might flow off unconfined. Further, to remove the stillicidium lacrymarum which had arisen from the compressed state of the nasal duct, he perforated the nasal bone from without inwards, so as to meet (if we understand him right,) the artificial canal just mentioned, and here put in a second tube. After a time, being convinced that the tears had a free passage, he removed the tube from the artificially-formed nasal duct; and feeling assured of the healthy state of the frontal sinus, he removed also the canula, leaving the opening into the sinus to close.

"The healing of the parts occupied twelve months, the frontal sinus being by that time considerably lessened in all directions, and the eye having partially returned into the orbit. The ulcer of the cornea cicatrized soon after the operation, leaving a leucoma, which during the succeeding years diminished considerably, so as to permit the sight to improve. The physiognomy of the body became much more tolerable. From the first night after the operation he enjoyed sleep, such as he had not had for years. The first six weeks he spent chiefly in sleeping and eating, whereby his exhausted constitution was speedily restored. After this Dr. Wuth saw the lad twice a year, and each time found the disfigurement to be diminished.

"With regard to the applications made to the parietes of the sinus, in order to restrain any new growth, Dr. Wuth was afraid to use lunar caustic or kali causticum, and employed laudanum, Goulard's extract, and kreosote, pencilling the parietes with a mixture of equal parts of the two former, and then applying a salve, composed of an ounce of zinc ointment and ten drops of kreosote, spread on lint."

From the above case, Dr. Wuth draws the following physiological and pathological conclusions:

1. The possibility of the continuance of the power of vision under gradual traction and elongation of the optic nerve.
2. The enormous enlargement of which the frontal sinus is capable,—equal in this case to the containing of three hen's eggs.
3. The return of this osseous cavity almost to its normal form.
4. The complete division of the supraorbital and frontal nerves did not in this case injure vision.
5. Pain is not a pathognomonic sign of the malignant or benignant nature of a swelling, a fact illustrated also by cases of induration of the mammæ and other organs, which by their spontaneous disappearance show their non-malignant nature.

Art. XVI.

The Lost Senses—Deafness. By John Kitto, D.D. (*Knight's Weekly Volume for all Readers.*)—*London*, 1845. 12mo, pp. 206.

This is a unique book. The autobiography of a totally deaf man, of one whose "deafness could not be more intense had the organs conducive to the sense of hearing been altogether wanting." The writer is an author much esteemed, we believe, by biblical critics. His powers of minute observation, and of vivid description, and his vast amount of information are displayed in the notes to 'Knight's Pictorial Bible,' which are his writing, and this little work leaves a strong impression of his intellectual power. We learn from it that he has secured by his own exertions an acknowledged place among men of letters, and that moreover literature is the permanent employment to which he dedicates his life, and that it enables him, like any other successful profession, to support his family. But we pass, at once to his own story.

Dr. Kitto sprang from the very poor. His father was a jobbing mason, in precarious employment. He was slating a house. This son, a boy of 12, who, to the neglect of his school-education, lent "his small assistance," was carrying up a load of slates, and was stepping from the ladder to the roof when his foot slipped and he fell backward, thirty-five feet into the court below. He was stunned. For a moment he was conscious whilst his father, followed by a crowd, was carrying him home; but the succeeding fortnight was a blank in his life, and when he "awoke one morning to consciousness, it was as from a night of sleep." He attempted to spring up in bed, marvelling he had slept so late, and was astonished to find that he could not even move. He was slow in learning that his hearing was entirely gone. His utter prostration overcame any curiosity; and it was not until some time afterwards on inquiring for a book that had

been promised to him, and was impatient at not comprehending the signs which were made to him, that one of his family wrote on a slate the awful words, "you are deaf." Awful, however, rather in retrospect, as he was a child, and knew not the future. "It was well I did not. It was left for time to show me the sad realities of the condition to which I was reduced." The recognized routine of remedies was tried. "They poured into my tortured ears various infusions, hot and cold; they bled me, they blistered me, leeched me, physicked me; and, at last, they put a watch between my teeth, and on finding that I was unable to distinguish the ticking, they gave it up as a bad case, and left me to my fate." Subsequently a seton in the neck, and electricity were tried, but "no good came of it." And with good sense he adds, "Indeed I have not sought any relief; and have discouraged the suggestions of friends who would have had me apply to Dr. This and Dr. That. The condition in which two thirds of life has been passed, has become a habit to me—a part of my physical nature. I have learned to acquiesce in it; and to mould my habits of life according to the conditions which it imposes; and have hence been unwilling to give footing for hopes, and expectations, which I feel in my heart can never be realized."

It was some time before he could leave his bed, and much longer before he could quit his chamber. Reading was his only resource. Before his accident he had been a lover of books; but this long spell at it went far to fix the habit of his future life, and the book he read ('Kirby's Wonderful Magazine,') was well calculated from the strange facts it recorded to draw his attention "to books as a source of interest and a means of information, and this was precisely the sort of feeling proper for drawing me into the habits which have enabled me, under all my privations, to be of some use in my day and generation."

At this period the art of reading was not diffused among the poor. "Many could read; but the acquirement was not in the same degree as now applied to practical purposes. It was regarded more in the light of an occult art—a particular and by no means necessary attainment, specially destined for and appropriate to religious uses, and Sunday occupations." The books at his command were but few and chiefly religious ones. The Bible, which he read quite through; Fox's Book of Martyrs, Josephus, Bunyan, Baxter, Sturm, and a few more. Good books all, "but the thing was you could see no other books than these." Periodical and daily literature, now so abundant had not then flowed over the land. His instinctive taste and an existence solitary and cut off from all social enjoyments by deafness, led him to read much, and his circumstances compelled him to read a few books. Thus by necessity he was obliged to comply with the good rule, to read much in a few good books. He was unconsciously drilling himself in the best habits of mind—application to a few subjects. Winchester or Eton might not have done more for him.

"The day came," he adds, "when I plunged into the sea of general literature, and being able to get nothing more to my mind, read poems, novels, histories, and magazines without end. Another day came, in which I was enabled to gratify a strange predilection for metaphysical books; and with all the novelists, poets, and historians within the reach of my arm, gave my days to Locke, Hartley, Tucker, Reid, Stewart, and Brown. I think little of these things now, and my

taste for them has gone by; but although I now think that my time might have been more advantageously employed, my mind was doubtless thus carried through a very useful discipline, of which I have since reaped the benefit. But amid all this, the theological bias, given by my earlier reading and associations remained; and the time eventually came, when I was enabled to return to it, and indulge it with redoubled ardour; and after that, another time arrived, when I could turn to rich account whatever useful thing I had learned, and whatever talent I had cultivated, however remote such acquirement or cultivation might at first seem removed from any definite pursuits."

Having commenced with this sketch of his early life and mental training, he digests the remainder of his information under such heads as appear to him "calculated to develop the more striking facts and circumstances in the physical, social, and intellectual condition of one who has the chief entrance to his inner being closed."

The first chapter is on "Speech." It is the common opinion that the deaf and dumb are dumb because they are deaf. Our biographer believes from his own experience that there is a physical impediment to speech as well as to hearing. Before his fall his enunciation was remarkably clear and distinct, but after that he spoke with pain and difficulty, so as not to be easily understood, and he was told his "voice had become very similar to that of one born deaf and dumb, but who has been taught to speak."

"Although I have no recollection of physical pain in the act of speaking, I felt the strongest possible indisposition to use my vocal organs. I seemed to labour under a moral disability, which cannot be described by comparison with any disinclination which the reader can be supposed to have experienced. The disinclination which he feels to leave his warm bed in a frosty morning, is nothing to that which I experienced against any exercise of the organs of speech."

The force of this tendency to dumbness was so great that for years he habitually expressed himself in writing, and it was owing to the kindness of two passengers, his companions during a voyage, who,—seeing that dumbness was merely a bad habit, entered into a conspiracy with the captain to understand nothing except what was spoken, that made a wholesome compulsion,—he again expressed himself orally in the ordinary intercourse of life. At first he was understood by strangers with difficulty, but practice has greatly improved his speech. The following remarks were written by a friend on it:

"It is pitched in a far deeper bass tone than is natural to men who have their hearing. There is in it a certain contraction of the throat analogous to wheezing; and altogether it is eminently *guttural*. It may be suspected that this is attributable to the fact that his deafness came on in boyhood, before the voice had assumed its masculine depth. The transition having taken place without the guidance of the ear, was made at random, and without any pains bestowed upon it by those who could hear and correct it."

His tendency is to express words which he has acquired since his deafness as they are written. This renders his English often more intelligible to foreigners than to his countrymen. To his own sensations he seems to speak in a loud whisper, but from the effect on others he finds this is not the case, but that his voice is loud and strange. It may be heard as a sound to an unusual distance, although the articulations can only be made out by a person quite near. He thus graphically describes its effect:—

"If I happen to forget myself, and speak to a companion while others are walking just before us, it is equally annoying and amusing to see the sudden start and abrupt turn of the persons behind whom we walk, at a sound seemingly not of this earth, and so much beyond the range of all ordinary experience."

A friend has described his voice as if formed in the chest, and issuing through a tube without being much modulated by its passage through the mouth,—a definition which agrees well with his own sensations.

This unwillingness to speak is a common attendant on deafness, arising probably from many causes. Dr. Kitto is fully persuaded that in his case there was a physical inability to speak, and he is so accurate and truthful an observer that his impression on this head is good evidence of the fact. As the structure of his vocal organs was perfect before the accident, we have to look at the nerves alone for any physical causes of the inability. The injury to the brain at the origin of the auditory nerves, which produced the deafness, might have extended to the roots of the nerves which supply the muscles engaged in the production of the voice. And as the difficulty seems to be in modulating the voice, and not in producing the sound, the nerves supplying the tongue and lips might have been most jarred. "This physical difficulty in the formation of articulate sounds," of which Dr. Kitto is fully persuaded, however, is explicable on the greatly increased difficulties to the education of the voice which deafness produces. We learn to speak from imitating those we hear. Remove an English child to France, and in a very short time he speaks his own language with difficulty, and even forgets it. A child who suddenly becomes totally deaf is just in the condition to forget his own language without learning another, and if, as in Dr. Kitto's case, his parents' are poor and leave him much to himself and his books, he would in a short time have as much difficulty in speaking his own language as if he had been wholly among those who spoke another tongue. If in addition to this cause it is taken into consideration that he has lost by deafness his chief guide in the modulation of his sounds, and therefore the difficulty of speaking is very much increased, it is not surprising that his unwillingness to speak becomes a confirmed habit, and that in the course of time the difficulty in utterance becomes so great as to require great exertion and perseverance to overcome. Besides, the mutual cooperation of ear and voice in one act is at an end; this harmony is lost, and the pleasure that arises from it (the pleasure which each man derives from his own voice, musical to him, however discordant to his neighbour) is gone. That great spur to conversatiou, the love of approbation, can actuate him no longer from whom all social amusements are cut off, and who is compelled to find his own pleasures, not among his kind, but in the world of silent nature, of books, and of his own thoughts. It would be strange, therefore, if want of practice in speech, with absence of stimulus to its exertion, and of pleasure in its performance, did not very soon lead to an inability in the formation of sounds, as hard to be overcome as a bodily imperfection. Some letters and private memorandums of Beethoven (who, from possessing a most exquisite ear, the delicacy of which, he says, few could imagine, became deaf at thirty) reveal the mental sufferings and mortifications he underwent, which were set down to misanthropy and moroseness. He also avoided speaking as much as possible.

Swift, who became deaf, did not speak more than once during the two last years of his life.

It is a striking proof of Dr. Kitto's powers of observation that he forms a distinct impression in his own mind of the voices of those who speak in his presence.

"The impression thus conveyed is produced from a cursory, but probably very accurate observation of the person's general physical constitution, compared with the action of his mouth, and the play of his muscles in the act of speaking. I form a similar idea concerning the laugh of one person as distinguished from that of another; and when I have seen such a person laugh, the idea concerning his voice becomes in my mind a completed and unalterable fact. The impression thus realized would seem to be generally correct. I have sometimes tested it by describing to another the voices of persons with whom we were both acquainted, and I have not known an instance in which the impression described by me has not been declared to be remarkably accurate. This faculty must be based upon experience acquired during the days of my hearing, and cannot be realized by the born deaf, seeing that it is impossible for them to have any idea of sounds produced by the action of the vocal organs, and still less of the peculiarities by which one voice is distinguished from another."

Dr. Kitto alludes to the power which some deaf persons are said to possess of reading off the words of another from the motions of his lips. A case had been mentioned to him "by an intelligent stationer, of a lady who came to his shop and asked for certain articles in what seemed to him a foreign accent, and who so readily understood and replied to what he said, that he had not the slightest suspicion of her being deaf and dumb. This was, however, a lady of fortune, who had from childhood been the sole object of attention to an experienced governess, which may go far to account for her extraordinary power."

"I can myself," says Dr. Kitto, "make out some single words and short sentences in this way, when I know that what is said must be one of three or four things. But I am thoroughly persuaded that this mouth-reading must be wholly inadequate to the purposes of *real* conversation, involving intercourse of the intellect and imagination."

The next subject is "Percussion." The loudest thunder is perfectly inaudible to Dr. Kitto; beneath the full peal of a magnificent set of bells he is unconscious of any sound; he has been in a besieged city, but heard neither the cannon nor the shells. But there is in his whole frame a peculiar sensibility to the slightest percussion, producing a sense of vibration, pervading the whole frame "to the very bones and marrow."

"When a percussion of any kind takes place upon the very framework with which my standing-point is connected, the sensation is powerful, and the intensity is in the ratio of the closeness of the connexion. The drawing of furniture, as tables or sofas, over the floor above or below me, the shutting of doors, and the feet of children at play, distress me far more than the same causes would do if I were in actual possession of my hearing. By being to me unattended by any circumstances or preliminaries, they startle dreadfully; and by the vibration being diffused from the feet over the whole body, they shake the whole nervous system, in a way which even long use has not enabled me to bear. The moving of a table is to me more than to the reader would be the combined noise and vibration of a mail-coach drawn over a wooden floor; the feet of children like the tramp of horses upon the same floor; and the shutting of a door like a thunder-clap, shaking the very house."

His wife has observed that the lightest foot-fall upon the same floor is quite sufficient to attract his attention and even to rouse him from sleep. He is not conscious of knockings at the door however violent, while the shuttings of the door are painfully distinct.

"The reason of this is obvious; the valve of the door on which the percussion is made by knocking, is a detached frame of wood hung upon hinges, and the vibration is therefore comparatively isolated, and not propagated throughout the frame of the house, as is the case when, in shutting the door, the valve itself strikes upon the doorpost, which is identified with the framework of the building."

This sensation gives him no direction as to the place from which the sound proceeds; whether it is on the same floor, or above or below it. If a thimble, pencil, or even a more minute object falls from the table to the floor, he is often aware of it when others sitting at the same table have not been apprized of it by the ear. The greater number the points of contact with the floor the stronger the impressions, hence they are more distinct when he sits than when he stands.

"And when the chair itself in which I am seated has been subject to the percussion, the sensation is such as baffles description. For instance; a few days since, when I was seated with the back of my chair facing a chiffonnier, the door of this receptacle was opened by some one, and swung back so as to touch my chair. The touch could not but have been slight, but to me the concussion was dreadful, and almost made me scream with the surprise and pain—the sensation being very similar to that which a heavy person feels on touching the ground, when he has jumped from a higher place than he ought. Even this concussion, to me so violent and distressing, had not been noticed by any one in the room but myself."

Although of course insensible to the sound of music, he has derived agreeable sensations occasionally from the vibrations of stringed instruments, and especially from the higher notes of the piano.

"I found that the notes were the most distinct to me when the points of my finger-nails rested upon the cover, and still more when the cover over the wires was raised, and my fingers rested on the wood over which the wires are stretched, and to which they are attached."

He has not cultivated this means of imperfect gratification. When blowing in wind instruments he is not aware of any sound.

We have given Dr. Kitto's experience on percussions fully, and in his own lucid language, as it is of much interest as he describes a class of symptoms which are we suspect very common, with an accuracy and minuteness which is original. Since reading them we are reminded of similar complaints from these causes made by more than one deaf person, whose nervous system was delicately strung; often giving rise to difficulties in explaining their consciousness as to some trifling sounds, with their total incapacity in hearing louder ones. Another, and perhaps a larger class of invalids are very subject to the same kind of annoying impressions, those suffering from nervous complaints, women especially, whose originally delicate nervous organizations are rendered more susceptible by disease. Any jars from doors or footsteps are to such the source of positive suffering which they cannot control; but as, at the same time, their sense of hearing is often morbidly acute, the two classes of effects have not been hitherto accurately separated, all the annoyance perhaps being

attributed to the sound when the jar of the easily-moved nervous system was the chief source of wretched feeling. The sufferers themselves are often well aware of the two causes.

We next come to his observations on *sight*. That the loss of one sense is compensated by the improvement in another, is not true physically in Dr. Kitto's case, unless his increased susceptibility to percussions may be so reckoned. His eyesight was not rendered more acute nor lasting. But mentally there is a compensation, and Dr. Kitto's close observation of his own mind, has enabled him to realize and to explain the effects upon it of the loss of one sense, and these materials towards the "philosophy of deafness" we gladly use. In the first place, it has developed "a sense of the beautiful in nature and art, and a love for it, a passionate love—which has been a source of his most deep and pleasurable emotions." This was owing to no æsthetic cultivation; no one suggested to him that such and such things were beautiful. His taste therefore "was more of the nature of instincts." It began to show itself soon after his accident "in a rapidly increasing admiration, and love of whatever gratified the eye, and a more intense abomination of whatever displeased it." The latter feelings were chiefly confined to "dead animals, especially as exhibited in shambles, and to persons deformed, or exhibiting in their countenances traits or expressions which I did not approve. This feeling became at length almost morbid; and I felt thoroughly miserable when in the same room with an ugly old woman, or with a man exhibiting distorted or imperfect features, labouring under any sinister or malignant expression in his countenance. I used to feel a strong inclination to fly at them, and to drive them from me." His range of pleasurable sensations were much greater. The moon was an object of his just idolatry.

"I have no recollection of earlier emotions connected with the beautiful, than those of which the moon was the object. How often, some two or three years after my affliction, did I not wander forth upon the hills, for no other purpose in the world, than to enjoy and feed upon the emotions connected with the sense of the beautiful in nature. It gladdened me, it filled my heart, I knew not why or how, to view the 'great and wide sea,' the wooded mountain, and even the silent tomb, under that pale radiance. This is one of the enjoyments of youth, which has not yet passed away. After this, I do not know that any class of objects in nature has acted so strongly upon my sense of the beautiful, or perhaps I should say of the sublime, as mountains. For to me high mountains were a feeling, from the time that I first gazed upon the glory of the Grenada mountains, as the sun cast his setting beams upon their tops, to that in which I caught the Titanic shadow of Etna in the horizon, or spent my days among the glories of the Caucasus, or wondered at the cloudy ring of Demavend, or mused day by day upon the dread magnificence of Ararat."

His enjoyment of trees was a rather later pleasure. There were but few in his native place by the sea-side, which he did not quit until twenty years of age. Travel perfected this enjoyment. The magnificent woods of England "called into full activity that perception of beauty in trees, which afterwards ministered greatly to my enjoyment as I travelled among the endless fir woods of northern Europe, and the magnificent plane trees of Media, and dwelt amidst the splendid palm groves of the Tigris." Since then I have seldom enjoyed serenity of mind in any house, from which a view of some tree or trees could not be commanded." His residence has been chosen with this view, and his study selected—

"Not for the room that might be in itself the most convenient, but the one from the window of which my view might, with the least effort, rest upon trees, whenever the eyes were raised from the book I read, or from the paper on which I wrote. In all cases, even the stillness of a tree has been pleasing to me; and the life of a tree, the waving of its body in the wind, or the vibration of its leaves and branchlets in the breeze, has been a positive enjoyment, a gentle excitement, under which I could have rested for hours. This strong feeling has enabled me to understand better than I otherwise might, the curious and often beautiful superstitions and idolatries which were associated with trees in the ancient times; and I have understood better than Aelian the class of associations which may have induced the Persian king to present the glorious plane near Sardis, with costly gifts, and to deck it with the ornaments of a bride."

That one thus thrown on his eye-sight for his pleasures should have early delighted in pictures, should have studied and got by heart each print in the scanty book-shop windows of his native place, and have subsequently found in the print-shops of London endless gratification, and that the National Gallery should have afforded him many happy hours, when in feasting his eye and imagination he purified his tastes, and strengthened his perceptions, is to be expected. That he is an accurate physiognomist at first sight is probable enough also, even although he is unskilled in the science of Lavater or Spurzheim, and the busy crowded thoroughfares and large assemblies furnish him with living pictures which he delights in scrutinizing. Amongst the "masses of ill-compacted matter, which has been cut up to form the aggregate of the insipid and characterless faces which crowd our streets," some one face really beautiful or ugly, really striking or repulsive, may be found to recompense the toil. Another compensation in his loss is the distinctness and permanence with which images received through the eye are impressed on his mind, and which as he suspects may be greatly owing to the eye being the sole channel through which images of objects have access to his brain.

"It thus happens that my mind retains a most distinct and minute impression of every circumstance in which, at the time of occurrence, I felt the slightest degree of interest; of every person whom I have at any time during the last twenty-eight years regarded with more than casual observation; and of every scene upon which, during frequent and long-continued change of place, I bestowed more than cursory notice."

By "cursory notice," he means seeing things without looking at them. This faculty is of much use and solace, for he calls up scenes with the *whole* of their circumstances.

"If I wish to recollect a person, along with him comes all the scenery amidst which I beheld him, and all the persons who were at the same time associated with him; and so, in like manner, if I wish to realize a scene, to conjure up the view of a place, it comes before me with the very persons I saw in it, and in all the identifying circumstances of form, and feature, and relative position. In actual travel I was loth to trust to a faculty which had not been sufficiently tried. I therefore diligently wrote up my travelling journals day by day. But although I had much occasion for the literary use of the facts and observations thus obtained and preserved, I have had scarcely any need to *refer* to these journals, seeing that whatever I wished to recollect became at once present to my mind in all its accessories and circumstances."

This distinctness cannot be referred to journalizing, as the recollections are equally clear as to one part, of 500 miles, where no journal was kept, and for home scenes.

That the power of recollection and of bringing up scenes so vividly and minutely, is a very uncommon one as to degree, self-observation, as well as observations of the impressions retained by others, daily and hourly testifies. Dr. Kitto's descriptions of the scenes, manners, and people of the east struck us before reading this autobiography, from their vividness, minuteness, freshness, and fulness. The impression was, "this is not a compilation, the writer has been at the spot with a keen practised eye." Such a power to such a degree, enabling him to make his mind "a temple for all lovely forms," seems almost a sufficient compensation for his loss. But the acuteness of sensibility which is the attendant of such gifts, renders their possessor more alive to his own deficiencies. Dr. Kitto does not shirk this painful subject, but next passes to his "disqualifications." This part, admirable though it be and worth reading and reflecting upon by all who are struggling onwards, is not so immediately related to the subjects we discuss as to admit our dwelling fully upon it. Even if it were, we could not analyse or compress what is so compact, and full of thoughts in consequence. The book is but a shilling. Buy it. Twelve pence is not ill spent, in seeing how a vigorous mind struggles through difficulties, all but insuperable; that makes its own way, not only through poverty, and want of sympathy, and absence of appreciation, and actual discouragement from well-intentioned friends, but also in spite of a physical defect which increased these hinderances to success a hundredfold, until it attained its right position among its fellows, and thereby content, bearing witness that "there is no one so low but that he may rise; no condition so cast down as to be really hopeless; and no privation which need of itself shut out any man from the paths of honorable exertion, or from the hope of usefulness in life."

To trace his progress, to see his pure love of knowledge for its own sake, driving him on and keeping him up to constant exertion with a tension which needed none of the stimulus of prizes, or even of fellow-feeling, to see him humbly submitting to employments against his tastes, and in which he could not place his heart, but which never led him astray from his one object, until at last, casting all other cares aside, and determining at all risks to act upon his "soul-felt conclusions, and to stand by the truth or fall by the error of his ineradicable convictions," and to learn that he has succeeded, must do all readers good. For his success has been won by unremitting toil, and his own strong instincts led him not to the mere gratification of his tastes, but into a path of earnest and useful labour, which he felt he had been prepared for walking in.

Dr. Kitto communicates with his own household, and with all those with whom he is on terms of personal intercourse, by finger-talking. Those interested either for themselves or for others in this art, would do well to consult this book, as his hints as to its practice must be useful. Of signs he has personally a great aversion, from their attracting the attention and curiosity of others, but he is fully convinced, that signs are the natural language of the deaf and dumb, and that time is thrown away in teaching them to speak. A difference of opinion exists on this point. The Abbé Sicard taught few of his pupils to speak. They have a great aversion to it, whilst they communicate with each other by signs, with an hilarity and "abandon" very exhilarating to witness. As Dr. Kitto observes, they must ever be cut off from *oral* intercourse with society, and

that "education therefore is the best which creates the highest appetite for books, and which, by conferring a complete mastery of written language, not only gives them as much intercourse with the minds of others as they are capable of enjoying, but opens up to them the wide world of facts and ideas which books contain."

His chapter on "society" is excellent. Full of nice discernment, genuine modesty, an acute appreciation of what is due to others, with indications of shyness and reserve, which his early life, studious habits, and his malady must have encouraged. With the full sense of the terrible privation he endures, and with every evidence from his entering into the subject minutely, that he has sharply felt its misery, yet the humour which everywhere gleams and enlivens his narrative, shows that he has neither become misanthropical, cynical, nor even stoical. But with manly good sense has learned, that the true art of enduring, is in submitting unconditionally to the will of Providence. He very judiciously says,

"It is surely a social duty in the deaf to avoid company, in the assurance that by going into it, or gathering it around him, he is only a stumbling-block to the pleasures of others, and is only laying up for himself a store of mortification and regret for those terrible disqualifications, which, in the solitude of his chamber, or in the presence of his trained domestic circle, he may half forget."

Dr. Kitto after concluding his own history, comments on that of Massieu, (the celebrated deaf-and-dumb pupil of the Abbé Sicard,) whose beautiful definition of gratitude, "the memory of the heart," has immortalized him. The following questions were put to him:

"Did you know what it was to hear?

"Yes.

"How had you learned that?

"A hearing female relative told me that she saw with her ears a person whom she could not see with her eyes; who was coming to my father. The hearing *see with their ears* during the night a person who is walking."

This answer is very analogous to that of the blind man, who explained his notion of scarlet, by comparing it to the sound of a trumpet. The blind man comparing a sight to a sound, whilst the deaf man compared sounds to sights. As Dr. Kitto says, the blind believe that sight is a kind of ocular hearing; and the deaf, that hearing is a kind of auricular sight. Dr. Kitto thinks, that the deaf-and-dumb believe there is a certain emission from the lips in motion, visible to others and not to themselves.

"Meanwhile it is certain that no *proper* idea of a sound can be entertained by those who never heard it, whatever be the degree of their education. The nearest approach to such an idea which they can possibly reach, is through the class of hitherto undescribed sensations which I have endeavoured to explain in the chapter on percussions."

This strikes us as a very acute and true remark, and a new one. Physiology confirms it, as both sensations depend on vibrations.

In bringing to an end our analysis of this 'lay' book, we trust that the pleasure we have taken in analysing it, will be conveyed in some measure to our readers, and will induce them to peruse the little volume itself. In a purely scientific point of view, it contains original matter, the result of the careful observation and the personal experience of many years, by one who can observe, and vividly convey his impressions. The remarks on percussions, as well as on the effect of the loss of hearing on the development

of those mental tastes which depend on the eye, and the high spiritual compensation thus afforded, rather than a physical one in the mere strengthening of the power of other senses, are worthy the attention both of the physiologist and metaphysician, and are proofs of the principle which in this review has been always strenuously insisted on, that the two studies should be united in order to throw any new light on that deep branch of inquiry. But besides its scientific value, another lesson may be incidentally learned from it;—the history of a manly, honest, earnest mind, pursuing knowledge from the pure love of it, without any ulterior view, and under difficulties seemingly overwhelming, and this perseverance ending (as perseverance does) in success, although that success could not have been foreseen, is good as an example in a profession like our own, where the scientific objects are so high, the difficulties to be overcome so numerous, and the attractions to take a shorter and easier road than persevering labour points out, are so numerous and seducing. The strong will, the self-dependence gradually growing out of and based firmly (as the event proved) on an accurate self-knowledge, and not on an inflated estimate of his own capacities; the freedom from vanity which this shows, and with it genuine humility, and self-distrust under new circumstances which may happen, and of which he has had no experience, and the feeling of satisfaction and content, his honest pride in having used his success to his own exertions, and yet his submission to, and acknowledgment of a guiding Providence, are traits of a character which cannot be studied without the conviction that it is a high one, under a wish (which may not be barren) that such was the reader's own.

ART. XV.

A practical Treatise on Healthy Skin: with Rules for the Medical and Domestic Treatment of Cutaneous Diseases. By ERASMUS WILSON, F.R.S., Lecturer on Anatomy and Physiology in the Middlesex Hospital, &c. &c. Illustrated with Six Steel Engravings.—*London*, 1845. Small 8vo, pp. 356.

"IN the following pages," commences our author, "I propose to make my reader acquainted with the structure and uses of the skin, in the hope of awakening his attention to the necessity and manner of training it to the purposes of health. I trust, moreover, by laying down correct and simple laws, to enable him to comprehend the principles upon which a sound and effective domestic treatment of its diseases may be conducted." We must confess ourselves to be rather at a loss to know, whether Mr. Wilson's objects be to address the general public alone, or whether he aims at including his professional brethren among his readers. If the former, we must say that we cannot find sufficient reason for selecting an isolated portion of general hygiène, treating it at such length and with such copiousness, and especially for appending to it so much information of a purely medical kind, though conveyed in language anything but professional;—except in those unworthy motives, which we have always felt it our duty to expose and condemn, from which we believe Mr. Wilson to be altogether exempt. If the latter, as would appear from the details here and there interspersed on some departments to which Mr. Wilson has devoted peculiar attention, as well as from the combination of rules

for the medical treatment, with those for the domestic management, of cutaneous diseases, we think that Mr. Wilson would have done much better to put his Exposition into a more professional form. We believe that there are many amongst his seniors, as well as his juniors, who would have gladly welcomed such a work, embracing the results of the continued attention which he has given to the subject, since the publication of his more formal Treatise, and devoted rather to the prophylaxis and the general management, than to the classification and particular treatment; of these ill-understood and most troublesome maladies.

The general management of the skin has been ably treated by many previous writers on popular hygiène; and particularly by Dr. Andrew Combe, whose chapter on this subject contains almost every direction of real importance. If the brain, the lungs, the heart, the stomach, the liver, the kidneys, &c. &c., were each treated in a separate volume, with the copiousness of illustration which Mr. Wilson has bestowed on his favorite organ, the Skin, the public would be in possession of a complete encyclopædical library of hygiène, which would not, we apprehend, be nearly as useful as Dr. A. Combe's comprehensive treatise; on account of both the expense and the time required for acquaintance with its contents. In some respects, however, the present publication is well timed: for it may serve to forward the "bath and wash-house" movement; which, combined with those which are taking place of early shop-closing, of draining and ventilation, and of public places of recreation, we look upon as among the most pleasing and favorable of the "signs of the times." If such be Mr. Wilson's purpose, we are ready to accept his contribution to the general impulse that is now operating so beneficially on the public mind, as one of no mean value, but we would have been better pleased if he had given a more prominent expression of his intentions, and had omitted many passages that appear to us to have somewhat of a too popular bearing.

We shall now extract, as specimens of the mode in which the different subjects are handled, a few passages which may, at the same time, communicate useful information to our readers. After a chapter on the scarf-skin, and another on the true skin, which contain clear and accurate descriptions of those structures, couched in popular language, our author proceeds to the perspiratory system; which he has investigated with particular minuteness.

"Taken separately, the little perspiratory tube, with its appended gland, is calculated to awaken in the mind very little idea of the importance of the system to which it belongs; but when the vast numbers of similar organs composing this system are considered, we are led to form some notion, however imperfect, of their probable influence on the health and comfort of the individual. I use the words 'imperfect notion' advisedly; for the reality surpasses imagination, and almost belief. To arrive at something like an estimate of the value of the perspiratory system, in relation to the rest of the organism, I counted the perspiratory pores on the palm of the hand, and found 3528 in a square inch. Now, each of these pores being the aperture of a little tube of about a quarter of an inch long, it follows that in a square inch of skin on the palm of the hand, there exists a length of tube equal to 882 inches, or 73½ feet. Surely such an amount of drainage as 73 feet in every square inch of skin, assuming this to be the average for the whole body, is something wonderful; and the thought naturally intrudes itself, What if this drainage were obstructed? Could we need a stronger argu-

ment for enforcing the necessity of attention to the skin? On the pulps (?) of the fingers, where the ridges of the sensitive layer of the true skin are somewhat finer than in the palm of the hand, the number of pores on a square inch a little exceeded that of the palm; and on the heel, where the ridges are coarser, the number of pores on the square inch was 2268, and the length of tube 567 inches, or 47 feet. To obtain an estimate of the length of tube of the perspiratory system of the whole surface of the body, I think that 2800 might be taken as a fair average of the number of pores in the square inch; and 700, consequently, of the number of inches in length. Now, *the number of square inches of surface in a man of ordinary height and bulk is* 2500; *the number of pores, therefore,* 7,000,000, *and the number of inches of perspiratory tube* 1,750,000, *that is,* 145,833 *feet, or* 48,600 *yards, or nearly* 28 *miles*. [Note. To the medical reader it may be necessary to explain, that the sebaceous system is included in the system of perspiratory glands and tubes in this calculation. I have ascertained beyond question, that the sebaceous system is the perspiratory apparatus of the greater part of the body, the true perspiratory glands and tubes being found only in certain parts. Therefore, the calculation which I have made on these premises must be considered as falling within rather than beyond the truth."] (p. 43.)

The next chapter, "On the oil-glands of the skin," contains an account of the sebaceous follicles, which differs in some degree from that which is generally received, and which is probably more accurate.

"The apparatus for keeping the surface of the skin bedewed with an oily fluid, resembles, in general particulars of structure and economy, that of the perspiratory system. It consists of minute tubes, which traverse the scarf and sensitive skin, and enter the substance of the corium, where they terminate in small glands. These tubes are similar in structure to the perspiratory ducts, being composed of three layers, derived respectively from the scarf-skin which lines their interior; the sensitive skin, which is the medium of distribution of their vessels and nerves; and the corium, with its fibres, which gives them strength and support. Like the perspiratory tubes, they are in some situations spiral, but this is not a constant feature; more frequently they pass directly to their destination, and they are also larger. The chief characters in which they differ from the perspiratory apparatus are, the straightness and greater diameter of their tubes, their absence in certain situations, as on the palm and sole, and abundance in others where their office is more needful, as on the face and nose, the head, the ears, &c., and the degree of complication in the structure of their glands. This latter character is sufficiently remarkable, since they offer every shade of complication, from the simple straight tube, to a tube divided into numberless ramifications, and constituting a little rounded arborescent mass, of about the size of a millet seed." (p. 56.)

Mr. Wilson considers that the healthy action of the sebaceous system is peculiarly interfered with by the "sedentary and irregular habits of refined society." Instead of being completed according to the normal standard, the secreting operations are irregular and torpid, the contents of the cells unnaturally solid and dense; they are only partly or not at all emptied, and are either thrown out in a mass upon the surface of the skin, or, if too dense and dry for this mode of escape, they collect in the tube of the gland, and produce an unnatural distension of it, which frequently gives rise to inflammation. It is in this condition of the sebaceous system, that the curious parasite, first discovered by Dr. Simon of Berlin, and subsequently investigated by Mr. Wilson, appears most disposed to lodgment and multiplication within the oil-tubes.*

* We cannot help noticing Mr. Wilson's defence of the title, which he has bestowed upon this animal, as showing a degree of disregard for the rules of zoological nomenclature, which we should scarcely have expected from him. Having designated it as the *Entozoon folliculorum*, and having

"The animalcule of the skin is found in the oil-tubes, whenever there exists any disposition to the unnatural accumulation of their contents; it is found in numbers, varying from one or two to twenty, in the interior of the little grooved cylinder, which is squeezed out by the pressure of the fingers; and this in an apparently perfect state of the health of the skin, cannot be said to be in perfect health when its functions are performed in a torpid manner. Now, as in the majority of mankind, and certainly in all the inhabitants of cities and large towns, the skin is more or less torpid in its functions, so the presence of this animal in the skin is the rule, its absence the exception. I have found it in all ages, from youth to old age, more numerously, it is true, at the latter than the former period, and in great and remarkable numbers during sickness. Under these circumstances I see no other conclusion open, than to assume that it performs some beneficent purpose in the economy of the skin; that purpose being, according to my belief, the disintegration of the over-distended cells, the impression of a new condition upon the contents of the cells, and the stimulation of their tubes to perform their office more efficiently. In corroboration of this view is the fact, that these little creatures increase in number when the vital powers decline, so that when the energies of the system are reduced by disease, and when the skin, participating in that reduction, is unable alone to fulfil its functions correctly, these little beings are produced to aid it in the work." (p. 62.)

This is certainly a somewhat favorable view of the purposes of this extensive parasitism; but we see no sufficient evidence in its favour; since the mere fact of multiplication of individuals under circumstances favorable to their existence, indicates nothing as to the final cause of their production. Doubtless it will be highly gratifying and instructive to the public mind, to be made acquainted with the fact, that the habits of "refined society" tend just as strongly to the production of this tribe of parasites, as the habits of the "great unwashed" favour the development of certain others, which are not in themselves one whit more disgusting. Here, as elsewhere, we see how nature manages to compensate one class for the exemption they may enjoy from the evils incident to another. The delicate town-bred lady of fashion, in descending from her carriage, shrinks instinctively from the mass of rags, filth, and vermin, which is brought

referred to the fact that the word entozoon is now used to distinguish a *class* of animals, he nevertheless is bold enough to add, "I do not think this an objection to its use as a generic appellation; and the specific name which follows renders any mistake impossible." (p. 61, note.) Such a method of nomenclature, carried to its legitimate extent, would produce inextricable confusion, and endless absurdities. Fancy our calling the window-swallow *Avis fenestrarum*; or the bed-bug *Insectum lectularium*. In Mr. Wilson's detailed account of the animal, published in the Philosophical Transactions for 1844, the generic name *Entozoon* was only suggested as provisional; the class, to which the animal should be referred, being still a matter of question. We should not have noticed the subject here, had it not been that the error of nomenclature is not only continued, but defended, by Mr. Wilson. And the error is the *greater*, because it is acknowledged on all hands, that the animal is *not* one of the class entozoa, being raised by its complexity of structure far above that group; so that it is very much as though Mr. Wilson were to christen a bat by the title of *Avis speluncarum*, because it flies like a bird, and lives in caves. The fact is, that the parasite in question is one of those animals which sometimes present themselves, to the perplexity of the systematic zoologist, but to the satisfaction of the philosophical student of nature who regards classes, orders, &c., as a machinery of human invention, adapted to serve the purpose of facilitating the acquisition of knowledge, but as having no real existence in creation. It unites the characters of the higher annelida or worm-tribe, of the lower arachnida or spider-tribe, and of the lower crustacea or crab-tribe, to such a degree, that it is yet doubtful which class ought to afford it a place. Its structure and history have been so ably elucidated by Mr. Wilson in the paper we have referred to, that little probably remains to be added to the information he has collected; and the difficulty in assigning its zoological position lies rather in our imperfect acquaintance with the nearest-allied forms among the classes just named. The investigator of the history of this parasite need certainly be in no lack of *subjects* for examination; for, strange to say, it is one of the most readily procurable of all living beings, especially in a town population.

into contiguity with her precious person by some pertinacious beggar; ignorant all the while that her sebaceous follicles give board and lodging to a host of parasites, whose number may equal that of the various kinds of "small deer" that nestle in the matted hair and tattered garments of the fellow-being whom she regards with such loathing. "Where ignorance is bliss, 'tis folly to be wise;" and some, perhaps, of Mr. Wilson's fair readers may not thank him for enlightening them on the subject. But we consider that such knowledge ought not to be withheld from deference to fastidious delicacy; and that, besides the immediate inducement it affords to the adoption of such habits as may free the sebaceous system of those unwelcome inhabitants, it may also have the beneficial *moral* use, of aiding in the demolition of those barriers between the classes of society on which we are thankful that every day is now making some fresh inroads.

The structure and development of the hairs, constitute the next subject of discussion; and these are described in accordance with the most recent microscopical observations. We cannot but think, however, that the comparison of the structure of the human hair with that of other animals might have been carried out a little farther, with much interest to the reader; and that it might have advantageously superseded the detailed account, than which nothing could possibly (in our estimation) have been more dry and purposeless, of the direction or *set* of the hairs on different parts of the body, occupying between *five* and *six* pages. This variety in the direction of the hairs can only be well observed in the down of the new-born infant; as the hairs soon cease to be developed on the greater portion of the surface, except in occasional instances. This down must be regarded as a rudimentary condition of the complete hairy covering of the lower mammalia, and particularly of the monkey tribe, in which the *set* of the hairs is obviously a matter of consequence, even serving in one instance to distinguish species that are otherwise nearly allied. But in the human species it cannot be regarded as of the least import; as the character disappears so early, and is restricted to hairs of such minuteness. The following observation is new, so far as we know, and of some interest.

"The short hairs of the skin are not unfrequently disturbed in their growth by a cause previously referred to in connexion with the hairs of the head; namely, deficient oleaginous qualities of the product of the oil-tubes. When this happens, the dry scales and cells of the oil and hair tubes collect at the aperture of the latter, and become a source of impediment of growth to the hair, which, as a consequence, assumes a coiled and twisted appearance. But sometimes this closure of the hair-pore takes place after the fall of the old, and previously to the growth of the young hair, in which case the latter is imprisoned in the tube, and there grows, although unable to escape. I have occasionally seen nearly the whole of the hairs of a limb thus imprisoned, and forming little spiral circles, (can such things be?) which are visible through the then horny scale of desiccated cells, which covers them, and keeps them down. The obstruction occasions, as may be anticipated, a good deal of itching and uneasy feeling in the skin, and is more or less alleviated by tearing up the filmy covering." (p. 80.)

The itching of the dry scalp is referred by Mr. Wilson to a similar cause; the hair not being positively kept in, but being retarded, by the accumulation of the hard unctuous matter poured into the tube; and being thus caused to make pressure on the bulb at its base, which is supplied with sensory nerves. "A natural remedy for the unpleasant sensation is at

hand; the nail is conveyed to the seat of inconvenience, it disturbs the impacted matter at the aperture of the tube, probably dislodges it, and the hair resumes its accustomed state." Prevention is better than cure, however, in this and other cases; and the proper use of soap and towel, comb and brush, will generally suffice.

It seems from the following observations that we have more hairs about our persons than we are commonly aware of:

"The invisible or downy hairs of the body rarely appear above the level of the skin, for when they do they necessarily fall into the category of the short hairs, and may best illustrate them by referring to a position in which their presence is invariable, although seen in that situation as an exceptional occurrence: I allude to the nose. The nose is ordinarily bald; but if the unctuous product of the oil-gland be squeezed out of its tube, and examined beneath the microscope, one or more of these little hairs will constantly be detected in the centre of the mass. Indeed, when the unctuous matter has been detained in the oil-tube for any length of time, the number of hairs may be considerable, as, for example, twenty, thirty, or even forty. Now, the whole of these hairs have, as far as we at present know, been produced by a single pulp, and having attained maturity, have been shed, to be carried out of the oil-tube with the unctuous substance; but the latter being retained, they have had time to collect, as we have seen, in astonishing numbers. I should be inclined to infer, from this circumstance, that these little hairs grow very quickly, and are shed at short intervals of time. On no other hypothesis can their numbers be satisfactorily explained. In their normal state and position, these little hairs are colourless and transparent, having rounded blunt points and brush-like roots; but under the influence of augmented action of the skin, they are susceptible of growth to a considerable extent, both in length and bulk, in fact, of becoming equal in dimensions to the short hairs of the body. Of this we have an example in the occasional growth of visible hairs upon the nose." (p. 82.)

Mr. Wilson expresses himself with great incredulity as to the rapid change in the colour of the hair, which has been alleged to take place not unfrequently, especially under the influence of strong mental emotion; and not content with expressing his entire discredit of the statements on record, as to the occurrence of such a change in a single *night*, he does not admit that even in a whole *week* anything more can take place than a whitening at the roots of the hairs. In fact, he considers it impossible, that the part of the hair which has once emerged from the hair tube can be affected by any subsequent changes in the condition of the body. We must confess that we cannot share with him in this incredulity; since there is nothing to our minds so imposssible, in the transmission of a chemical agent capable of effecting the change, from the base to the tip of the hair. What we know of the nutrition of other extra-vascular tissues fully warrants this idea; and it is further sanctioned by the fact, that the change in the colour of the entire fur, which takes place in some animals under the influence of cold, may be effected, under peculiar circumstances, within the time specified by Mr. Wilson. Thus a lemming, caught in the summer during one of the Arctic voyages, and kept in the cabin of a ship at a warm temperature, retained its summer coat far beyond the usual time; but when taken on deck on the 1st of February, and exposed to the intense cold of 30° below zero, the colour of its fur began to change in a few hours, and it was turned to a pure white in the course of a week. Now as we have no reason to believe that the direct influence of cold upon the pigmentary

matter of the hair could produce any such effect, we must attribute the change to some chemical influence propagated from the bulb. It would be interesting, however, for the sake of obtaining satisfactory proof of this position, to repeat this experiment, with the additional test of exposing a small quantity of hair, cut off before the animal had been subjected to a change of temperature, to the same degree of cold. If this should remain unaffected in hue, it is evident that the alteration must really take its origin in the growing point of the hair.

We cannot regard Mr. Wilson as justified in expressing his complete incredulity of the statement of Bichât, that he has seen hairs growing from mucous membranes. We cannot regard the assertion as by any means destitute of *à priori* probability, the relation of the skin and mucous membrane being kept in view; and we see no reason why the evidence of an observer so well qualified as Bichât should be set aside so unceremoniously.

The chapters on the maintenance of the health of the skin almost constitute a complete system of hygiène; the precepts given in regard to diet, exercise, clothing, &c., being just such as would lead to the maintenance of the health of the body in general, and therefore that of every one of its component organs. Therefore, the author's warnings aginst over-feeding, against the injurious practice of "converting the inside into a medicine-chest," with the view of expelling one evil by the introduction of another; against excessive, improper, or insufficient clothing; against excessive or insufficient exercise, &c. &c., all tell as much on the health of the stomach, the liver, &c., and numerous other organs, as on that of the skin. This we consider to be one of the unfavorable results of the isolation of this subject from general hygiène. The recommendations in regard to ablution as a prophylactic, and to the more systematic use of water in the cure of disease, fairly belong, however, to this department of the special hygiène. Mr. Wilson's remarks on these subjects are written with force and spirit; and his chapter on hydropathy contains what appears to us a fairer view of the merits of that method of treatment, than we have elsewhere seen from the pen of a regular medical practitioner. The following is its commencement:

"The advantages to health of a judicious and sound system of diet, clothing, exercise, and ablution, cannot be better illustrated than by reference to what has been termed 'the water-cure.' The water-practice has effected important results

lepra, may be undertaken by unqualified domestic practitioners, appears to us a very injudicious, not to say dangerous measure.

When we next meet with Mr. Wilson we hope that it will be upon less doubtful ground. We shall with pleasure welcome him back to his own proper path,—that of accurate observation and clear description.

Art. XVIII.

1. *Saggio Illustrativo le Tavole della Statistica Medica delle Maremme Toscane. Compilata per ordine di S.A.R. il Gran-Duca di Toscana.* Da Antonio Salvagnoli-Marchetti Medico ispettore della provincia di Grosseto.—*Firenze*, 1844.

Essay illustrating the Tables of the Medical Statistics of the Tuscan Maremma. Compiled by order of H.R.H. the Grand Duke of Tuscany. By Antonio Salvagnoli-Marchetti, Medical Inspector of the Province of Grosseto.—*Florence*, 1844. Folio pp. 89.

2. *Versuch einer Medicinischen Topographie und Statistik von Berlin.* Von Dr. H. Wollheim, praktischem Arzte, Wundarzte und Geburtshelfer. Mit einem Vorworte von Dr. J. L. Casper.—*Berlin*, 1844.

Medical Topography and Statistics of Berlin. By Dr. H. Wollheim, with a Preface by Dr. J. L. Casper.—*Berlin*, 1844.

We unite our notice of these works because they belong to one class, not because their subjects correspond.

I. The first work before us is one of a class which we are happy to say has greatly increased both in number and value during the last few years; recording the results of the application of medical science in ameliorating the condition of a people, reforming their habits, and not only remedying their diseases, but pointing out the means of avoiding them; thereby converting a miserable and unhealthy population, into a happy and prosperous community. Perhaps our author has been too intent upon his statement of what the country *is*, to give the necessary attention to the question of what it may *become* under scientific management, but his work still points out the evils to be remedied and their probable causes, which is a great step towards their removal.

The Tuscan Maremma, the principal part of which is the province of Grosseto, comprehends a large extent of territory, partly mountainous, partly hilly, and partly consisting of beautiful and very extensive plains. Its extent is 1,439,999 square *agrarj*, about 1,050,000 of which are hilly or mountainous, 380,000 are plains. Rivers, lakes, and water-courses occupy 54,000, and 344,022 are wooded lands. It lies on the shore of the Mediterranean from San Vincenzio to the river Chiarone, the boundary of the Papal States. Its elevation above the level of the sea varies greatly; part is at the same level, while one mountain is 5298 feet, and the summit of another 3211 above it. The insalubrity of the air is the greatest in the plains, slight among the hills, and altogether imperceptible in the mountains. Its effects are manifested, not only in the plains on the sea-shore, but also inland, along the courses of the rivers and in their prox-

imity. The effects of the malaria are felt in villages, 714 and 975 feet above the level of the sea, while some towns at the sea level and along the coast are perfectly healthy.

The province of Grosseto lies between 28° 12′ to 29° 6′ longitude, and 42° 22′ to 43° 6′ latitude. The climate varies with the elevation of the district. Twenty-two years' observation have shown, that at sun-rise and mid-day the thermometer is two degrees higher than at Florence in winter, and in summer one degree lower. Scarcely any difference is observed in the barometer. In April and May, the thermometer rises nine degrees in the three hours following sun-rise. The hygrometer exceeds by twenty degrees the limits to which it arrives in Florence, and generally in the evening marks from twenty-five to thirty degrees more than in the morning. Thus the climate of the plains is warm and moist. The scirocco wind frequently prevails, and during the warm season is very injurious. Healthy persons feel themselves depressed, the muscular motions languid, the head heavy and painful, somnolence continual and appetite diminished; convalescents readily suffer relapse, and sick persons become worse. These changes are so constantly observed in the hospitals, that no doubt can be entertained of their cause.

There is a board of health in this province composed of a royal commissioner, an officer of the board of municipal superintendence, and a medical inspector. This board superintends everything relating to the public health, and not only the hospitals, but also the medical men and druggists of the province. There are four large and five small hospitals. The province is divided into sixty-eight circuits, and ninety-three medical men perform the medical duties. There are seventy-two pharmacies, which are divided into two classes at the pleasure of the proprietors, and for each class there is a catalogue of medicines, with which they must be constantly supplied. Every medical man is obliged to return to the board of health, a weekly report of the number of sick entered under his care, and of those who have been cured, died, or have left the district. A similar report is made from the hospitals. The reports furnish the elements of medical statistics, and also give an exact idea of the sanitary condition of each district. Besides the resident practitioners, the government retains seven physicians and one surgeon, to afford their assistance in urgent cases, or during considerable extension of disease.

In 1841, the population was 73,966, of which 36,169 were males, 34,239 females, composing 15,598 families, the ratio being 4·83 to the family. About 12,260 inhabit the plains, and 61,906 the hills and mountains. In the summer 3358 emigrate; but this number varies with the salubrity of the season. In 1841, the legitimate births were 3123, illegitimate 132, marriages 817. The number of inhabitants to the square mile is 42·43 for the whole district. In the most unhealthy districts the proportion is from 12·15 to 18·8. In the most healthy, from 90·61 to 161·79, and at Giglio, a sterile but very healthy rock, 224·87. In the summer, 7539 males and 1433 females came to the plains for agricultural labour.

Before 1840, vaccination was altogether neglected, and in some districts inoculation was still practised with much ill effect. In 1840, laws were carried into effect to guard from inoculation and promote vaccination, and with segregation of every case of smallpox the disease soon ceased. In

1841, 203 persons suffered from smallpox and 16 from varioloid disease; 17 only had been vaccinated, and in 7 only was the cicatrix observed. In 16 of these 17 the disease was varioloid. In two individuals vaccinated during the epidemic of variola, the variola and vaccinia developed themselves contemporaneously, and both pustules went regularly through their course. In these two cases the variola was not confluent. In 1842, 32 cases occurred. Of these, 18 had not been vaccinated; and of the other 16, 11 had varioloid disease, in 4 the cicatrices were spurious, and in 1 no sign of vaccination was discovered. Five only of the 16 had true variola. The belief has obtained in Tuscany, that the vaccine virus by reflected transmission through the human body loses its effect as a preservative, and this belief has been strengthened in proportion with the increasing number of cases of variola developed in persons who had been vaccinated. Accordingly, since 1841, the Tuscan physicians have adopted the plan which has been followed in Lombardy since the occupation of Milan by the French, of vaccinating cows by virus taken from man, and then retaking this virus to serve again for our species. Virus taken in this way from the cow was used with the best result in Grosseto in 2999 persons.

We cannot go through the long table and statistical results given by our author with regard to the diseases of the year from June 1840 to May 1842. The principal points are, that 35,619 cases of illness occurred, and the proportions were intermittent fevers 54·58 per cent., pernicious fevers 1·04, continued gastric fever 8·20, rheumatic fever 4·89, phlegmon 3·03, pleuritis and pneumonia 6·10, remittent fever 4·13, chronic diseases 1·69, chronic diseases of the lungs only 5 decimi per cent. On the whole in every 100 permanent inhabitants of the province, the sick have been in the proportion 35·64, and of 100 temporary inhabitants 30·07. The agriculturalists have suffered in the proportion of 82 per cent. Other classes only 18. Relapses have been 12 per cent. Cures 96·31. Deaths 3·69.

Of 1677 deaths, 249 were from intermittent fever, 236 from gastric and rheumatic, 263 from pleuritis and pneumonia, 64 from dropsy, the result of chronic pleuritis or splenitis. Thus these diseases are the most common and also the most fatal of the province, including about two-thirds of the total deaths. The mean duration of life in the province is 22·50 years, while the proportion of deaths for the whole province is 3·19 for every 100 permanent inhabitants; in the island of Giglio it is only 1·92.

Similar tables follow for the succeeding year when the intermittent fevers were only 39·42 per cent., and the temporary inhabitants suffered rather more than the permanent ones (35·50 to 34·75 of the latter.) In both years the number of widows has far exceeded that of widowers, in the proportion of 4 to 14 in one year, and 2 to 12 in the other. This is explained by the greater exposure of males to causes of disease, and it is stated, that the women of the province commonly marry two or three times. The proportion of male deaths was 61·50, and of female 38·50, in the second year of this report. July, August, September, and October are the months in which fever prevails, very few of those engaged in the harvest escaping the effects of the malaria. The difference of temperature between the day and night in this season is from 10 to 14 degrees, and

there is great humidity in the atmosphere. The inhabitants of the neighbouring mountains descend in parties of fifteen or twenty for harvest labour, bringing women with them, and even before their arrival they commence the abuse of wine, spirits, and venery. When in the plain they frequently sleep in the open air, or more commonly in large open huts, women and men together. Their diet consists in the morning of bread, often not good, and cheese; at mid-day, of bread boiled into a sort of soup with water, seasoned with onions, oil, and vinegar, and eaten with the hands! and in the evening, of what they call *acqua cotta*, which is bread, boiled as before, and seasoned with salt, oil, and pepper. Their drink is wine, often spoiled or made more spirituous with brandy, and water, which is generally of a bad quality. Their labour is most fatiguing, because they work by the piece and not by the hour, and in fields where there is no shade from the concentrated rays of the sun. In order to escape the heat as much as possible, they work very early in the morning, and so become exposed to a dew, so heavy as completely to wet their clothes. The labourers on the ditches and embankments, and those employed in the manufacture of charcoal and potash, work under the same circumstances as the agricultural labourers, and suffer in the same proportion, and the military also from exposure when on sentry duty. Twenty-four cases of malignant pustule occurred, in every case from the patient having touched or eaten flesh of animals dead from a similar disease. It is stated that in some cases, no local contact took place, and that the disease was developed solely from the sufferers having eaten the diseased flesh.

When speaking of the *cause* of the fever, the author inclines to the belief that the mixture of salt with fresh water in the marshes greatly increases the intensity of the miasmata, because pestiferous marshes have become almost innocuous as soon as the ingress of salt water has been prevented. He gives instances of this, one in particular where near Viareggio, a pleasant town has sprung up, now selected as a watering-place, in the very months when before the district was uninhabitable. The phenomena resulting from the admixture of salt and fresh water are also developed, when the bottom of a pond or lake is composed of marine mud, that is to say, when it has formed part of the bed of the sea; and also when the old bed of the sea, covered by a thin stratum of earth, becomes wet with rain water. Miasmata are evolved by the decomposition of the marine clements, acted upon by the filtration of fresh water, and easily rise into the air by holes and fissures in the stratum of earth produced by acidity. Many parts of the maremma are of this formation.

The *Salmastraie* is supposed also to cause the evolution of miasmata. This term implies the extent of soil covered by saline efflorescence, formed of a thin stratum of earth, which makes a sort of natural filling up (colmata naturale) to an old marine marsh. This marshy bottom, called *cuora marina*, is composed of animal and vegetable substances more or less decomposed, and of hydrochlorate and carbonate of lime and soda. If the superimposed earthy stratum be very thin, it offers but a slight obstacle to the miasmata formed below by chemical decomposition. When it is of such a thickness as to prevent their free escape, a saline efflorescence of the hydrochlorates and carbonates appears in a given point, extends itself, and at last acquires such an extent as to cover several square miles. When

this efflorescence appears, the pre-existing vegetation becomes la soon dies, and disappears altogether, while gradually another veg adapted to the saline nature of the soil shows itself.

The putrefaction developed in marine animal and vegetable bod means of fresh water, and vice versâ, undoubtedly gives rise to emar extremely injurious to the human organism, in the opinion of our a and he says, the idea has been proved to be correct by the experim Savi upon the putrefaction of *Cara,* and from repeated observation the putrefaction of the *Aliga,* when on the sea-shore it is bathed wit water.

In some cases, the mixture of salt water in the marshes does no from the entrance of sea-water, but from mineral springs which c sea salt.

The author insists strongly on the influence of the scirocco wind, in rendering the emanations more deleterious, or in depressing the organism so as to render it more susceptible to their effects. He co these effects with those resulting from the admixture of salt an water, the scirocco wind which is impregnated with saline particles, an admixture of sea and land air.

Dr. Salvagnoli's remarks on the influence of wooded land up health of a district are so sensible, that we extract them, not or good specimen of his style, but also as they may afford more usefu to such of our readers as are interested in the study of medical topo

"The presence of crowded and extended woods, according to some, the contrary their destruction, according to others, cause malaria. Targ the authority of Doni, considered woods injurious, not only for being retain and imprison the principle constituting malaria, but also from b he believed, capable of producing them. Brocchi, on the contrary, ass in Rome the air was deteriorated by cutting down the woods. Thouvene and Gioia also record facts in favour of the supposition of the utility o Such a disparity of opinion proves, in my judgment, that there are circumst which a too extensive and general clearing of woods may be equally inju allowing trees and shrubs to increase and multiply without the regu man. We find certain districts and houses, with a perfectly healthy atm in the midst of extended woods, while others in similar situations, suf malaria. It appears to me reasonable to say, with Savi, that woods are i only accidentally, and never by themselves. Indeed, it is known that the vapours evolved into the atmosphere by woody plants, are in less quant those which they absorb; that instead of pouring into the air principles inj life, they emit a most useful one, oxygen, and remove hurtful ones, as acid. It is said against forests, that where they cover the soil drainage difficult, and therefore that swamps frequently follow. This is true o regard to woods in valleys and plains, but even then it is not the woo produce malaria, but the stagnant waters contained in them; and the evil the men must blame themselves who allow the former to incre their habitations, and do not open or maintain the necessary drain said, lastly, that woods prevent the free action of the north wind; this is inherent only to their situation respectively to towns and districts, and and marshes that need to be purified by north winds. Woods are us respect to the salubrity of the air,—1, because they absorb much humi the soil, and render it drier and more healthy;—2, because all plants, ticularly those with falling leaves, remove from the air principles un to respiration, and distribute a large quantity of oxygen;—3, because

fend the soil from the too ardent rays of the sun, and too great intensity of frost, and thereby prevent the alterations and decompositions which frequently cause injurious effluvia;—4, because they help to impede the direct action of the Scirocco and Lebeccio winds, the former injurious to the health of the inhabitants, the latter to the fertility of the country" (pp. 44-5.)

It appears evident, that the specific cause of the fever of this Maremma is some principle evolved during the decomposition of animal substances in the marshes, and the humidity of the atmosphere and changes of temperature, however great, can only be regarded as auxiliaries, as they exist in as great a degree in districts not subject to diseases of an intermittent type. It is still curious to observe the relations between the changes of season, and the type of the diseases of the Maremma. From January to June, the diseases peculiar to temperate regions are observed, and during the other months of the year in proportion with the increase of atmospheric temperature, fevers become aggravated according to their original force, and pass gradually from intermittence to remittence, and from this to continuity, and assume the aspect of diseases peculiar to southern regions. These phenomena may be expressed by the following formula: in direct ratio with the simultaneous increase of heat and of atmospheric humidity, owing to the influence of climate, miasmatic diseases pass from intermittence to continuity, and become at the same time always more severe.

The period of latency or incubation of the miasma after its introduction has not been determined.

A full account is given of the symptoms of the fever and of the post-mortem appearances, but we find nothing peculiar to the district in question. The most frequent type is the quotidian; the tertian, so common in the north of Europe, is not unfrequent in the winter in the Maremma. When the quotidian fevers of summer prolong themselves into the winter they readily pass to the tertian and quartan type. The numbers in three years were remittents 2066; quotidian 39,923; tertian 10,192; quartan 1544; pernicious 1346.

The access of these fevers in nine cases of ten, occurs during the day; in quotidian, generally in the morning and at mid-day; in tertian and quartan, in the afternoon and towards evening.

We now come to a highly interesting question which has lately excited considerable attention, and upon which the researches of our author are calculated to throw some little light,—the antagonism between the causes of intermittent fevers, and those of phthisis pulmonalis.

From a table showing the diseases of the permanent inhabitants of the Maremma for three years, it appears, that of 81,731 sick, there were only 100 cases of phthisis, or one phthisical patient to every 817. According to Broussais, in Algiers where intermittents prevail, of 40,000 sick in the French army only 62 were phthisical, that is, 1 to every 650 sick, and 1 death to 102 deaths from other causes; while in France there is 1 death to 5 from other causes, and in England the mortality from phthisis, constitutes a sixth part of the whole mortality. Scrofula is also very rare in the Maremma, and also in Algiers; but the author does not explain what he means by this term, and as he states, that three fourths of the phthisical have been previously scrofulous, we conclude that, probably his distinc-

Our author overlooks one important circumstance, the due consideration of which tends to vitiate the whole of his conclusions; it is this: he, as indeed almost all who have treated of this matter, seems entirely to forget the fact, that their observations can only apply to *overt* or confirmed disease, *post-mortem* examinations not being *general;* whereas, all that is essential of the malady (tubercles) may have existed in those dying of fever, without being detected. Moreover, where so many are carried off in early life by another disease (fever) we may reasonably infer, that a certain proportion of them might have become phthisical had they lived. We might as reasonably conclude, that phthisis was a most rare disease among the soldiers of Napoleon, if we comprehended in our calculations the hundreds of thousands who fell under the sword of the enemy, or the inclemencies of climate.

Interesting physical and chemical observations on the blood of the inhabitants of the Maremma have shown, that the properties of blood, taken even in simple inflammations, differ greatly from those of the blood of inhabitants of healthy districts.

"So great and so constant is the difference, that from the physical examination of the blood only, almost without error, the physician may judge if the patient resides constantly in an unhealthy atmosphere, and if the liver and spleen have become altered." (p. 65.)

"The blood is very black, forms a coagulum, which I could scarcely call grumous, by no means tenacious, separating, when first drawn, a very little serum, and which breaks up without any resistance to a round blunt stick; when the stick is passed through the coagulum, it does not present sufficient resistance to be raised: occasionally it is covered by a stratum, more or less thick, of a yellowish substance, of gelatinous consistence: it never forms the true tenacious, resisting, concave coat, and never or almost never that compact, contracted, conical coagulum, separating a large quantity of serum, by which the phlogistic diatheses are principally signalized. The coagulum readily breaks up or dissolves, (si squaglia,) and then separates a large quantity of yellow, opaque, or greenish serum. These physical qualities of the blood are most marked in the summer and autumn. A more tenacious coat, and more resisting coagulum, are more common in the winter, and especially when the north wind prevails." (pp. 65-6.)

Chemical analysis has shown a regular diminution in the quantity of fibrin, albumen, and fat. Cholesterin which ought not to be found separated from the other constituents of the bile was found in appreciable quantity. We have altered the table of M. Salvagnoli, giving only the parts in 1000, of the fibrin, &c.; he has given their actual weights, which is here unnecessary. The blood examined was taken from four individuals.

No. 1. Had suffered eighteen months from intermittent, and had engorgement of liver and spleen.

No. 2. Had inhabited the district twelve years; was suffering from quartan; liver and spleen also enlarged, the former very painful.

No. 3. Inhabitant of five years' standing; suffered from intermittent for seven months; slight engorgement of liver; extraordinary enlargement of spleen.

No. 4. Inhabitant of nine years; suffering from tonsillitis; had undergone long courses of endemic fevers; spleen engorged but indolent.

	Fibrine.	Fat.	Colouring Matter.	Albumen.	Water and Salts.	REMARKS.
	Parts in 1000.					Salts and other substances.
1	2·90	0·15	211·27	48·71	737·67	Chlorides, lactates, almost absolute deficiency of phosphates, cholesterin.
2	2·06	0·21	235·63	56·61	705·49	Chlorides, lactates, almost absolute deficiency of phosphates, much cholesterin, biliary colouring matter.
3	2·29	0·13	217·54	47·59	732·45	Chlorides, lactates, almost absolute deficiency of phosphates, smaller quantity of cholesterin than in the former cases.
4	1·96	0·16	135·61	53·10	809·17	Chlorides, lactates, phosphates, and an appreciable quantity of the colouring matter of the bile without a trace of cholesterin.

The remarks on *treatment* are sound, but do not add to our previous knowledge. Some experiments, however, were made to determine the relative value of the different salts of quinine, and of various other substances. We regret that the number of these was not more limited, as those employed would then have been tried upon a greater number of patients, and the results consequently much more worthy of confidence. Twelve preparations of quinine were employed, and on the whole the sulphate found most beneficial, but when given as usual dissolved in water with excess of sulphuric acid, gastric and cephalic symptoms were very common, and often rendered a discontinuance of the medicine necessary. This was obviated by manufacturing a crystallized neutral sulphate of quinine, which is soluble in eleven times its volume of distilled water. This hint is worth the notice of our pharmacologists.

Of the other substances experimented upon, although none have shown an antiperiodic action equal to that of quinine, still the camomile, the extract of fillirea, (Fillirea latifolia) fillirina and its sulphate, the extract of olive, and a substance called olivina, the bitter principle of the olive leaves, are worthy of considerable attention. The results of their exhibition were very satisfactory in many bad cases during the worst part of the season. On economical grounds alone, a trial of these remedies in periodic diseases should be made in our hospitals. Olive leaves, and the fillirea might be readily procured at a very trifling expense, and perhaps render us great assistance in case of any deficiency in the supply of cinchona. The powdered camomile flowers succeeded well, but it was difficult to give a sufficiently large dose. Might not the active principle be prepared for the market?

Salicine and arsenious acid were both employed without good effect. The fillirea and fillirina were used in seventy-two cases. The camomile

We have thus gone through the valuable work of M. Salvagnoli, and have condensed into this article such of its contents as we believed likely to be useful or interesting to English practitioners. The style is clear and simple, the reasoning generally sound, and our only regret is, that the statistical information was not presented in a more condensed form, and for a longer period, and that M. Salvagnoli had not devoted a chapter to the means of remedying the evils he has so well described.

II.—The second book on our list is one of those minute and laborious works in which a German author delights, but which an English reviewer approaches with a feeling akin to despair. The multiplicity of subjects of which it treats, and the very slender tie by which they are held together, render a connected account of it next to impossible. All that he can hope to do, is to select a chapter here and there, and cull from it such facts as promise the largest share of amusement and instruction. This is our aim in the present short notice; we shall, therefore, clear at a leap the first fifty pages, with the commendatory preface of Dr. Casper, and alight on the chapter treating of the population of Berlin.

In the interval from 1590 to 1841, the population of the city increased from 12,000 to 321,505. This increase was far from regular, for in the year 1661 the number of inhabitants fell as low as 6500. The population for the year 1841 presents a remarkable approach to equality between the number of male and female inhabitants, the former being 160,802, and the latter 160,703. To the males, however, will have to be added the military, to the number of 11,744. This approach to equality in the number of male and female inhabitants is only observed in the one year 1841, the females exceeding the males in all the preceding years, with the exception of 1840, when the males were slightly in excess. The 321,505 inhabitants, male and female, were thus distributed according to age. Up to the end of the fifth year, 38,815; from the end of the fifth to the end of the seventh years, 12,286; seven to fourteen, 38,162; fourteen to sixteen, 10,803; sixteen to forty-five, 183,904; forty-five to sixty, 31,673; sixty and upwards, 15,862. The males exceed the females up to forty-five years of age, but fall considerably short of them after that period.

From 1820 to 1840 the civil inhabitants of the city increased by 105,756, of which increase, 24,626 was the excess of births over deaths, and 81,130 the excess of immigrants over emigrants.

The number of married persons in Berlin, in the year 1841, was 42,752 males and 42,864 females, the excess of females being accounted for by the circumstance of several of the husbands being engaged in travelling or being in prison beyond the limits of the city. The number of births for the same year was 10,757, which gives rather less than one birth annually to four married couples. The average number of children to a marriage was $3\frac{1}{8}$, or, after some necessary correction, 3.

The proportion of births to the total population, on an average of years, is 1 to 27, and, for the year 1841, 1 to 30-31. The male births were to the female as $1\frac{1}{18}$ to 1, in other words, out of 37 new-born infants, 19 were males, and 18 females. There was one marriage to 100 inhabitants.

Twin births formed $\frac{1}{108}$ of the total births, and there was one triplet in 6279 births.

Such are the principal contents of the chapter on population, from which, passing over some eighty pages treating of matters of no medical interest, we turn to the chapter on the morality of Berlin, which opens with a passage, in justification of our choice, setting forth the intimate relation which exists between morality and health. The first point to which our attention is drawn, is the proportion of illegitimate births. It appears that, on an average of seven years, there was one illegitimate birth to $6\frac{9}{10}$ legitimate, or $14\frac{1}{2}$ per cent. Among the Roman Catholic inhabitants, we are told, the proportion is not much better, but among the Jews it is nearly six times better than the general average. This proportion of 1 to 9 contrasts favorably with several of the large continental cities. In Jena it is 1 to 7; in Stockholm, Dessau, and Göttingen, 1 to 6; in Dresden and Leipzig, 1 to 5; and in Paris, 1 to $3\frac{1}{2}$. St. Petersburg presents the very favorable proportion of 1 to 20. The statistics of suicide present some points of interest. In the interval from 1788 to 1797, only six suicides a year occurred in the city of Berlin; from 1799 to 1808, twelve a year; but from 1813 to 1822, as many as fifty-four. This increase is out of all proportion to the increase of population, for during the first period, the proportion which suicides bore to the population was 1 to 26,000; during the second period 1 to 13,000; and during the last period, 1 to 3000. The proportion of female to male suicides at these three periods respectively, was 1 to 8, 1 to $3\frac{1}{2}$, and 1 to $5\frac{5}{17}$. The crime was most frequent between the ages of 40 and 50, least so between 70 and 80; but one case is recorded under 10 years, and two above 80. The most common causes were insanity and drunkenness; fear of punishment and want ranked next; and religious enthusiasm was the least common cause. The greater number of the criminals belonged to the class of artizans. The favorite mode of death in Berlin was by hanging; next to this by the pistol. In Paris, where suicide, like everything else, must be done with *eclat*, the pistol or a leap from a window is preferred. To the acknowledged suicides must be added a certain proportion of 70 bodies found in the water in the course of two years. The season of the year does not appear to have exercised the same influence on the frequency of suicide as it has been shown to have done at Paris.

The number of suicides at Berlin, when compared with that occurring in several large cities, is considerable. During the last-named period (1813 to 1822) it was, as we have seen, 1 in 3000. In Naples it was 1 in 27,230; in London, 1 in 21,491; in Milan, 1 in 18,021; in New York, 1 in 9474; in Hamburgh, 1 in 4500; in Leipzig, 1 in 3143; in Paris, 1 in 2215; and in St. Petersburg, 1 in 416 (?) This chapter on the morality of Berlin concludes with the somewhat startling statement, that a separation takes place for every 200 or 300 marriages, a fact which points either to a low state of morals or to unusual facility of divorce.

We now pass on to the chapter on the mortality and duration of life in Berlin. In the interval from 1819 to 1841 there died on an average 1 in 36 of the inhabitants. The numbers dying, at different ages, were as follows. Out of 1000 living of both sexes, the deaths during the first year amounted to 282 males and 266 females; under 10 years the

numbers were 465 males, and 473 females; under 20, 492 males and 502 females; under 30, 578 males and 565 females; under 40, 650 males and 633 females; under 50, 734 males and 706 females; under 60, 822 males and 783 females; under 70, 907 males and 870 females; under 80, 971 males and 957 females; under 90, 999 males and 995 females; and under 97, 1000 of both sexes. The living for the same periods are easily ascertained by a simple process of subtraction. Still-births were in the proportion of $\frac{1}{18}$ of the total mortality.

Several tables of considerable interest to the statistician will be found from page 353 to page 386, but our limits will not allow us to do more than refer to them for information.

We have noticed only a few of the contents of this very laboriously compiled volume. Those who wish for very minute and trustworthy information on almost any imaginable subject which can be comprised under the two heads of medical topography and statistics, are referred to Dr. H. Wollheim's work, where they will most probably find what they are in search of.

ART. XIX.

The Nature and Treatment of Gout. By WILLIAM HENRY ROBERTSON, M.D. Physician to the Buxton Bath Charity. *London*, 1845. 8vo, pp. 372.

IT seems to us, that in writing this treatise Dr. Robertson has had it in view *to make a book*, and with this we do not altogether find fault. Mean motives (we use not the word in an offensive sense) have produced excellent ends; an interest the most personal has often swayed a man to an enterprise, a course of action, which, as regards others, has all the effects which philanthropy itself would desire to accomplish. The wish that Dr. Robertson as Physician to the Buxton Bath Charity, may feel to render his name noted in association with the successful treatment of gout, is at once natural and laudable; and to this, as already stated, we have nothing to object. We could have wished, however, that the author, in attaining his end, had inflicted on the public a less plethoric volume than he has done. Why! if gout consists in redundant, unassimilated, unreduced humours, then is Dr. Robertson's volume congenitally gouty. It is tumid with undigested extraneous matters, and if it should be born again (we mean in the way of a second edition) we should recommend that the author, before his second literary accouchement, should, by something of the nature of intellectual phlebotomy, practised on himself, lessen the chance of his again giving birth to a production, the component parts of which, even were they all sound, must at least be owned to be monstrously developed.

We do not say that Dr. Robertson's book is a stupid one, or without some useful matters; what we assert is, that in proportion to such useful matters, its bulk is unnecessarily great, and that too much of the work is occupied with diffuse, desultory observations.

The treatise is divided into eight chapters, the first of which is introductory, and the succeeding ones are devoted to a consideration of the causes, nature, history, and treatment of gout; "the treatment and m-

nagement of the gouty habit of body," (chapter 7,) and the means of preventing the gout.

The first or introductory chapter presents nothing of novelty or interest. In the second chapter, or that in which the remote causes and predisposing cause are considered, we have a simple recapitulation of what is to be met with in every treatise on the subject of gout. "Hereditary influence" is stated to be the "principal remote cause." Sedentary habits, undue exercise of mind, intemperance in food, &c., are successively and prolixly discussed. The exciting causes are more summarily noticed. These are stated to be sudden changes of temperature, "an immoderately large and heavy meal," an excessive use of wine, &c. The nature of gout is next considered. It is stated to consist (p. 69.) "in the deposition of lithic (uric) acid and its compounds, with alkalies, and principally with soda, in the fibrous tissues."

The lithic acid diathesis, according to the author, presents itself under two forms, or rather has two modes of manifesting itself. In the one, the acid and its compounds escape in the urine; in the other, they are deposited in the tissues above named. The author accordingly seems to consider gout, or the gouty diathesis, essentially to consist in a morbid tendency to the deposition of uric acid or its combinations, and in a "much disordered condition of the tissue in which it is formed." (p. 72.) Dr. Robertson thinks it not improbable that the acid, though, as constituting gouty concretions, or chalk-stones, always found in a combined state, is originally deposited pure, and gradually afterwards takes soda from the blood. "This," he observes, p. 73, "would serve to explain the immediate deposition of lithic acid, where it is formed, and likewise that other alkalies besides soda are often found combined with it, although in comparatively small proportions, soda being the principal alkali of the system." He elsewhere re-states this view, (p. 78,) and expresses it as his opinion that the irritation of the capillaries of the fibrous tissues, caused by "the throwing down of crystalline particles of lithic acid, "or of its compounds with alkali," may be the true cause of gouty inflammation.

This deposition of lithic acid has been by some ascribed to "defective oxygenation of the organic atoms which have served their purpose in the economy," and which require an additional proportion of oxygen in order to form them into a soluble product capable of being eliminated as urea. The author, with some plausibility, argues, that if this view were true, gout should be most found where food containing much carbon was used, and least where such diet was least employed; but we know the contrary of this to be the case.

The chapter, on the history of gout, contains a review both of the symptoms of an arthritic attack, and also of the derangements of other organs, which accompany it. The liver is usually much disordered in its action, and the kidneys equally or more so. We are, however, glad to hear Dr. Robertson bearing witness to a fact we have ourselves sometimes observed, namely, the presence of gout without derangement of the renal secretion. There has been an endeavour of late to exalt the indications afforded by the urine, even beyond their rank, high as that may be allowed to be, and to exaggerate the frequency with which these indications may be detected. "In some of the worst cases of gout," says Dr. Robertson,

(p. 130,) "I have met with, the urine was never deranged in quantity or character, either during or between the paroxysms." He adds, however, that in these cases, the hepatic, or some other functions, were usually disordered.

The usual deposits in an attack of gout are, in the acute stage, lithic acid and lithates of ammonia and soda. Phosphatic deposits may occur after some time, more especially if the patient's constitution be shattered. These consist chiefly of the phosphate of lime and of magnesia.

In regard to therapeutical indications afforded by the states of the urine, the author makes an observation (p. 133) in keeping with the one we have just quoted above, and the accuracy of which we can confirm, and which we cite as calculated to check the disposition prevalent with the class we may call urinary pathologists, too much to found treatment on a single branch of symptomatology, and not, as true physicians ought to do, on a comprehensive survey of the aggregate of morbid phenomena. "As to the treatment of gout," says Dr. Robertson, "it will be found that the condition of the urine, judged of *per se*, has little to do in regulating or modifying it, however valuable it may sometimes be, when looked upon as subsidiary to other treatment."

We cannot accompany the author through his long and (if condensed and better methodized) really valuable history of gout. The fault we have to find with this portion of the work is this, that the author, by noticing in connexion with gout, almost every symptom of derangement incident to the human body, and the occasional association of which with arthritic disease may be termed casual, destroys, as it were, the *individuality* of the malady of which his work specially treats, and converts his book into a sort of confused jumble of universal pathology.

The treatment recommended by the author, is, on the whole, judicious, and is, in short, that which is usually pursued. Leeching, he observes, as well as the lancet, or cupping, are seldom of the slightest use in simple gout: they may do harm, but do no good. Purgatives and diaphoretics, he states, to be the evacuants chiefly to be depended on; and he might have added diuretics, a free action of the kidney being nearly as indispensable as a free action of the bowels. The hostility of Sydenham to purgatives, both during the gouty paroxysm and the interval, is well known. The author does not entirely adopt on this subject the opinion of the great authority just named; neither do we. In a multitude of cases of gout, both during the paroxysms and at intervening periods, judicious regulations of the bowels, we do not say violent purging, is of evident use, as far as our observation extends. The purgatives must not, indeed, be cold and drastic, but warm, cordial, and what is called stomachic. The author justly discountenances the addition of narcotics to purgative medicines: the union is at once irrational, useless, and pernicious. Colchicum, so far as it is a narcotic, is to be excepted from this remark. We have not yet brought ourselves to regard it as a narcotic at all, but attribute the abatement of pain, which it often affords, both in gout and rheumatism, to its setting suspended secretions free, as for example, the biliary and the urinary. Its lowering of the heart's action we refer to the same cause.

We shall not follow the author through the remaining part of his volume, in which, had brevity and the omission of speculative discussion but been

more attended to, the useful practical matter, now made to appear only *longis intervallis*, would have presented itself to much more advantage, and would have been more conveniently and readily got at by the reader. He finishes his volume by two chapters, the one of which is devoted to the consideration of the treatment and management of the gouty habit; and the other to the prevention of gout. Both of these contain no small amount of extremely useful and intelligent hints, directions, and suggestions, and are less chargeable with unnecessary prolixity than the earlier portions of the work.

Our opinion of Dr. Robertson's treatise may be gathered from our preceding remarks. It is this; that within the 372 pages constituting his volume, there is matter which, if properly condensed and arranged, would form a very useful practical volume of 120 pages, that is, one third the size of the book actually before us. In other words, we think two thirds of the volume might with advantage be spared to us. At the same time we cannot conclude without expressing our opinion that the author is a man of very considerable intelligence; and many even of the "faults" of his volume "lean" to "the side" of ability.

ART. XX.

The Nature and Treatment of Cancer. By W. H. WALSHE, M.D., Professor of Pathological Anatomy in University College, Physician to University College Hospital, and to the Hospital for Consumption and Diseases of the Chest.—*London*, 1845. 8vo, pp. 590.

THIS work has reached us at so late a period of the quarter, as puts it out of our power to lay before our readers any such account of it as its importance demands. We have, however, seen enough in the imperfect examination we have been able to make, to convince us, that the volume before us constitutes one of the completest and most valuable monographs of an individual disease that exists in medical literature. In our next Number we hope to prove the correctness of this judgment by an ample analysis of its contents. We are induced to notice the volume on the present occasion, chiefly that we may make our readers acquainted with the improved method of treating cancer, introduced into practice a few years since by Dr. Arnott,—and which is here fully set forth by Dr. Walshe for the first time. We have been constantly expecting that this great invention—for it is really such—would have been given to the world, formally and in detail, by its author; but Dr. Arnott would seem to be as careless of ordinary fame, as he manifestly is of what is as generally prized in these later days—money. He has successively given to the world—truly *given*, given unconditionally and without any hope or possibility of pecuniary return—invention after invention, all designed for, and ministering, in the most striking manner to, the improvement of human health and comfort, and the relief of human suffering; which, if he had been so minded, might have become to him the certain means of almost boundless wealth. First, the hydrostatic or water-bed, and strap-hammock

bed, for invalids; then the self-regulating air and water stoves known by his name; then the air-pump, the self-poised chimney-valve, and fish-gill air-warmer, for heating and ventilating ships, public buildings, and rooms; and, lastly, this philosophical apparatus for supplying pressure in morbid growths: a series of effective appliances for the promotion of human weal which, we venture to say, will not merely transmit Dr. Arnott's name to future times as one of the most inventive geniuses and enlightened philanthropists, but will even form no mean element in constituting the present time an epoch in the glorious history of human amelioration. Had NEIL ARNOTT lived in the later days of classical antiquity, he would have had statues raised to him in his lifetime in the public places of his country; had he flourished in a yet earlier age, he would have come down to posterity among those benefactors of their race, whom the simple and beautiful faith of Humanity's childhood has gratefully enrolled among superior beings.

Dr. Walshe's volume is divided into two parts: "The first is devoted to the subject of Cancer in general; the second to the description of the disease as it occurs in all those tissues and organs of the body in which experience has established the fact of its existence." In both of these departments, we believe the reader will not be disappointed if he expects to find everything that was already known on the subject of cancer placed in the clearest possible light, and not a little also that is novel both in fact and doctrine.

We must here also refer, in passing, to another thing that has particularly struck us in going over Dr. Walshe's book—its strikingly practical character, and the author's enlarged general views of the nature and proper treatment of cancer. In the pages of Dr. Walshe, the disease is no longer the local, external, or *surgical* malady which it has been too much the fashion to consider it; but a true constitutional, general, *medical* disease, of which the local manifestation, particularly on the surface, is a comparatively small and unimportant feature. A very large part of the treatise is devoted to the disease as it affects internal organs, the diagnosis of which is here put in a much clearer light than heretofore. The strongest proof we can adduce of the constitutional nature of cancer is the fact, incontrovertibly made out by Dr. Walshe, that extirpation or ablation of the local affection by the knife, should only be had recourse to in an infinitely small proportion of cases.

Before introducing to the reader the subject of *compression*, which is the more immediate object of this notice, we cannot resist the pleasure of quoting two other short passages, which will give a little insight into some of the original and ingenious views of the author.

Dr. Walshe's views of the Nature and Progress of Cancer.

"I had scarcely commenced the study of adventitious products, before I became convinced that much of the obscurity pervading the subject arose less from its nature, than from the erroneous manner in which its investigation had been conducted. I found observers had overlooked the fact, that the higher orders of these products were real existences developed within existences, and possessed of two distinct modes of life: a life, subject to its variations of health and disease, irre-

the conditions of the various structures and functions of that organism. I saw that phenomena, accessory and contingent, were confounded with others necessary and essential; and that, as a natural consequence, misapprehension of many pathological relations had followed Desirous of removing this source of unsound doctrine, as it affects the most important of adventitious growths, Cancer, I have separately considered (on the plan habitually employed in my Lectures) the healthy and diseased conditions of vitality of that product." (Preface, p. i.)

"The following is the view which, I conceive, may be taken of the nature and sequence of the phenomena of the disease. A certain constitutional state exists, and may continue to exist for a variable period without giving functional evidence of its presence, although the blood and solids of the body are specially modified. In consequence of local injury, or otherwise, exudation takes place; upon that exudation the constitutional state has impressed special attributes and tendencies, (p 50;) among these attributes ranks an intrinsic power of vegetation. This vegetating faculty of the exudation reacts on the system by constantly draining it of a portion of its nutrient materials,—the progeny feeds upon the parent organism, and the first phasis of evolution is accomplished. But the natural tissues have been so modified in properties by the constitutional state, that they are incapable of resisting the encroachments of the vegetating exudation, and hence become the seat of atrophous, ulcerative, and other modes of destruction. Discharges of various kinds now still further drain the system of its fluids, and impair its vital energies; and the second phasis is established. Meanwhile secondary alteration of the blood is effected; this fluid becomes the vehicle for the circulation through the system of elements possessed of a germinating force,—these stagnate, are deposited, and new local vegetations spring into life and activity. The same series of phenomena is again and again gone through; until the system, drained of its reparative fluids in feeding exudations and supplying discharges, exhausted of almost every drop of pure blood through the influence of secondary cancerous impregnation, paralysed in its nervous energies by physical anguish and deficiency of pabulum, sinks in the struggle against the superior powers of the new existences it has created,—and in death is closed the third phasis of the disease." (pp. 189-90.)

Of Compression in the Treatment of Cancer, more especially by means of Dr. Arnott's Apparatus.

"In the year 1809 Mr. Samuel Young conceived and acted upon the idea that the continued nutrition of scirrhous tumours might be completely prevented, and the absorption of existing substance insured, by submitting them to methodic compression. The results of the practice, as made public by himself, have been condensed as follows by Dr. A L. J. Bayle. The number of cases given is nineteen; of these, seventeen relate to cancer of the breast, two to ulcers of the cheek and upper lip. Twelve cases terminated by cure; five were considerably benefited; the two cutaneous ulcers improved somewhat. The

a character with which Mr. Young never sought in the remotest degree to invest it; it would in truth be just as wise, observes the latter, to speak of the pad of a hernial truss as a specific against strangulation, as to assign the character to compression in cancer.

"The testimony of Mr. Travers (loc. cit. p. 306) is favorable to the practice. He has known tumours, such as those already described (p. 206), 'gradually reduced, and at length absorbed by equal and persevering compression, as by strips of soap and adhesive plaster, or, what is better, by an elastic roller passed many times round the chest, with layers of the Amadou smoothly interposed between the turns of the roller.' M. Recamier has employed compression upon a very large scale, and the more important part of his results is as follows: 'Of one hundred cancerous patients, sixteen appeared to be incurable, and underwent only a palliative treatment; thirty were completely cured by compression alone, and twenty-one derived considerable benefit from it; fifteen were radically cured by extirpation alone, or chiefly by extirpation and pressure combined, and six by compression and cauterization; in the twelve remaining cases the disease resisted all the means employed.' MM. Blizard and Masson have published three cases, and M. Carron du Villards three others,—in all of which irregular nodular scirrhi, the seats of lancinating pain, &c., were removed by compression. Dr. A. L. J. Bayle (loc. cit.) gives, as the general results in 127 recorded cases, 71 cures, 26 instances of improvement, 30 of total failure. These results, the most favorable on the whole that can be adduced in favour of any mode of treatment, bear scrutiny of the severest kind. It is no doubt true, that, in some of the cases alleged to be cancerous, neither of the anatomical species of that affection existed; but it is on the other hand perfectly unquestionable that many of the absorbed growths were not only actually scirrhous, but had already become the seat of ulceration, when submitted to compression.

"Difference of opinion has existed as to the best mode of applying compression. M. Recamier employs perfectly smooth disks of agaric, laid over each other, and retained *in situ* by a roller, as the compressing materials. M. Bégin sometimes substitutes a laminated plate of lead, modelled to the tumour, and surmounted with a pyramid of graduated compresses. This application (which is far from a novel one) frequently becomes painful, and cannot be endured. M. R. recommends a renewal of the apparatus every day, or every second day: M. Bégin thinks it better to change only when the bandages grow loose; and prefers, in consequence of this view, an elastic corset, capable of accommodating itself to the decreasing size of the part, as the compressing agent, wherever circumstances admit of its use. But all contrivances of these kinds are ineffectual, for various reasons: in the first place, they exercise unequal and irregular pressure on the tumour; in the second, they confine the movements of the chest to a degree varying with the amount of constriction; in the third, the force employed is not directed against the diseased mass alone, but wasted in great measure upon the healthy parts; and, in the fourth, while the difficulty of applying the apparatus effectually is extreme, it invariably loosens and becomes more or less disarranged within a short period after its application. Besides all this, the least unevenness in the material lying next the diseased structures renders the compression unbearable, from the pain it produces. These are the chief reasons, doubtless, which have hitherto prevented compression from taking its ground as a general system of treatment of various external cancers.

Dr. Arnott's Plan.

"The fertile ingenuity of Dr. Neil Arnott (already so successfully and so variously employed in devising mechanical means of relieving human suffering) has triumphed over these difficulties. Dr. Arnott has invented a method of applying

compression, which, while it is free from all the objections mentioned, is philosophical in principle, and possessed of peculiar practical excellences. His apparatus consists of a spring, an air-cushion supported by a flat resisting frame or shield, a pad, and two belts. The spring, which is of steel, is the compressing agent,—its strength being varied with the amount of pressure it may be desirable to obtain. The shield varying in shape somewhat with the circumstances of particular cases, is generally slightly convex on the external surface, of circular or oval outline, and formed of a rim of strong wire, connected at two opposite points by a flat piece of iron, which serves for the support of the spring, screws, &c., the whole being covered with jean. To the rim of this shield is sewn a sort of conical cap of soft linen, designed to receive the air-cushion, to keep it *constantly slack*, and prevent it from slipping about when applied. The air-cushion thus kept slack, fashioned into a sort of double night-cap, lying in apposition with the inner surface of the shield, and sufficiently filled with air to prevent the latter from pressing directly on the part, receives within it the tumour to be compressed. One end of the spring is attached by screws to the external surface of the frame, and the other end to a solid but soft pad, placed wherever the counter-pressure is to be made. The straps are used to keep the apparatus steadily fixed. Let us suppose that the breast is the region to which the instrument is to be applied; the position of its various parts will appear, as they are represented in the figures. The spring may either be passed over the shoulder or round the waist; the latter mode of application suits best, when the tumour is seated towards the external border of the breast, and inclined to slip towards the axilla.

"The mechanical advantages of this mode of compression are, that the movements of the thorax are not interfered with; that the amount of pressure may be regulated to a nicety; that the whole morbid mass undergoes constant, equable, and uniform pressure; that the part is protected from external injury, (a point of serious importance;) and that, unless in a very few exceptional cases, the apparatus may (either with the shoulder or waist-spring) be very easily arranged. It is necessary that the amount of pressure should be low at first (say 2lbs.,) especially in the cases of nervous, irritable people,—in fact, that the instrument should rather supply a support for, than exercise pressure on, the part; that the entire morbid structure (as well as any connected loose soft parts, which might be injured by accidental pressure of the rim of the shield,) should be included within the cushion; and that in all cases there be a distinct thickness of air-cushion between the shield and the skin.

"The effects produced by pressure are removal of existing adhesions, total cessation of pain, disappearance of swelling in the communicating lymphatic glands, gradual reduction of bulky masses to small, hard, flat patches or rounded nodules (which appear to be, both locally and generally, perfectly innocuous,) and in the most favorable cases total removal of the morbid production. The relief of pain afforded by the instrument is, without exaggeration, almost marvellous; this effect is insured by the peculiar softness and other properties of the air-cushion, the medium through which the pressure of the spring is transmitted to the surface. Females unable to obtain sleep even from enormous doses of laudanum cease instantaneously to suffer on its application; and sleep thenceforth, as though they were perfectly free from the disease.

"There are certain conditions which either interfere altogether with the use of the instrument, or reduce it to a merely palliative agent. These conditions are, more particularly, excessive bulk of the new growth, and such localizations of this structure as place any portion of it beyond the reach of pressure. Persons of irritable skin and temperament, and prone to become œdematous or anasarcous, are with some difficulty manageable. Less is to be expected in cases of encephaloid cancer than of the other species, and in cases of infiltrated than of tuberous accumulation. If the morbid mass be extensively softened, ulcerated, or in a state of fungous vegetation, palliation is all that can be fairly hoped for. Adhe-

situation where a bony or other solid support exists behind the growth and where a point for counter-pressure can be had. The mamma, the limbs, the surface of the thorax or cranium generally, are the seats in which the mode of treatment is most readily applicable. I see no reason why cancer of the testicle might not be treated thus; and gentle pressure on this plan deserves a trial in certain cases of cancer of internal parts (it would surely relieve pain), provided the general functional relations of those parts do not interfere (and they will often not do so) with the adoption of such pressure.

"The system of pressure now described, useful as it is independently of all other treatment, may be rendered more efficacious by the association of other external means and internal remedies. The following case exemplifies the power of such combinations.

Case of Cancer by Dr. Walshe.

"I was requested (March 3d, 1843) by Mr. Langley to see a lady affected with scirrhus of the breast, which it was proposed to remove with the knife. Exactly five months and a half ago was attracted by slight pain to the right breast and found there a lump about the size of a small hazel-nut, not tender to the touch, unattended with soreness or discoloration of the skin; it increased but little in size till the last six weeks, within which time it has enlarged to its present bulk; suffers scarcely any pain in the tumour itself, but has lancinating pain above the nipple in the indurated part of the gland to be presently described. In its general outline the right breast is double as large as the left (it has always been somewhat the fuller of the two;) the subcutaneous veins more visible than on the opposite side; at the axillary border of the gland is an excessively hard, solid, defined, rather moveable tumour; the finger may almost be slipped behind this, but at its inner edge it is continuous with another indurated mass, obviously a portion of the mammary gland itself in a state of infiltration; the tumour is finely knotted on the surface, the infiltrated part somewhat more largely so; besides this, the substance of the gland is indurated and knotty, especially above the nipple; the nipple is less prominent than on the healthy side, but is not actually drawn in; the areola is unaffected; there are no adhesions of the skin or alterations of its texture; no enlarged glands in the axilla, but slight thickening and hardening of some of the absorbent vessels leading thither; no discharge of blood ever from the nipple; when the tumour examined, not painful, but a short while after became so. Measurements: whole breast 4¾ inches broad, 3¼ inches vertically; tumour 1¾ inches broad, 1¼ inches vertically. The soft parts about are flaccid and yielding. I recommended the following pill: ℞. Arsenici iodurat. gr.j. extract conii ℈ij. in pill xvj. dividend: j. bis die s. Light nutritious diet; moderate walking exercise. March 8. Applied the slack air cushion (diameter of bag 6¼ inches, pressure of spring 3¼ pounds;) the whole mass of the breast included, except about half an inch at the left superior angle, where merely cellular tissue. No annoyance of any kind experienced by the patient, except slight impediment to respiration, which ceased in a few minutes. March 10. Pain totally removed; size of general mass of breast somewhat diminished, but the tumour is only rendered more prominent and apparent by this. No inconvenience is experienced from the instrument except in the back; the patient being thin, the pad presses uncomfortably on the spine. March 12. Tumour appears very distinctly reduced in bulk, more elastic, less stony in feel, less pointedly knotty on surface, less sharply defined: all these changes are to a very small amount, but

they are nevertheless positive. Catamenia for last two days. March 30. Tumour (which has been gradually decreasing) is now scarcely more than half the original size; some slight indication of absorbents with tenderness of the skin, and slight redness; nothing in axilla; (instrument much disturbed yesterday in the carriage, and edge pressing against skin.)

"From this period until the middle of August the progress of things was slow and interrupted; twice, at the menstrual period, the tumour enlarged slightly, without however becoming painful. I gradually increased the force of the spring (which had always been carried over the shoulder) to six pounds, and diminished the diameter of the bag 3½ inches,—by which a great increase of pressure was obtained; and, at the date named, the tumour was about one third only of its original size, had become freely moveable under the skin, and the general knottiness and hardness of the gland had almost disappeared; patient had lost altogether the headaches which used to torment her. I now lost sight of the patient (first through my own, and then her absence from town) till the middle of November. During all this period the instrument had been more or less neglected, and not applied at all for the last month; the use of the pills had also been interrupted. November 23, 1843. The tumour is now at least half as large again as when I first saw it; it is more painful than ever, and has reacquired all its original characters; it is, however, still non-adherent. I reapplied the cushion, of the diameter first used, and within a week a favorable change had taken place; the tumour continued thenceforth to diminish in size, until it was reduced to the size of a hazel-nut. This little nodule appearing immoveable, I (at the close of January, 1844) directed the iodide of lead ointment to be smeared on the part twice daily, the pressure being at the same time continued. The effect was almost immediate, so much so that the patient, after the lapse of a fortnight, requested to be allowed to use the inunction without the pressure: the impulse to absorption had been given; the tumour steadily decreased in size, and had totally disappeared at the close of April, 1844. I have within the last few days (August, 1845) examined the breast, and found it in every respect like its fellow, *without a vestige of tumour or induration of any kind.*

"Here then was a tumour, which (though it had so far not given rise to any of the more terrible evils appertaining to cancer) yet possessed in the clearest and best defined manner the sum of characters assigned by universal experience to growths pursuing the common course of that disease; and this tumour disappeared completely under the persevering use of the means described. Had the growth been removed with the knife, the chances (as will fully appear in the sequel) are extremely strong that, within the present period, the disease would have reappeared, and perhaps destroyed its victim. But from the very perfection of success in this and similar cases an objection may arise in some minds. It may be urged that, as in such cases a mass, composed of indefinitely vegetating cells, is removed from its original site by absorption, the displaced cells may in some new abode germinate and flourish. But the objection is a fallacious one. The absorption effected in such cases must, on physiological grounds, be considered of the kind I have termed *unproductive* (see p. 105;) and clinical experience, so far as it has yet gone, corroborates, in the non-reproduction of tumours thus dispersed, the justness of the physiological principle." (pp. 207-14.)

In the original work, Dr. Arnott's apparatus and its mode of application are illustrated by a woodcut. And we may here observe, that the volume also contains one beautiful copper-plate, exhibiting the general phenomena of the pathology of cancer, and several other woodcuts devoted to the same object.

1. *Organon der Heilkunst.* Von SAMUEL HAHNEMANN. *Dresden*, 1819. 8vo, pp. 574.

2. *Fragmenta de Viribus Medicamentorum positivis, sive in sano corpore humano observatis.* A SAMUELE HAHNEMANN, M.D. Edidit T. F. QUIN, M.D.—*Londini*, 1834. 8vo, pp. 214.

3. *Samuelis Hahnemanni Materia Medica Pura, sive Doctrina de medicamentorum viribus in corpore humano sano observatis, e Germanico sermone in Latinum conversa.* Conjunctis studiis ediderunt Dr. STAPF, Dr. G. GROSS, et E. G. à BRUNNOW.—*Dresdæ*, 1826-28. 2 vol. 8vo, pp. 450, 378.

4 *Die Chronischen Krankeiten, ihre eigenthümliche Natur und homöopathische Heilung.* Von Dr. SAMUEL HAHNEMANN. *Dresden*, 1828-30. 4 Bände, 8vo, pp. 241, 362, 312, 407.

5. *Pharmacopœia Homœopathica.* Edidit T. F. QUIN.—*Londini*, 1834. 8vo, pp. 164.

6. *Homœopathy Unmasked; being an Exposure of its principal Absurdities and Contradictions: with an Estimate of its recorded Cures.* By ALEXANDER WOOD, M.D. *Edinburgh*, 1844. 8vo, pp. 196.

7. *An Introduction to the Study of Homœopathy.* Edited by J. J. DRYSDALE, M.D. and J. R. RUSSEL, M.D.—*London*, 1845. 8vo, pp. 253.

8. *An Inquiry into the Homœopathic Practice of Medicine.* By W. HENDERSON, M.D., Professor of Medicine and General Pathology in the University of Edinburgh.—*London and Edinburgh*, 1845. 8vo. pp. 238.

9. *Homœopathic Domestic Medicine.* By J. LAURIE, M.D. Third Edition. —*London*, 1846. 8vo, pp. 576.

ALTHOUGH the subject of Homœopathy has been but little adverted to, and never formally noticed, in the pages of this Journal, we have not been unaware of its claims to attention, nor regardless of its remarkable progress in every country of Europe, both as a system of medical doctrine and a system of medical practice. We ought probably to have noticed the subject long ago. At any rate, we can refrain no longer from doing so—now, when one of the publications whose title heads this article, shows that the new doctrine has found its way into the halls of one of our most estimable universities, and is openly advocated and promulgated by its professor of pathology. On the present occasion, however, we do not intend to examine the homœopathic doctrine fully or systematically: this we may probably do on another occasion, and at no distant date. All that our other engagements and the space now at our command will permit, is, to lay before our readers some hasty sketches and some fragmentary views relating to the general subject, which have long occupied our thoughts, and which are now, as it were, forced from us somewhat suddenly and prematurely by the perusal of Dr. Henderson's book.

pathy, was born at Meissen, in Saxony, in the year 1755, and died at Paris, only three years since, in the eighty-eighth year of his age. No careful observer of his actions or candid reader of his writings, can hesitate for a moment to admit, that he was a very extraordinary man,—one whose name will descend to posterity as the exclusive excogitator and founder of an original system of medicine, as ingenious as many that preceded it, and destined, probably, to be the remote, if not the immediate, cause of more important fundamental changes in the practice of the healing art, than have resulted from any promulgated since the days of Galen himself. Hahnemann was undoubtedly a man of genius and a scholar; a man of indefatigable industry, of undaunted energy. In the history of medicine his name will appear in the same list with those of the greatest systematists and theorists; unsurpassed by few in the originality and ingenuity of his views, superior to most in having substantiated and carried out his doctrines into actual and most extensive practice. Nor will the overthrow of his system, as a system, deprive him of his fame, so long as Paracelsus, and Stahl, and Silvius, and Boerhaave, and Brown, and the other hundred heroes of theoretical renown, are remembered by their successors in the schools of medicine.

The thoroughly radical change in the theories and practice of medicine, propounded in the system of Hahnemann,—a change equivalent to a total reversal and subversion of almost all that had preceded it,—naturally roused great and general opposition to it in the minds of medical men. This, and the seemingly-monstrous extravagance of one of its main dogmas—that of infinitesimal doses—so abhorrent at first sight to common sense, and so obnoxious to the attacks of a facile ridicule—has, up to this day, prevented common justice being done to the new system, and to its author and his successors. By most medical men it was taken for granted that the system was one, not only visionary in itself, but was the result of a mere fanciful hypothesis, disconnected with facts of any kind, and supported by no processes of ratiocination or logical inference; while its author and his apostles and successors were looked upon either as visionaries or quacks, or both. And yet nothing can be further from the truth. Whoever examines the homœopathic doctrines as enounced and expounded in the original writings of Hahnemann, and of many of his followers, must admit, not only that the system is an ingenious one, but that it professes to be based on a most formidable array of facts and experiments, and that these are woven into a complete code of doctrine with singular dexterity and much apparent fairness. And it is but an act of simple justice to admit, that there exist no grounds for doubting that Hahnemann was as sincere in his belief of the truth of his doctrines as any of the medical systematists who preceded him, and that many, at least, among his followers, have been and are sincere, honest, and learned men. That there are charlatans and impostors among the practitioners of Homœopathy cannot be doubted; but, alas, can it be doubted, any more, that there are such, and many such, among the professors of orthodox physic?

On these grounds, then, it appears to us reasonable, that the claims of Homœopathy, regarded as a system of medical doctrine, ought to be

supplied in its favour by its professors, than we have of rejecting any other evidence in favour of any other medical doctrine, theoretical or practical.

The first idea of the fundamental doctrine of Homœopathy, seems to have entered the mind of Hahnemann in the year 1790, (the forty-fifth year of his age,) while engaged in translating Cullen's Materia Medica into German. Dissatisfied, it is said, with the author's attempt to explain the action of bark in curing intermittent fevers, he resolved to make trials with it on his own person,—he being then in perfect health. Having taken a sufficient quantity of this drug, he affirms that he was speedily attacked with symptoms resembling those of ague; "and forthwith," says his historian, "arose in his mind a conception of the great truth which was destined to constitute the basis of the new art of medicine." * 'May not,' he reasoned, 'the power of cinchona to cure ague, depend on its power to excite in the healthy body a similar disease?' With the view of testing the truth of his hypothesis, he tried the effects of other medicines on himself and others, and always, it is said, with the same result, viz. "that the medicines excited in the healthy body the same symptoms which they were capable of removing when these occurred naturally in the diseased body." Proceeding then to examine the records of medicine, as to the effects accidentally produced by poisons and other strong drugs, and finding everything, as he believed, confirmatory of his own views derived from experiment, he hesitated no longer to consider as established, and to promulgate the grand and universal law, that "every (dynamic) disease is best cured by that medicine which is capable of producing in the healthy body similar symptoms, or a similar disease, (ὅμοιον πάθος;")† or, as it is usually stated more briefly, *similia similibus curantur—Like are cured by like*, i.e. *homœopathically*. The doctrine was hence named *Homœopathy*, and those who adopted it *Homœopathists*, or *Homœopaths*. In contradistinction, the common medical doctrine was named, from employing, in the treatment of disease, medicines producing an effect not *like* (ὅμοιος,) but *different* (ἄλλος) from that produced by the disease, *Allopathy* (ἄλλο πάθος), and its professors Allopathists or Allopaths. It is convenient, for the sake of brevity, to make use occasionally of these terms. Possessed of this, as he conceived, unfailing clue to all the mysteries of therapeutics, he and his disciples commenced an extensive and long-continued series of trials of the effects of various medicines on their own persons, and on the persons of others. The results of these experiments are recorded in Hahnemann's 'Fragmenta de viribus Medicamentorum positivis,' and 'Reine Arznei-mittellehre,' or 'Materia Medica Pura,'—the former first published in 1805, the latter in 1811. The results of the whole of these proceedings was regarded by Hahnemann as confirming, in every case, his great primary law, and as extending its application practically to a vast number of diseases. All that was requisite, hence

* Viso symptomatum utriusque morbi concentu tactus, magnam statim promulgavit veritatem quæ novæ artis medicæ fundamentum facta est. (*S. Hahnemanni Materia Medica Pura.* Dresdæ, [illegible] Introductio Edit. p. vi.)

† Ibid. p. vii.

forth, to the successful treatment of diseases, was the selecting the medicine whose effects on the healthy body came nearest the symptoms of the particular disease to be treated. This selection was rendered easy, as to numerous diseases,—i.e. so far as the experiments had gone,—by reference to the published records of the experiments; and the knowledge was to be extended by further trials of the same kind with other medicines.

Hahnemann gave this as the *rationale* of the cures thus effected, viz. that of two similar actions developed in the same part, the stronger destroys the weaker; but he regarded his doctrine as substantially based *on experience,* and therefore as independent of any theoretical explanations. The curing of diseases *homœopathically*, that is, with medicines producing similar symptoms in the healthy, was, he maintained, *a fact* which could not be disputed, whether the theory invented to explain it was true or false.

It would appear that the doctrine of *infinitesimal doses* constituted no original or necessary part of the general doctrine of homœopathy. In complete accordance both with the theory and the primary experiments, medicines might be given homœopathically, and still in appreciable doses. And all the accounts we find in the writings of Hahnemann, on the origin and establishment of this part of his doctrine, strike us as being much less explicit than might have been expected on a point of such essential importance, and which has always constituted so prominent a feature of his system. He merely informs us in his *Organon*, (§ 240 et seq.) that it being found injurious, in the treatment of diseases, to produce a *medicinal malady much* greater than the *natural malady* intended to be cured, the object of the practitioner should be to produce an affection in the least possible degree greater than that to be removed, so that when the latter vanishes, the former may leave no trace behind: in other words, the energy of the medicament being expended in extinguishing its hostile *double,* none is left to harm the constitution of the patient. But owing to the remarkable sensibility of the diseased body to the agent producing a like action in the part affected, it is, he says, very conceivable how an extremely minute dose of a well-chosen remedy should suffice to produce the necessary degree of action. And experience, he assures us, verifies this presumption; it being found, on trial, that it is hardly possible to attenuate too much the dose of a remedy, provided it be well chosen. "It is of little consequence that this attenuation may go so far as to appear impossible to common physicians, whose minds are only conversant with gross material notions. Vain declamations (he truly adds) must cease in the presence of an experience that cannot err." (§ 278.) In the fourth edition of the 'Organon,' he tells us that experience had led him to diminish the doses much more than he thought necessary at first; and this smallness of dose, astounding as it is, now constitutes, as we have already said, one of the most striking parts of the practice of homœopathy, and is, indeed, now universally considered as inseparable from it, and even an essential part of it.

The consideration of this reduction of the homœopathic doses, from a sensible to an infinitesimal amount, suggests to the sceptical or suspicious mind another explanation of the cause much less favorable to

administered in sensible doses, on the homœopathic principle, *similia similibus*, they were found to act *not* beneficially, because any effect they produced was, at best, not curative, and, probably, was injurious by disturbing the curative effects of nature. When they were reduced to infinitesimal doses, they ceased to produce *any* effect on the system, and so came to *seem* beneficial by not interfering with the *vis medicatrix*.

There seems also to be a contradiction in the facts, as well as the reasoning of Hahnemann, in regard to this matter. He says it is from the sensitiveness of the affected part being exalted to an extraordinary pitch by the disease, that the remedy operates in the infinitesimal dose. If this is the case, how does he explain the alleged facts, on which all his therapeutics is based, viz. the production of such a multitude of symptoms (i.e. medicinal diseases) in the healthy body, as recorded in his 'Materia Medica Pura,' and his 'Fragmenta?'

Every one has heard of this incomprehensible posology; but we are inclined to believe that few, if any, but the homœopathists themselves, or those who have read their books, (and only a small number have) are aware of its infinite and astonishing minuteness. What passes respecting it, in common medical parlance, is regarded as a playful exaggeration of the truth, garnished good humouredly for the nonce, like the ornamental facts of the story-teller. And it is no wonder that this is so. Mere imagination, working primarily on its own ground, could never have reached such a climax of the marvellous. Here, assuredly, if anywhere, the truth, if truth it be, is stranger than fiction.

So minute are the doses prescribed by the Hahnemannic school, that they are scarcely conceivable by the human mind. They defy all the powers of chemistry and physics to detect in them any trace of the remedial substances which they profess to contain, and they almost confound arithmetic in reckoning their amount. We are not ashamed to confess, that our own powers are inadequate to put down in figures an ordinary homœopathic dose, and we suspect that many of the homœopathists themselves would find themselves in the same predicament on trial. The following are the different *attenuations* or doses used:

First	=	one hundredth of a drop or grain.	
Second	=	one ten-thousandth	do.
Third	=	one millionth	do.
Sixth	=	one billionth	do.
Ninth	=	one trillionth	do.
Twelfth	=	one quadrillionth	do.
Fifteenth	=	one quintillionth	do.
Eighteenth	=	one sextillionth	do.
Twenty-first	=	one septillionth	do.
Twenty-fourth	=	one octillionth	do.
Twenty-seventh	=	one nonillionth	do.
Thirtieth	=	one decillionth	do.

The primary dilutions or attenuations are used comparatively rarely; the higher ones, as the sixth, twelfth, twenty-first, and thirtieth, very commonly. It may be worth a moment's trouble to try how far we really understand or comprehend these numbers. Looking at the first of these

we have no difficulty. The *hundredth* (100th) part of a grain, is intelligible enough; the *ten-thousandth* (10,000th) is comprehensible, but begins to waver before the mental view; while the *millionth* (1,000,000th) part of a grain, puts our powers of comprehension on the rack, and leaves us in a chaos of undefined entities or non-entities, we know not which. We fancy that we grasp the reality, and then it instantly vanishes as a phantom, even beyond the sphere of imagination itself. Having got so far, the additional subdivisions, or attenuations, scarcely add to our difficulties. The mind, in any such case, is occupied by a word more than a thing,—and whether the word be a Millionth, Billionth, or Decillionth, the power of comprehension seems the same. And yet the *actual difference* between these quantities is immense,—so immense as to be almost as inconceivable as the actual things themselves. This will be more intelligible, we think, by setting it down in words thus:—

One thousand thousands, is . . . A Million.
One million millions* . . . . A Billion.
One million billions . . . . A Trillion.
One million trillions . . . . A Quadrillion.
And so on to . . . . A Decillion.

Now, we believe this last denomination (according to the English mode of numeration) would stand thus in figures:—

1,000000,000000,000000,000000,000000,000000,000000,000000,000000,000000,000000,000000.

Imagine, if you can, a grain of silica, or charcoal, or oyster shell, (powerful remedies, according to Hahnemann and his followers, in this attenuated condition,) divided into this number of parts: and one of these parts is not only a fit and proper dose to be given as a remedy for severe diseases, but is an agent of such potent influence on the animal economy, that *one dose* of this amount will continue acting for thirty, forty, or fifty days, and must not be interfered with by any repetition of it, for fear of deranging or destroying its curative virtue! Thus, Hahnemann tells us that a sextillionth of a grain of carbonate of ammonia will act beneficially upwards of thirty-six days;† that the decillionth of a grain of oyster shell (*calcarea*) will require forty, fifty, and even more days, "to effect all the good it is capable of;"‡ that a similar dose of plumbago (*graphites*) will act for at least from thirty-six to forty-eight days:§ and a like dose of phosphorus, at least forty days.‖ "Of such minute division," remarks Dr. Alexander Wood, in his very clever pamphlet, "no language can give even the slightest idea, and though calculations may express it in figures, yet they fail to convey any mental conception of the amount." He accordingly gives the following analogical illustrations, as tending, at least, to help us to comprehend the unbounded vastness, or, rather, infinite littleness, of the subject contemplated, if not to compass themselves in our minds.

"A billion of moments have not elapsed since the [Mosaic] creation of the world, and, to produce a decillion, that number must be multiplied by a million seven separate times. The distance between the earth and the sun is ninety-five millions of miles; twenty of the homœopathic globules, laid side by side, extend to

* This is according to the English mode of calculation. The French calculate by thousands—not by millions; e. g. with them a billion is a thousand millions only.

† Die Chronischen Krankheiten, band ii, p. 20. ‡ Ib. p. 67. § Ib. p. 148. ‖ Ib. iii, p. 48.

about an inch, so that 158,400,000,000 of such globules would reach from the earth to the sun. But when the thirtieth dilution is produced, each grain is divided into 100,000; 000,000; 000,000; 000,000; 000,000; 000,000; 000,000; 000,000; 000,000; 000,000 parts,* so that a single grain of any substance, in the thirtieth dilution, would extend between the earth and the sun 1,262; 626,262; 626,262; 626,262; 626,262; 626,262; 626,262; 626,262; 626,262 separate times!" (p. 108.)

After this, the more familiar illustrations that one hears of, such as a grain or drop of the original medicine being dissolved in the lake of Geneva, the Caspian, or the Mediterranean, and then a drop of the marine solution given as a homœopathic dose, will hardly appear extravagant.

It is, however, but justice to Hahnemann and his followers to state, that they only then attribute such powers to their infinitesimal doses, when the remedies are prepared in a peculiar manner; maintaining that new properties and powers are developed in them by the frictions and shakings to which they are thereby subjected. The evidence they adduce in support of this opinion, is entirely derived from *experience*, they say: medicines prepared in their peculiar manner being found capable of curing diseases, while, if otherwise prepared, they are not.

The character of this evidence may be more particularly considered hereafter; we will only now remark, that its validity will depend entirely upon the quality of the evidence which they can adduce under the name of *experience*. If they adduce no other proof but the fact of diseases ceasing on or after the employment of their medicines, the fact, though repeated *ad infinitum*, if standing simply by itself, must go for nothing in the way of proof. If they can show a sufficiently large number of instances of two parallel series of diseases, the one series treated homœopathically, the other left to nature, and show that all or the vast majority of the one set were cured or benefited, and the other set not, — then, indeed, we shall be prepared to admit the conclusiveness of the argument based on experience. And in this case we must concede to the Homœopathists, that no argument based on the mere ground of a positive inconceivableness of a dose, or a supposed impossibility of its action, will have any weight. "Empty declamations," to repeat Hahnemann's own words, "must give way before the might of infallible experience."

The doctrine of infinitesimal doses, based, as it is, on the alleged infinitesimal sensitiveness of the diseased body, or, at least, of the affected part, must, as a matter of course, draw after it, as a corollary, the necessity of a strict regimen during the cure of diseases. If medicinal substances, reduced below the standard of mental conception, are able to produce such great effects on the animal system, *a fortiori* may many other things entering the body in the shape of food or drink, or acting on it from without, produce similar, or, at least, somewhat analogous effects, to the great detriment of the individual and the utter counteraction or derangement of the remedial process instituted by the homœopathic medicament. To be sure, it might be argued that, as the former class of substances are not "prepared" according to the homœopathic formula, they ought not to act so energetically. But to this it is replied, that many of the substances in question are taken in such large doses, that they affect the system *allopa-*

* We believe Dr. Wood is here under the mark, and that the real sum is that given by us above.

thically, or, in other words, openly and palpably. And this is, indeed, true. It was therefore necessary, on the principles of the new doctrine, that this matter of regimen should demand the strictest attention. In *acute* diseases, mania excepted, Hahnemann advises the instinctive desires of the patient to be complied with, in regard to food, drink, temperature, &c.; but in *chronic* diseases a rigid system of exclusion is enforced.

This fact, the peculiar *homœopathic regimen*, is one of great importance in every point of view, and must never be lost sight of in our attempts to estimate the value of homœopathy as a system of therapeutics.*

So far, it must be allowed, the doctrines of Hahnemann have either a show of reason in themselves, or, at least, claim to be founded on grounds even superior to reason—experience and experiment. There is, however, an important part of the system founded by him, which is essentially hypothetical, and which it is hardly possible for even his own disciples, especially those educated in this country, to assent to. This is his doctrine of the origin and nature of *chronic diseases*. Hahnemann maintains that all these, or with hardly an exception, are derived from three cutaneous diseases, *syphilis*, *sycosis*, and *psora*, or common itch. Of the whole class of chronic diseases, he attributes one eighth part to the two former maladies, and the remaining seven eighths to the last. In nearly all chronic diseases, then, the real object of consideration with the physician, and the thing to be cured, is not the ostensible diseases, but their all-pervading cause and basis, the psora, or itch. This psora he considers to be originally a disease of the whole system, which only shows itself locally on the surface in its progress. If it be cured in this form by local means, it infallibly gets worse internally, and may long subsist in this condition—for many years even—before it puts on the semblance of any formal chronic disease. In its amorphous state it may be so latent, that, although existing for years, the patient is entirely unaware of being out of health. Often, however, it is productive of a vast variety of obscure symptoms, which are seldom attended to, but which are recognizable by the adept. It is curable in this state; but it is seldom submitted to cure until it has declared itself under the guise of some more formal chronic malady. Then the patient seeks relief from medicine; and wo be unto him if he is treated by an allopath, or even by a homœopath on the general principles of homœopathy! No: it is not sufficient that the remedies prescribed are *similia similibus* simply; they must be taken from a special circumscribed class, named *anti-psoric*, discovered by Hahnemann to be gifted with a specific virtue for the cure of the itch and the itch-diseases.† It is laid down as an axiom, that *nature cannot possibly cure any of these*

* The following is a list of things forbidden, given in a note to § 260 of the 'Organon:' coffee, tea, beer, drinks containing aromatics, spiced chocolate, scents and perfumeries of all sorts, tooth-powders (liquid or pulverised) containing aught medicinal, perfumed bags, high-seasoned meats, ices and pastry flavoured with aromatics, all herbs and roots having medicinal properties, cheese, meats too long kept, pork, goose, duck, too young veal. The following things are also prohibited: over-indulgence at table of all kinds, too much salt or sugar, all spirituous drinks, over-heated rooms, sedentary life, passive exercise on horseback or in a carriage, sleep after dinner, sexual pleasures, exciting books, uncleanliness, anger, vexation, scorn, exciting play, over-exertion of mind, marshy districts, confined localities where the air is stagnant, &c.

† In a note appended to the first section of the 'Chronischen Krankheiten; 'On the Nature of Chronic Diseases,' we have an enumeration of some of the diseases derived, from psora. This list contains the names of *all* our common chronic diseases, of which 120 are formally named.

chronic diseases; but that, unless treated properly, that is, homœopathically and anti-psorically, they must infallibly get worse until they end in death. And it would seem that, although always curable by the proper remedies, they are as chronic in their cure as in their nature. The anti-psorics may work *tuto et jucunde*, but hardly *cito*, since we find Hahnemann declaring that no one but a quackish ignoramus can fancy that a disease of so long standing can be cured in a few weeks;* or, if so cured, it is only for a time, to burst out with ten-fold fury by and bye. If this doctrine be the truth, we are sorry for the many patients with chronic diseases cured so rapidly by Dr. Henderson. By this time, we fear, he will have them all again on his list, in the full horrors of this universal original sin of psora. As the disease may be of ten or twenty years' standing in the body of the unconscious patient, Hahnemann says that a cure effected in one or two years must be considered as rapid. Some of our allopathic readers will be the less surprised at this *festina-lente* proceeding, when they are informed that the true anti-psoric treatment in such cases forbids the repetition of the single decillionth dose first prescribed, until after the lapse of twenty, thirty, forty, or even fifty days! The total number of anti-psoric remedies detailed in the treatise on Chronic Diseases is twenty-two. Mercury is considered as a false anti-psoric, and its employment denounced as producing the most dangerous consequences; often, indeed, benefiting speedily, but only for a time; the disease returning in a vastly greater degree or worse form.† And here, again, we fear that our friend, Professor Henderson, who employs this medicine in these psoric diseases, must have forgotten the instructions of his master, and must look forward to the relapse of some of his best cases, so triumphantly but unscientifically cured by "*Mercurius* 6."

The preceding is a brief outline of the main doctrines of Homœopathy; very imperfect indeed, and confessedly doing injustice to the large and important subject; yet, it is to be hoped, accurate as far as it goes, and assuredly drawn up honestly and candidly. It is not our intention, on the present occasion, to submit the doctrine to any minute or formal critical examination: before, however, proceeding to the notice of Dr. Henderson's book, we wish to make some cursory remarks upon it in its double aspect, as a system of doctrine, and as a practical art.

As has been already stated, we think it impossible to refuse to homœopathy the praise of being an ingenious system of medical doctrine, tolerably complete in its organization, tolerably comprehensive in its views, and as capable of being defended by feasible arguments, as most of the systems of medicine which preceded it. It is quite another consideration whether it is TRUE.

If homœopathy can defend itself with more feasible arguments than many of its opponents imagine, it is assuredly obnoxious to objections which it cannot easily rebut. These may be found in ample detail in Dr. Wood's pamphlet, and in many other books and journals of easy access to the reader. We would here indicate a few of the more important which must present themselves to most minds in considering the general question.

* "Nur ein gewohnlichen, unwissender Curirer kann leicht versprechen, eine schwere, langwierige Krankheit in 4, 6 wochen zu heilen." (Kron. Krank. B. I. 230.)

† Chronischen Krankheiten, B. ii, p. 12.

1. We hold the great alleged fact from which the doctrine took its rise, to be no fact at all; or, at least, not to be a fact of that generality of manifestation, which a theory said to be of universal applicability, ought to rest upon. We deny, on the one hand, that many of the medicines said by Hahnemann to be capable of exciting artificial diseases, or the symptoms of diseases, in the healthy body, are really possessed of such powers. We instance, in proof of our assertion, the very medicine which gave rise to the idea of the doctrine in its author's mind—cinchona. We deny that it will produce ague, or anything like ague, or any other form of fever, in the majority of human beings; and so of a large proportion of the homœopathic remedies in most common use. On the other hand, we affirm that some medicines are capable of curing morbid conditions of the body which are incapable of exciting any such condition in the healthy body.

2. We affirm that a large proportion of the experiments performed by Hahnemann and his friends, with the object of ascertaining the therapeutic properties of medicines, are altogether fallacious; and that the alleged facts thereby elicited are not facts at all. We believe that of the numerous—we had almost said innumerable—symptoms recorded in their trials, the vast majority bore no other relation to the medicaments swallowed, but the relation of sequence. Not a shadow of *proof* exists that the symptoms were the consequence or direct effect of the medicine; while a thousand reasons can be adduced for supposing the contrary to be the fact. As the doses administered in these trials—at least, in the later and principal trials—were administered in infinitesimal doses, we are fully warranted in even denying entirely that *any* effect was produced by them. Before we can be called on to admit the recorded phenomena as *consequences* of the medicines, we have a right, as in the case of the treatment of diseases, to call for a parallel series of healthy persons set down to record all their sensations for days, after taking *no* medicines. This the homœopathists cannot give us. In these experiments it seems to be taken for granted that every bodily or mental change, every sensation, every action that occurred subsequently to the medicine being taken, was *caused* by the medicine. Every feeling and occurrence was recorded, and everything is admitted as matter of course. Yet no unprejudiced person, who examines these records even superficially, can for a moment believe that one half or one tenth of the symptoms recorded, were, or could be, produced by the medicaments swallowed. The very *number* of the symptoms stated to be produced, independently of their character, suffices to show the absurdity of the conclusions drawn. Thus, for example, 1090 symptoms are recorded as effects of oyster-shells (calcarea); 590 as produced by plumbago; 1242 as the effects of the ink of the cuttle-fish (sepia). If we had room to give specimens of the various symptoms, no doubt could remain on any candid mind as to the utter want of any necessary connexion between a vast proportion of them and their alleged causes. Among these symptoms thus alleged to be produced, we have almost every sensation which man or woman can feel, derangement of nearly every function of the body, and many formal diseases, *surgical* as well as medical.

3. Even in the cases where positive effects are produced on the healthy body by medicines in sensible doses, these effects (except in a very small number of instances) bear a most imperfect resemblance to any natural

diseases which are called *latent*. How many diseases have been detected only on dissection after death, and which have escaped the recognition of the most experienced physicians? Every physician, for example, has met with cases of chronic pleurisy with extensive effusion into the chest, which presented *no pectoral* symptoms, and which were only detected by auscultation. How could the fitting remedy for such cases be selected on the principle of *similia similibus?*

4. Many persons deny the truth of the homœopathic therapeutics, on the mere ground of the extreme improbability of the *theory* of disease adopted by the homœopathists. We do not admit the validity of this objection. If we once admit that the homœopathic doses possess a medicinal potency, and that this potency exerts itself in exciting actions analogous to those of certain diseases, we see nothing unfeasible in the doctrine that the new artificial action should destroy the previous natural or morbid one. At least, this is as good and rational theory as most of our orthodox medical theories. And, indeed, it is supported by several strong analogies afforded both by pathology and (allopathic) therapeutics.

5. But to admit the potency of the homœopathic medicaments is not so easy. Indeed, it is so difficult, that all the arguments that have hitherto been adduced in support of the affirmative of the proposition, are incapable of making any impression on ordinary minds, while the glaring improbability of the fact lies open before them. All the arguments of weight seem to be on the other side; and nothing but the demonstration of the truth—if truth it is—by positive physical facts within the sphere of the senses, can ever win assent to it. The reasons against the doctrine are so manifold and obvious that it is almost unnecessary to state them. That substances possessing a power of acting on the animal economy in doses of a certain appreciable amount, and which are found to lose their power when administered in quantities still appreciable but less than this amount, should once more acquire the same or similar properties, when this lesser quantity is rubbed for a few minutes in a mortar, or shaken for a second or two in a phial, would be a thing most strange and unaccountable. That when the quantity was reduced not merely below an appreciable amount, but so far below this as to vanish utterly from the senses and set at defiance all power of detection, and almost of calculation;—nay, that when attenuated to such a degree as to be inconceivable by the human mind, the substance should not only regain the potency it had lost, but a potency vastly greater—would surely be still stranger and still more unaccountable. But when—going far beyond all this—we find the homœopathist maintaining that substances utterly powerless in a state of sensible bulk, even in the greatest amount, acquire astonishing powers by mere subdivision, without any discoverable change in their physical or chemical properties,*—can any proposition be submitted to human apprehension that seems more utterly improbable—more ludicrously absurd? To be called on to believe that the decillionth of a grain of charcoal or oyster-

* It will be afterwards seen that Hahnemann says the chemical properties are changed by attenuation; but the arguments he brings in proof are invalid.

shell, is capable of producing hundreds of the most formidable symptoms, and of curing, as by magic, the most inveterate diseases—while we can take ounces, nay, pounds, of the very same substance into our stomachs with no other inconvenience than its mechanical bulk—seems so gratuitous an outrage to human reason that the mind instinctively recoils from the proposition.

It is, however, but fair to give the reader an opportunity of exercising his own judgment on this question, by stating the precise nature of the manipulations to which the remedies are subjected, and under which these marvellous powers are said to be developed. It is also reasonable that he should be made aware of the kind of arguments by which it is attempted to explain the manner in which so extraordinary a change takes place, or rather to illustrate, by analogy, its possibility at least, if not its probability. This we shall now do.

We translate the following directions (which must be rigidly followed,) from the introduction to Hahnemann's work on psoric or chronic diseases.

Ninety-nine grains of sugar of milk are pulverized and divided into three parts, each of course containing 33 grains. If the medicament to be prepared is solid, one grain, if liquid, one drop, is added to one of the parts of sugar of milk in an unglazed porcelain mortar: the two substances are mixed together for a moment, by means of a horn or bone spatula, and are then rubbed (with a middling degree of force—mit einiger Kraft) with the pestle, also unglazed, for *six minutes:* the mass is then scraped [by the bone or horn spatula we presume] from the bottom of the mortar and the pestle, during the space of *four minutes* more: it is then rubbed as before *six minutes: four minutes* are again consumed in scraping the mass together. The second portion of sugar of milk is then added, the two are stirred with the spatula for an instant, and are then subjected for six minutes to similar trituration. The powder being again scraped together during the space of four minutes, is once more triturated for six minutes, and this time more forcibly (kräftig gerieben). Being again scraped for four minutes, the third and last portion of the sugar of milk is added; the whole is mixed by the spatula, and then forcibly triturated for six minutes; again scraped four minutes and again triturated six. The powder is then carefully removed from the mortar and pestle and deposited in a stoppered phial. This is the *First degree of attenuation,* or the hundredth degree of power.

To raise the medicament to the *Second degree of attenuation,* or the 10,000th power, one grain of the powder thus prepared is mixed with one third of ninety-nine (thirty-three) grains of sugar of milk; these being well stirred with the spatula, are forcibly triturated for six minutes and scraped for four; and the same operations are performed on adding the second and third portions of the sugar of milk respectively. The powder is then preserved as before in stoppered phials.

It thus appears that each attenuation is effected by means of six triturations of six minutes each, and six scrapings of four minutes each; the whole period of preparation occupying exactly the space of one hour.

To obtain the *Third attenuation* (the *millionth*) a grain of the second attenuation is taken and treated precisely in the same manner. The higher attenuations are obtained from this third-power powder, by means

one hundred drops of strong alcohol and one hundred drops of distilled water, both of low temperature (keller-temperatur, cellar-temperature) are mixed together by means of *ten shakes of the arm* (mit 10 Arm-Schlägen).* One grain of the powder of the third or millionth attenuation being placed in a phial, a hundred drops (or one half) of the diluted alcohol is poured upon it, the stoppered phial is then turned slowly round on its axis (um seine Axe langsam gedrehet) for some minutes, until the powder is dissolved, and then twice shaken.

The next dilution or attenuation is formed, by adding one drop of the preceding to ninety-nine drops of pure alcohol, and giving the phial containing these, two shakes. The next attenuation is formed precisely in the same manner, by adding one drop of the preceding solution to ninety-nine drops of alcohol, and consummating the union by the same double arm-shaking. And all the higher dilutions are obtained exactly in the same way, one drop of the immediate predecessor constituting the hundredth part of its successor.

The shakings must be of moderate force, and in order that they may be uniform, the phials must be of such a size as to be exactly two thirds filled by the liquid.

Finally, in order to fit the medicines for actual administration, fine globules of sugar of milk are prepared, (as near as possible of the same size and about that of a poppy seed) two hundred of which weigh one grain or thereabouts. These globules are all moistened with the proper attenuation, by being touched by the moistened stopper of the phial containing it, and are themselves preserved dry in stoppered phials, ready for being swallowed in such numbers as are prescribed.†

We cannot find in Hahnemann's writings any explanation of or reason for the *precise* and *peculiar* mode and amount of the manipulations prescribed. He, however, gives in many places reasons why, or, at least, analogical illustrations how, it may result, that the rubbings and shakings, added to the infinitesimal subdivision, may confer on the substances operated on, the new properties ascribed to them, the acquisition of which he admits to be almost miraculous, and conferring on homœopathy, and especially on himself, for having made the discovery, no slight honour and glory.‡

In proof of this he instances the solubility of substances in water, spirits of wine, &c., which were previously insoluble in any such medium, such as petroleum, oyster shells, silica, the metals, &c.; the non-alteration of substances previously alterable when exposed to certain agents, as phosphorus exposed to air, neutral salts exposed to acids, &c. In illustration of the power of infinitesimal doses to act on the human body, he instances the matter of contagion, magnetism, animal magnetism, &c.

* This is the direction in the text, but in a note the author says, that, for several years past he had employed only *two shakings* instead of ten (ein zweimaliges Schütteln mit zwei Arm-Schlägen statt des Zehnmaligen) having been convinced by many comparative trials on the sick that the lesser number is not only sufficient but preferable. He adds, though not very intelligibly, that two shakes develop as great an *amount* (Menge) of medicinal power, though in a lower *degree* (nicht in so hohem Grade). (Band II, s. 10.)

† Chronischen Krankheiten. b. ii, pp. 5-11.

‡ Ibid. b. ii, s. 1-2.

One or two obvious remarks suggest themselves in reference to what immediately precedes. The consideration of the *peculiar* manipulations inculcated in the preparation of the medicines, can hardly fail to produce an impression very unfavorable to the author of them. In the first place, it is manifestly *impossible* for any human being, during the course of a long life, much less in the course of a few years, to have performed a sufficient number of experiments, or made a sufficient number of comparative trials, to enable him to state with any degree of certainty, that these particular manipulations and none others, were the exact and exclusive means to produce the effect desired. Thousands and tens of thousands of instances would be insufficient, as could be shown mathematically, to enable the experimenter to decide whether there should not, for example, be *three* shakings instead of *two*, or whether the triturations and the scrapings should not be each of *five* minutes, instead of the one being *six* and the other *four*. In the second place, it certainly has a very suspicious look of a foregone conclusion, rather than of a legitimate deduction from facts, that all the scrapings and rubbings to which each remedy is subjected, in each single stage of its transmigration, should occupy exactly *one hour* and not one minute more or less. (That time as well as the degree of friction, &c., is not a matter of indifference in Hahnemann's estimation, is obvious, from the change he was induced to make in the number of shakings, from the original ten to two.) And then the slow turning of the phial on its axis, the directing one set of triturations to be stronger than another, &c. Altogether, it must be admitted, that the whole complexion of the thing bears a much closer resemblance to what we have heard or seen of magical ceremonies and the tricks of conjurors, demonstrations for effect and to produce an impression, than to any operation of a scientific or *bona fide* character.

As to the argument founded on the alleged solvency of substances previously insoluble, it must go for nought, as it is well known that many substances are found in nature dissolved in minute quantities, or, which is the same thing, diffused in particles of invisible dimensions, in water and other fluids, although they are not soluble in mass or under ordinary circumstances.

Many other objections to the doctrines of homœopathy have been made; and it would not be difficult to add to their number; but enough has been already said to prove its unsoundness as a theory; and if it came before us only as a theory, it would be unnecessary to waste more time in the discussion of its merits. The days are long past in medicine, when anything merely theoretical could claim prolonged attention. No doctrine, however ingenious, not based on positive demonstrable facts, will any more be regarded but as a piece of poetical speculation, which may indeed amuse the fancy, but can never influence the conduct of scientific men, much less of practical physicians. But homœopathy comes before us in a much more imposing aspect, and claims our attention on grounds which cannot be gainsaid. It presents itself as a new art of medicine, as a mode of practice utterly at variance with that long established in the world; and claims the notice of mankind on the irresistible grounds of its superior power of curing diseases and preserving human life. And it comes before us now, not in the garb of a suppliant, unknown and helpless, but as a

considerable size in Germany, France, Italy, England, or America, that does not boast of possessing one or more homœopathic physicians, not a few of whom are men of high respectability and learning; many of them in large practice, and patronised especially by persons of high rank. New books on Homœopathy issue in abundance from the press; and journals exclusively devoted to its cause are printed and widely circulated in Europe and America. Numerous hospitals and dispensaries for the treatment of the poor on the new system have been established, many of which publish Reports blazoning its successes, not merely in warm phrases, but in the hard words and harder figures of statistical tables. The very fact of the publication of a *third* edition of such a large and expensive work as Dr. Laurie's (No. 9 of our list,) proves how widely the practice is spread among the public generally. The last triumph which homœopathy has achieved, is the conversion of the Professor of Pathology in the University of Edinburgh from the old faith.

As an established form of practical medicine, then; as a great fact in the history of our art; we must, nolentes volentes, consider homœopathy. If, as is maintained by its advocates, it is indeed true, that with its infinitesimal doses it cures diseases; nay more, that it cures them exactly according to the ancient *beau ideal* formula, *tuto cito et jucunde;* and cures them also in a larger proportion than is done by ordinary treatment; it matters but little whether its theory is false or true. If it can *prove* to us, that it does what we have just stated, we are bound to admit, and we are prepared to admit, that this is a kind of evidence sufficient to overthrow all the arguments we can bring against it, however strong, and all our reasonings, however just: improbabilities, however glaring, and even what seem impossibilities, must go for nothing. As Dr. Henderson truly says,

"It is in vain that physicians attempt to oppose the system by commenting on the flaws in the hypotheses formed to explain it, the incidents which are said by its founder to have led him to the discovery of what is peculiar in it, or the alleged blunders of its practitioners. There is no hypothesis in homœopathy that is of the smallest consequence to the practice of it. The question now is, not whether it originated in a mere speculation, or an induction of facts, but whether it be, as actually employed in the treatment of disease, a valuable acquisition to the practice of medicine; and it is of little consequence to the fundamental importance of the system, that its practitioners should be chargeable with occasional errors of diagnosis, as great, or greater, than those which are every day committed by others." (p. 44.)

In this point of view, then, what has homœopathy to present to us?

The subject here to be considered naturally divides itself into two parts: 1st, As to the *absolute power* of homœopathy to cure diseases: 2dly, As to its *power relatively to that of allopathy*.

1. In regard to the first head of the inquiry, we think we are justified in stating that no unquestionable evidence exists as to the absolute power of homœopathy to cure diseases. The only way in which this power could be effectively established, would be by the institution of an experiment, on the large scale, on two sets of parallel cases of disease, the one treated homœopathically, the other treated *apparently* in the same manner,

but with fictitious globules in lieu of the real globules of homœopathy. An experiment of this sort properly conducted on a sufficiently large number of persons, for a sufficiently long period, would settle the question of the absolute potency or impotency of the homœopathic treatment. At present, we have no such experiments conducted on a sufficiently large scale to render the result valid. Such experiments as have been made—and several have been made in the German hospitals—must be considered, as far as they go, as unfavorable to the claims of homœopathy. Experiments made in private practice, in a small number of cases, at most, are still less entitled to consideration : as far as they go, the results obtained by them also tell against homœopathy. Nevertheless, we are disposed to waive this evidence on the positive side of the question, as being inadequate, and therefore conclude, as above, that we have no unequivocal evidence to prove that medicines administered homœopathically, and in homœopathic doses, have a positive power of curing diseases.

2. On the second head of the inquiry, our evidence is very different both in character and amount. Here homœopathy can adduce evidence of precisely the same kind as allopathy. The homœopathic evidence, however, is so much less than the allopathic, in absolute amount, that we must declare, *in limine*, that it is quite insufficient to enable us to come to a sure conclusion on *the whole question* at issue between the parties. Much too short a period has elapsed since the establishment of homœopathy, for it to possess the requisite data that could enable it to contend with an opponent which has at command the accumulated materials supplied by millions of observers during an experience of two thousand years. And even admitting, as we readily do, that a vast part of those materials is utterly valueless, still it would be unfair to put them in competition with the scanty evidence furnished by a few observers during a few years,—which evidence, moreover, is precisely of the same general character as that of the older school, and consequently deteriorated by the same proportion of what is inadmissible. Nevertheless, it would surely be most unwise, and even unphilosophical, to come to the conclusion, that, because we are not yet in a position to decide the question absolutely and definitively, we should therefore refuse to entertain it at all. Matters that immediately and nearly concern human health, human happiness, or human life, cannot be so treated. And therefore it is, that, imperfect as the data are, we feel bound not to dismiss the subject of homœopathy without a brief inquiry, at least, into its pretensions and merits as a branch or form of practical medieine. In doing so, it is quite unnecessary, after what has just been stated, to sift *all* the evidence the new doctrine can supply us on this head; for the purpose we have in view, the materials contained in two of the books before us are quite sufficient.

The Hospital of the Sisters of Charity in Vienna was opened in 1832. It is situated in a healthy suburb, and has thus advantages over the great general hospital of the same city. It contains at present upwards of fifty beds. In the beginning of 1835, the management of the hospital was committed to Dr. Fleischmann, and since that period all the patients have been treated according to the homœopathic system exclusively. In the Introduction to the Study of Homœopathy, by Drs. Drysdale and Russel, there is a translation of a report of Dr. Fleischmann, exhibiting a tabular

view of the cases treated at this hospital during eight years—from the beginning of 1835 to the end of 1843. The total number of patients treated was 6551, and the following are the general results:

Remaining from 1834	27
Admitted	6524
Cured	5980
Dismissed uncured	112
Died	407
Remaining	50

The list includes all the usual diseases, acute and chronic, found in hospitals, and some surgical cases. The following extract shows the number and events of some of the more important and best marked diseases:

	Admitted.	Cured.	Uncured.	Died.	Recovered.
Abscess of the brain	3	..	..	3	
Apoplexy	9	4	2	3	
Cancer of stomach and uterus	5	..	2	3	
Amenorrhea and Chlorosis	90	89	..	..	1
Ascites	14	10	1	3	
Diarrhœa	114	112	..	2	
Dysentery	44	42	..	2	
Erysipelas of the face	181	177	1	2	1
Fever, excluding typhus	1036	1007	1	17	11
Typhus, abdominalis	819	669	2	140	8
Influenza	52	51	..	1	
Dyspeptic affections	173	172	..	..	1
Gout, acute and chronic	102	97	1	4	
Headaches, various	61	61			
Articular inflammations	211	203	..	2	6
Meningitis	17	15	1	1	
Bronchitis	15	15			
Ophthalmia	51	30	1		
Endocarditis	29	29			
Pericarditis	2	2			
Enteritis	6	1	..	5	
Pneumonia	300	280	..	19	1
Peritonitis	105	100	..	5	
Pleuritis	224	221	..	3	
Measles	25	23	..	2	
Phthisis	98	..	27	71	
Rheumatism, acute and chronic	188	188	..	..	
Scarlatina	35	31	..	2	2
Smallpox	136	120	..	11	5
Tonsillitis	300	299	..	..	1

It is well known to all physicians accustomed to statistical inquiries, that, without a minute classification of the individual diseases included in any general report of cases, showing the sex, age, condition of the patients, the precise character or *genius* of the prevailing diseases, the season, the date of the disease when brought under treatment, &c. &c., no trustworthy comparison can be instituted between any two lists of diseases, however similar in name, and although occurring in the same locality. The difficulty of comparison will, of course, be considerably enhanced, when the countries, nature of the localities, general habits, &c. &c. of the patients, are different in the two cases. It would, therefore, lead to no useful purpose to institute any close comparison of Dr. Fleischmann's bare skeleton tables with any similar tables of diseases treated allopathically in this country or

elsewhere. The conclusions deducible from such a comparison, whether for or against either mode of treatment, could not be admitted as of any positive weight in settling the practical question at issue. To enable us to do this effectually, we would require from each party an incomparably greater number of cases, observed and treated through a long period of time, and each disease discriminated, and all classified according to the rigid requirements of statistics. We do not, however, mean to say that such lists as those of Dr. Fleischmann's are unworthy of notice and incapable of furnishing any information of consequence. This is not the case. Although yielding us no positive results or such data as science demands, they unquestionably furnish us with isolated facts of great value, and even supply materials which may be worked into such rude approximations to truth, as medicine has, alas, been too long content withal. These tables, for instance, substantiate this momentous fact, that all our ordinary curable diseases are cured, in a fair proportion, under the homœopathic method of treatment. Not merely do we see thus cured all the slighter diseases, whether acute or chronic, which most men of experience know to be readily susceptible of cure under every variety of treatment and under no treatment at all; but even all the severer and more dangerous diseases, which most physicians, of whatever school, have been accustomed to consider as not only needing the interposition of art to assist nature in bringing them to a favorable and speedy termination, but demanding the employment of prompt and strong measures to prevent a fatal issue in a considerable proportion of cases. And such is the nature of the premises, that there can hardly be any mistake as to the justness of the inference. Dr. Fleischmann is a regular, well-educated physician, as capable of forming a true diagnosis as other practitioners, and he is considered by those who know him as a man of honour and respectability, and incapable of attesting a falsehood. We cannot, therefore, refuse to admit the accuracy of his statements as to matters of fact; or, at least, to admit them, with that liberal subtraction from the favorable side of the equation, which is required in the case of all statements made by the disciples and advocates of new doctrines. Even after this rectification, we see that enough remains to justify the inference above deduced. No candid physician, looking at the original report, or at the small part of it which we have extracted, will hesitate to acknowledge that the results there set forth would have been considered by him as satisfactory, if they had occurred in his own practice. The amount of deaths in the fevers and eruptive diseases is certainly below the ordinary proportion; but, for reasons already stated, no conclusion favorable to homœopathy can be thence deduced. It seems, however, reasonable to infer that, even in these cases, the new practice was not less favorable to the cure than the ordinary practice. In all such cases, experienced physicians have been long aware that the results, as to mortality, are nearly the same under all varieties of allopathic treatment. It would not surprise them, therefore, that a treatment like that of homœopathy, which they may regard as perfectly negative, should be fully as successful as their own. But the results presented to us in the severer internal inflammations, are certainly not such as most practical physicians would have expected to be obtained under the exclusive administration of a thousandth, a millionth, or a billionth part of a

grain of phosphorus, every two, three, or four hours. It would be very unreasonable to believe that, out of 300 cases of pneumonia, 224 cases of pleurisy, and 105 cases of peritonitis, (in all 629 cases,) spread over a period of eight years, *all* the cases, except the fatal ones, (27 in number,) were slight, and such as would have seemed to us hardly requiring treatment of any kind. In fact, according to all experience, such could not be the case. But, independently of this *a priori* argument, we have sufficient evidence to prove that many of the cases of pneumonia, at least, were severe cases. A few of these cases are reported in detail by Dr. Fleischmann himself, and we have ourselves had the statement corroborated by the private testimony of a physician (not a homœopath) who attended Dr. F.'s wards for three months. This gentleman watched the course of several cases of pneumonia and traced their progress, by the physical signs, through the different stages of congestion, hepatization, and resolution, up to a perfect cure, within a period of time which would have appeared short under the most energetic treatment of allopathy.

In examining Dr. Fleischmann's report, the sagacious physician will not fail to be struck by the fact, that the relative proportion of cures, and the relative mortality of the different diseases, one to another, are precisely the same as he is accustomed to see in his own practice. Slight diseases are all cured by the homœopathist, as by the allopathist; dangerous maladies kill a considerable proportion of the patients of both; very dangerous ones, a still larger proportion; and the class of diseases which all true observers and honest reporters have declared rebellious to their most strenuous medical efforts, are found to occupy the same black column in the tables of the old and the new school.

Thus: the cases of dyspepsia (173), the cases of headache (61), the cases of chlorosis (90), the cases of tonsillitis (300), the cases of simple rheumatism (188), the cases of bronchitis (15), are ALL *cured;* while we have 1 death in 52 cases of influenza, 2 deaths in 114 cases of diarrhea, 2 deaths in 181 cases of erysipelas of the face, 2 deaths in 211 cases of arthritis. As we advance to the still more dangerous class of cases, we find the loss proportionably greater: thus, out of 44 cases of dysentery we have 2 deaths, out of 9 cases of apoplexy we have 3 deaths; out of 14 cases of ascites 3 deaths, and 1 not cured; out of 1036 cases of ordinary fevers we have 17 deaths, while out of 819 cases of typhus we have 140 deaths; out of 524 cases of pneumonia and pleurisy we have 22 deaths; out of 105 cases of peritonitis 5 deaths, out of 136 cases of small-pox 11 deaths, out of 35 cases of scarlatina 2 deaths, and out of 6 cases of enteritis 5 deaths; while *all* the cases of cancer (5), *all* the cases of abscess of the brain (3), and *all* the cases of phthisis (98), are either registered as fatal, or as "dismissed uncured,"—which, of course, means the same thing. The only cases in the list which do not seem, at first sight, to come within the above category, are the cases of endocarditis and pericarditis (31), which are all reported as cured. These are, no doubt, severe diseases; and this may seem an uncommon amount of success; yet, when it is considered that the number of cases is not great, that the diagnosis of endocarditis, and even of pericarditis, is less easy and certain than that of many other diseases, and that it is not so much

in their primary condition as in their ultimate effects, that these diseases are dangerous, we believe that even the degree of success here recorded cannot, in fairness, be admitted as any deviation from the ordinary course of events in allopathic medication.

The remarks above made are even of more importance, in relation to the general subject now under consideration, than they may seem to be at first. They not only show that the *kind* of successes and failures experienced by the homœopathists, is precisely the same as that experienced by the allopathists; but they also seem to show that the medication of the former can boast of no *peculiar* virtue whereby it can achieve triumphs in fields altogether forbidden to the latter. Under the influence of medicines, all of which must be considered *new*—new absolutely, or new in their form, mode of administration, and principle of action,—we would have hardly expected to find the old relations of curability and incurability exactly preserved. Does not this fact, common to both, seem to point to a *community of power, or want of power*, in the two classes of agents, rather than to a speciality of action and potency in one?

The materials furnished by Professor Henderson towards the solution of the practical portion of the question agitated between the old and new systems, are very inferior in amount and in intrinsic value, to those of Dr. Fleischmann. They are, however, not unimportant; and they are the more to be prized as the evidence they supply is of a somewhat different kind from that adduced by the German physician.

The first part of Dr. Henderson's book consists of general observations deprecatory of the wholesale condemnation of homœopathy by the medical profession; of apologetical statements in relation to his own secession from the established faith; of arguments in defence of his own views against the accusations of his opponents; and a detail of reasons why homœopathy ought to be studied and tried, at least, if not embraced by the professors of medicine generally. This introduction is written in a philosophical, fair, and candid spirit, and bears the impress of sincerity. It is, however, a production of no great power, and is, moreover, disfigured by a vicious style. It is a tolerably fair specimen of that cloudy and verbose style first introduced by Dr. Chalmers, and which has seduced so many Scottish writers from the path of plain English. In this sort of writing, you gather the author's meaning rather from the impression conveyed by any passage as a whole mass of words, than by the direct communication of definite ideas by words and phrases of precise import. It is to the ordinary language of lettered Englishmen, what the mountains of Scotland, when enveloped in November mist, are to the same mountains when standing out clear and defined in the sun of June: you know the mountain is there, you recognize its broader features, but you see nothing clearly and distinctly.

The second part of Dr. Henderson's book, which alone we have here to do with, consists of a detail of one hundred and twenty-two cases of disease treated homœopathically by Dr. Henderson, in dispensary and private practice. They all bear the impress of being faithfully related, though most of them are deficient in essential details, and many of them

are utterly valueless to any class of inquirers. Dr. Henderson seems to have exercised his usual fairness in selecting the cases for publication :—

"I have" he says, "contented myself with adhering strictly to the following determination in regard to the details that I should publish, namely, that they should consist of every case of which an account was written at the time it first presented itself, and, of course, before anything was known of the effects which might succeed treatment. That determination, in respect to every case, so taken down, whether successful or not, has been fulfilled . . . . and I think a perusal of these cases will satisfy the reader that they have not been selected on the ground of anything that indicated the approach of a spontaneously favorable change, or made them to differ from the ordinary character of cases of the classes to which they respectively belong." "The following conditions for making a selection from among those (cases) that should occur, for the purpose of publication, were considered advisable, namely, that they should not be of a slight nature, such as commonly yield with ease to confinement and restriction of diet,—that they should not include disorders previously subject to repeated spontaneous alternations of decline and increase,—and that there should be some reason to suppose that the persons subjected to the treatment were likely to give a fair trial in point of time and attention. Cases of pulmonary consumption, and most of those in which old organic disease was the apparent cause of the sufferings which existed, I did not think likely to furnish important results in general [and were therefore omitted.]" (Pref. pp. 53-55.)

As our object in the present article is not to expose the failures or blunders of the homœopathists, but to endeavour to ascertain the truth, whatever it may be, respecting the alleged powers of homœopathy to cure diseases, we shall pass over all the cases in Dr. Henderson's list, which either tend to prove nothing, one way or other, or are more adverse than favorable to the claims of the new practice; and we shall give our principal attention to those cases which the author himself must consider the best, inasmuch as many of them certainly seem, at first sight, to bear unequivocal evidence in favour of the treatment adopted.

The first twenty-four cases are examples of acute disease of slight severity, and supply no evidence, *pro* or *con*, worth quoting. They are principally cases of tonsillitis, dysentery, and erysipelas of the face. Every physician of experience would have expected them to get well under any treatment. It is but fair, however, to say, that they got well, as fast apparently under Dr. Henderson's treatment, as they would have done under ordinary medication. The twenty-fifth and twenty-sixth cases are well marked cases of acute rheumatism. They terminated in the short space of about five days, under the use of bryonia, assisted occasionally by aconite and belladonna, in doses of a billionth of a grain. The twenty-ninth and thirtieth cases are cases of severe neuralgia, the former speedily relieved after *one* dose of dulcamara, the latter, after *one* dose of aconite. It would be unfair to deny, that the result obtained in these four last cases would have been regarded as very satisfactory under any mode of allopathic treatment.

The thirty-third and thirty-fourth are cases of pneumonia. The first case proved fatal; but as the treatment was partly homœopathic partly allopathic, no inference can be drawn from it. The second was a well-marked case in a girl ten years of age. All the ordinary physical signs existed. Phosphorus and bryonia were the principal remedies administered, and the patient was convalescent about the ninth day.

After the large list of cases of pneumonia, successfully treated by Dr. Fleischmann, the result of this of Dr. Henderson's creates no surprise, and adds nothing to the strength of the evidence in favour of the homœopathic treatment of this disease. Such results may indeed astonish our heroic bleeders and mercurializers, or may even turn them, being so full of faith in drugs, to the pole opposite to heroism, homœopathy itself. But if mere recovery from an attack of pneumonia is to be admitted as evidence in favour of treatment, our heroes of the lancet and pill have other claimants for their suffrages besides the homœopathists. "In order to appreciate thoroughly (says M. Grisolle) the value of the various kinds of treatment cried up in pneumonia, it is indispensable that we should know accurately the progress, duration, and most frequent termination of it when treated purely on the expectant plan; but we have not this medium of comparison. It is indeed true, that M. Biett treated during a whole year all the cases of pneumonia that came into his wards, with emollient drinks and cataplasms only, and the mortality was very inconsiderable. M. Magendie employs no other treatment in the same disease."* We may add that, to our knowledge, the same plan has been followed in one at least of the large hospitals of Germany, and the result was considered to have been far from unsatisfactory. And M. Grisolle informs us, that he himself, in the year 1840, treated eleven cases of pneumonia, "all perfectly characterized by the auscultatory phenomena, and by the expectoration," nearly in the same manner. The whole treatment consisted in confinement to bed, rigid diet, pectoral ptisans, and (rarely) a mild laxative such as castor oil. All the patients perfectly recovered, the mean term of convalescence being the 11th or 12th day. Dr. Henderson misjudges these cases in terming them "slight," in comparison with the one treated by him. They seem to have been fully as severe; and although M. Grisolle himself speaks of the symptoms as "sufficiently mild" to justify the experiment he undertook, they can hardly be regarded as slight, when we are told, that in nine out of the eleven cases, the disease "had reached the stage of red hepatization" before the treatment commenced.

Dr. Henderson's cases, No. 36 to 46 inclusive, are examples of headache, chiefly chronic, many severe, of long standing, and several of them rebellious to all former treatment. The results of the treatment of these cases are, upon the whole, very favorable; and several of the *cures* would, unquestionably, if occurring under ordinary treatment, have been regarded as not only striking but triumphant. We quote one of these.

"CASE XLIII.—*An Unmarried Lady, aged* 30.

"1*st April* 1845.—She is very spare and sallow, and subject to severe headaches. The pain affects the whole head, and is particularly intense on the right side and front, especially above the eyes. It is of a heavy compressive character, and accompanied by a sense of heat. Sometimes on the right side acute shootings occur. There is also much giddiness during the attacks.

"The sufferings come on in paroxysms, which last about twelve hours, and are particularly severe in the mornings, and attended with flushing often. Nausea and vomiting also accompany the attacks generally, and last for several hours. She is obliged to remain in bed while the paroxysms continue. Though they occur commonly in the morning, they are easily brought on by fatigue, and some-

* Traité Pratique de la Pneumonie, p. 560. † Ibid. p. 561.

times by even moderate exercise in the open air. She cannot endure a bright light or noise when they are present, without the sufferings being aggravated. *She is rarely more than two days free from the severe attacks, and has more or less headache every morning, which goes off after breakfast.* Bowels regular. She does not take medicine of any kind, as it never gives her relief. She sleeps well. The catamenia are regular, and occasionally excessive. Tongue clean. *She has been subject to these attacks for above sixteen years, without any material difference during the whole time.* Bellad. 12, 2ce a day.

"*9th.*—She has had no severe headache since; indeed, any pain that has occurred has been so slight, that it is only on particular inquiry that she states she has had any. What has occurred has been very slight, only on one or two occasions, and for a very short period. No nausea or vomiting. Cont. Bellad.

"*15th.*—No severe attacks, and only slight headaches in the morning, ceasing after breakfast. Calcarea 6, 30, once a day.

"*29th*—The slight morning headaches continue. Sepia 30, once a-day.

"*6th May.*—The slight headaches have been very trivial, and for some days absent. No other ailment. Sepia 30, every second day.

"*26th.*—No headache of any kind since last report; a little confusion only on getting up. No nausea, &c. Omit Sepia

"*2d July.*—Has continued perfectly well." (pp. 121-2.)

Had this case of purely nervous headache come under the care of any of us allopathists, and had we prescribed for it "nervines," or "antispasmodics," or arsenic, steel, or other "tonics," according to our theories, our school, our experience, or our fancy, we should have certainly gained great credit from our patient and her friends for our "wonderful cure;" and some of us, doubtless, would have received it as our due. Had it taken place in the practice of a zealous and ambitious doctor under six-and-thirty, the probability is immense that it would have occupied a niche among the triumphs of medicine which crowd the weekly pages of our contemporaries. And we can see no sufficient reason why Dr. Henderson should not have his credit also. Philosophers, indeed, and hard-headed sceptics like ourselves, might demur to the claims of both; and might seek for an explanation of the facts beyond the limits of both *pathies.* Headaches and other nervous maladies do sometimes come to an end of their own accord; and as such an event is certainly *possible*, even immediately after the swallowing of a new drug, (and, possibly, *in consequence of the very swallowing*, and not the *drug*,) we leave it for the consideration and calculation of the wise, which of these two events was most probable: 1st, That the headache might have *chanced* to stop of its own accord on the very day it did, or was *charmed* away by the very *prestige* of homœopathy acting through the imagination; or, 2d, That one quadrillionth (1000000,000000,000000,000000,000000,000000*th*) of a grain of belladonna, and one decillionth (our printer has not *noughts* enough for this,) of a grain of the all-potent "sepia" did the feat.—*Non nobis est tantas.*

Cases 56 to 82 are examples of chronic disorders of the stomach and bowels, under the various forms of gastralgia, vomiting, dyspepsia, constipation, with headaches, &c. They are of the same general complexion, both as to character and result, as the cases of more formal headache. Several of them are certainly striking examples of rapid and most brilliant cures—cures, that is, just as good and as well authenticated as those of allopathy—*post hoc, ergo propter hoc.* We quote one case in illustration, chiefly because it is short.

"CASE LXXV.—*A Clergyman, aged about* 35.

"*6th May*, 1844.—Spare, and of ordinary complexion. Incapable of considerable exertion without fatigue. Had been subject to dyspepsia for a long time, and to irregularity of the bowels, which were habitually slow, and often constipated. *At length it became necessary for him to take a pill every second night, which he has continued to do habitually for above two years.* When so situated as to be unable to take his usual aperient, the bowels are confined for several days, and until he has recourse to medicine. Nux vom. 3, night and morning.

"*10th.*—Bowels began to act moderately on the 7th, and have been moved daily since, but not copiously. Cont.

"*19th.*—Bowels act daily without pills. Cont.

"*19th June.*—Has continued to take the Nux vom., and the bowels have been perfectly easy and regular.

"*21st August.*—Has taken no Nux vomica for six weeks, and the bowels have been in excellent order. His general health is better since commencing the treatment than for years before; his appearance is more robust and ruddy, and his strength is much improved.

"*January* 1845.—Has continued well in every respect." (pp. 171-2.)

Can anything in therapeutics surpass the evidence of the marvellous effect produced in this case by the 1000,000 part of a grain of nux vomica? One thing at least is certain, that the practice of Dr. Henderson conferred an inestimable benefit on his patient. But whether his *nux vom.* 3, was the cause or the occasion of this, is a question which may receive some elucidation from the contents of an admirable pamphlet published some years since by Dr. Henry of Dublin, entitled, 'A Dialogue between a Bilious Patient and a Physician,'* the object of which was to prove the evil of habitual medicine-taking, and the all-sufficiency of diet and exercise in such cases. Dr. Henderson's case may even have some light thrown on it from a much humbler source. Many years since, in the golden prime of our dispensary days, when we had remedies for every disease, and faith in many, we well remember, on one occasion, to have been not a little gratified and flattered by the brisk and cheerful response of a little girl to our morning query of "How do you do, my dear?" "Oh! I'm a great deal better *now*, Sir," said the little girl. Bravo! thought we, for our new mixture of seven ingredients—with its Basis, its Adjuvans, its Dirigens, its Corrigens, its Constituens! † "How long have you been so much better?" we asked, wishing to know the very hour of our triumph. "Since Friday, Sir," said the little girl. "Hah! what happened on Friday?" said we, musing as to the particular crisis which we had brought to pass. "Please Sir," said the little girl with a curtsey, "Please Sir, my medicine was done." And so, possibly, when this honest clergyman began to take the inconceivable *nux vom.* 3, the actual *medicine* which he had been swallowing for years *was*, like the little girl's, *done*.

Case 86 is a good one for the *post-hoc* school, whether homœopathic or allopathic:

"*A young lady aged* 19. August 5. For between two and three years has been subject to diarrhea with pain in the bowels, *after intervals rarely exceeding a week*. The attacks last for several days, and the bowels are moved from six to ten times a day. She is ill at present with one of them. [How many days has she been ill?] *Pulsat.* 6, *twice a day*. 29th. A day or two after last report the diarrhea ceased, and has not recurred. 10th Sept. Continues without having

* See this Journal, vol. VII, p. 476.

† See Paris's Pharmacologia.

had a return of diarrhea; a length of interval which she *does not remember* to have occurred since the complaint began." (p. 181.)

When the intervals *did* exceed a week, how much did they exceed it? Did they ever reach four weeks? If the young lady could not *remember* this, Dr. Henderson should have made inquiry of those who could, before he adduced this flimsy case as evidence of the potency of his billionth of a grain of pulsatilla. Does Dr. Henderson think it a strange thing in the economy of nature, and only to be explained by the *Deus ex machina* of homœopathy, that a case of diarrhea, *characterized* by intervals of health, should stop *as usual*, although an incomprehensible something was given, and that it should not return for a few days longer on one particular occasion? These may seem little things to comment on, but surely little things will not be despised by the homœopathists of all men; and here they very significantly show the sort of philosophy we have to deal with. Men capable of admitting cases of this kind as evidence—and we could extract fifty from Dr. Henderson's book much feebler than this—are demonstrably disqualified to treat of things which demand for their handling the stern logic of a masculine mind.

While on the subject of diarrhea, we may here state a little fact not very irrelevant to the present discussion. Many years ago, when in charge of a large body of men in the public service, we had occasion to treat an epidemic diarrhea, of considerable violence but not dangerous. Finding our patients recover as fast under one as another of several methods of treatment adopted, we thought there would be no unpardonable *lèse-majesté* either to our royal master of London or our divine master of Delos, in carrying our trials one step further. Accordingly, we put half of our remaining patients on a course of orthodox physic, and half on homœopathic doses of flour (*farin.* 30) in the shape of bread-pills; and it puzzled us sadly to say which was the most successful treatment. *Query:* As there certainly *was* a decillionth of flour in each of our doses, and as this had undergone not a few "triturations and scrapings and shakings" in the barn, in the mill, on the crane, in the warehouse, in the joltings of a long land-journey, and in the infinitesimal vibrations of a ship in a long sea-voyage (we were then within the tropics), in the bakery, in the surgery, in the mortar (unglazed), on the slab, in the pill-box, in the patient's hand (with two arm-shakings,) in his mouth, in his throat, in his œsophagus,—who shall deny us the merit of having wrought our cures homœopathically? If it is asserted that *farina* is not found among the homœopathic remedies, we reply that charcoal is, and the *onus probandi* that the one is not as good as the other, lies with our opponents. If it is asserted that *farina* has not been "proved" on the healthy, and that it therefore comes not within the category of the *similia similibus*, we content ourselves with simply denying both assertions, and we pledge ourselves to produce, on trial, as many symptoms in a healthy man with the one as with the other. But even if our theoretical arguments should be rebutted, we take up our ground with Hahnemann and Dr. Henderson, and reply to all cavillers, that we have evidence beyond and above all theory—we have the irrefragable evidence of *facts*. For why? Have we not given our remedy, and has not a cure ensued? And "must not vain declamations be silent in the presence of infallible experience?"

It is unnecessary to proceed with the examination of Dr. Henderson's cases. They are all of the same general character; and the minutest analysis of them would not alter the conclusion to which the portion already commented on, infallibly lead. This conclusion is similar to that we drew from the cases of Dr. Fleischmann. In the present case we shall state it in the words of Dr. Henderson himself:—

"I can hardly conceive," says Dr. Henderson, "that those who are better entitled to judge, will find it difficult to admit, on the supposition that the cases have been exactly as related, that there has been a proportion of success among them with which they would have been fully satisfied, as the result of the ordinary means." (p. 49.)

Whether we may be ranked among those "who are better able to judge," or not, we do not know; but we do not hesitate to declare, that the amount of success obtained by Dr. Henderson in the treatment of his cases, would have been considered by ourselves as very satisfactory, had we been treating the same cases according to the rules of ordinary medicine.

In making these admissions in respect to the instances of treatment supplied by Drs. Fleischmann and Henderson, we wish formally to guard ourselves against being supposed to admit, at the same time, as if it were one and the same thing, or as if the one was a corollary of the other, that the result of the homœopathic treatment *generally*, is, and will be, as successful as the result of the ordinary treatment generally. It is *possible* that this may be the case; but, as we have no certain evidence that it is so, it would be absurd on our part to assume that this is the fact. We wish to keep strictly within the record, which goes no further than this, that a certain definite number of cases of disease, treated homœopathically by these two gentlemen, appear to have had as successful results as if they had been treated allopathically, or according to one or other of the prevailing modes of ordinary practice. No documents are in existence calculated to lead to a judgment of the *general question* at issue between the two doctrines.

But many of our readers, we expect, will be of opinion that, in admitting what we have done, we are betraying the cause of legitimate medicine, and lending our aid to extend the heresy of homœopathy. If such should be the result of our admissions, we cannot help it. We have said only what we believe to be true; and if what we believe is in reality the truth, the promulgation of it cannot lead to evil. Truth is good. If the art of medicine, as we profess and practise it, cannot bear investigation, and shrinks before the light of truth, from whatsoever quarter it may come, it is high time that it should cease to be sanctioned and upheld by philosophers and honest men. If, on the contrary, it be true and good—even if it be only but partially true and moderately good—the stirring touch of inquiry and the stimulus of opposition cannot fail to benefit it in the end.

What, then, it will naturally be asked, is the explanation of the momentous fact we have announced, that a considerable number of diseases have been, and perhaps continue to be, treated as successfully by homœopathists as by allopathists? *Is it, that the one kind of treatment is as good as the other?* IS IT, THAT HOMŒOPATHY IS TRUE?

To both of these queries we give an unequivocal and decided negative,

so far, at least, as this can be given in a case where we have, as yet, no demonstrative proof on one of the sides of the question. We may, indeed, have proof sufficient to satisfy any reasonable mind, that the theory or doctrines, or principles of homœopathy are false; but, as yet, we have no demonstrative evidence that it is false in its practical bearings—false, that is, powerless, as a means of curing diseases. It will not be disputed by any one conversant with the history of medicine, that these two things are not only distinct, but independent of each other. We can, however, assert with the greatest positiveness, that, as far as the evidence supplied by the documents now before us, or the evidence we have been able to gather from other published writings of the new school, goes,—there exists not a tittle of actual PROOF that homœopathy is true in this aspect. On the other hand, we have not a little positive evidence to prove that it has often failed to cure in cases where, according to its principles and the alleged experience of its professors, it ought to have cured, and in which allopathy did effect a cure. Still, this is only negative proof, and might be accounted for or explained away on grounds that would not necessarily compromise the existence of homœopathy as a means of cure. In a case so extraordinary, so marvellous, it may be said, as that of homœopathy, nothing short of the most positive and demonstrative evidence of its curative powers can be accepted; nothing, in short, will suffice but the *experimentum crucis* of a comparative trial, on the large scale, of *its* powers, on the one hand, and of *nature's* powers, on the other. Until it can be proved by clinical experience on an extended field, and on two parallel groups of similar diseases, that homœopathy cures better than nature, we are warranted by every principle of philosophy, not merely in doubting, but in denying its truth.

It would be easy to give numerous and strong reasons for the necessity of insisting on this extreme degree of evidence in the case of homœopathy. We shall only here advert to a single one. If, for the sake of argument, we were to admit that homœopathy were partially true, and, therefore, that it might fairly be received as one of the recognized methods of treating diseases, it would appear to us, according to our present light, to be very unfortunate for medicine if this were done. The guiding principles of homœopathy appear to us to be of that character, which must render its exercise very injurious to medicine as a branch of science. Based, as it is, on mere extrinsic, secondary phenomena, or symptoms, and exclusively engaged in the search for and adaptation of specific remedies to such phenomena, we cannot but regard it as calculated to destroy all scientific progress in medicine, and to degrade the minds of those who practise it. Its direct tendency seems to be that of severing medicine from the sciences, and establishing it as a mere art, and thus converting physicians from philosophers to artisans. Of course, if, by such a conversion, diseases were to be better treated and more speedily and frequently cured, it would be not only absurd, but transcendently wicked so to sacrifice the welfare of humanity for the sake of a scientific phantom; but, as we have said, it is anything but proved that such a result would follow the change, and therefore, until the proof is obtained, it behoves all who regard the prosperity and dignity of true art, to resist its progress.

But, such being our estimate of the character and powers of homœopathy, on what principle can we explain the fact, above admitted, that diseases have been cured and continue to be cured, alike under its ministration as under that of ordinary practice? *Is it, that* ALLOPATHY *is false also? Or is it, that, to obtain an explanation of the fact, we must pass by both, and fix on some* THIRD POWER, *coincident with both, yet belonging to neither?*

We cannot give to these queries, as to the former, either a simple negative, or simple positive reply. In answer to the first, we would say, that allopathy is certainly *true*, in a limited sense, that is to say, it unquestionably possesses, to a certain extent, the power of curing diseases. It is, however, *not true*, in an absolute sense, or in the sense in which it is regarded by some, inasmuch as it does *not* cure a great proportion of the diseases it is supposed to cure. In answer to the second, we admit that there is a third power, common to or coincident with both, which, while it explains all the triumphs of homœopathy, reduces those of allopathy within much narrower limits than its more zealous votaries are wont to assign it: *this is* THE POWER OF NATURE.

And here we must be permitted to enter into a little detail; as the placing this subject in its true light appears to us a matter of great importance, not merely in relation to the main object of the present discussion, but in its bearings on the subject of Practical Medicine generally, and especially on the momentous question of its improvement, or, if we may be allowed to say so, its REFORMATION, which we think is impending.

Much confusion and difficulty have been thrown over our consideration of the question of the nature and powers of homœopathy, and many disturbing and distorting influences here come into play in our attempts to form a just estimate of the value of allopathy, because of our misappreciation, on the one hand, of the actual powers of nature in freeing the body from the diseases that arise in it, and because of misappreciation, on the other hand, of the powers of art in working to the same end.

Health is such a blessing and disease such an evil, that the existence of the desire to get rid of the latter, and thus to recover the former, must be coextensive with the possession of reason by the organism that suffers. Strongly to desire is equivalent to the origination of action to gratify the feeling. Hence the origin of the medical art, which must have been coeval with the origin of man himself; hence the conception and formation of plans for the purpose of relieving pain, and of theories to account for and explain them, springing up in the mind of the first sufferers, and growing in number and variety from that time to the present; hence the constant interference of art with the natural processes of disease in the human body. When in process of time, medicine came to be established as a distinct profession, such interference necessarily became much more frequent and much greater; until, at length, the result was, that *all* diseases occurring in civilized communities, were interfered with as a matter of course. In the long succession of human generations, almost everything possible, physical or moral, was at one time or other tried, with the view of proving its possession or non-possession of remedial powers. The necessary consequence has been, the fixing in the minds of men, not merely of the professors of the medical art but of mankind

in general, these two notions, first,—that nature was inadequate to the cure of most diseases, certainly of severe diseases; and, secondly, that art was adequate. And these notions have not only come down to us as heirlooms of physic, but have been almost universally received as axioms, without investigation, both by the medical profession and the public. The result of all this has been, that the members of the medical profession at all times, and more especially in modern times, have been kept in a state of forced ignorance of the natural progress and event of diseases; in other words, of the true *natural history of diseases* in the human body; and they have been and continue to be almost as ignorant of the actual power of remedies in modifying, controlling, or removing diseases, and from the self-same cause, viz., that as art has almost always been permitted to interfere in the morbid process, it has been impossible to say what part, if any, of the result was attributable to nature, or what part to the remedies employed.

And yet, that nature *can* cure diseases without assistance from art, is a fact demonstrated by evidence of the most unequivocal kind, and of almost boundless extent. It suffices here to refer cursorily to a few of the more open sources of such evidence.

1. The cure of diseases among uncivilized nations of ancient and modern times, under the sole influence of magic, charms, or other practices equally ineffective.

2. The general treatment of diseases in the ruder and simpler times of physic, as recorded in the writings of the early fathers of our art.

3. The record of innumerable cases in the works of medical authors, more particularly before the eighteenth century, in which, from various causes, no medical treatment, or one demonstrably powerless, was employed.

4. The records of the *Expectant system* of medicine, long and extensively prevalent in various parts of Europe; also of other analogous systems of practice in vogue at different times in various countries, which could exert no substantial influence on disease or on the animal economy.

5. The wide-spread and frequently the exclusive employment, more especially in modern times, of universal, or as they are now called, quack medicines, under the use of which almost all curable diseases have frequently got well. Whether these medicines consist of inert substances, or of substances of positive medicinal power, the inference derived from their employment is nearly the same. All of them have, most indubitably, *cured* (to use this word in its common acceptation) a vast number of diseases; and whether the event was consequent on the use of a substance of no real power, or possessing *a particular power only*, must be allowed to be nearly the same thing. In our own day we have seen many large fortunes made in this country by the sale of various patent drugs of this kind—from Solomon's Balm of Gilead to Parr's Life Pills; and this fact alone proves their *real efficacy*, that is, proves it on the very same grounds of evidence admitted in legitimate medicine. Success, that is, the apparent cure of diseases on an extensive scale, could alone keep up a sale of them so extensive as to enable their proprietors to accumulate large fortunes. And of this kind of success—that is, the getting-well of patients under their use, according to the legitimate *post-hoc* mode of reasoning, every medical man must have witnessed many instances.

6. The now fashionable system of Hydropathy furnishes strong and extensive evidence of a like kind, although on somewhat different grounds. This mode of treating diseases is unquestionably far from inert, and most opposed to the cure of diseases by the undisturbed processes of nature. It, in fact, perhaps affords the very best evidence we possess of the curative powers of art, and is, unquestionably, when rationally regulated, a most effective mode of treatment in many diseases. Still it puts, in a striking light, if not exactly the curative powers of nature, at least the possibility, nay facility, with which all the ordinary instruments of medical cure (drugs) may be dispensed with. If so many and such various diseases get well entirely without drugs, under one special mode of treatment, is it not more than probable, that a treatment consisting almost exclusively of drugs, may be often of non-effect, sometimes of injurious effect?

An intelligent and well-educated hydropathical physician, on whose testimony I can entirely rely, informs me, that in a great many cases that have come under his care in a hydropathic establishment, he has observed the symptoms amend on the first commencement of hydropathic remedies, with a suddenness and speed which he could not conscientiously ascribe to the influence of the means used, but which rather appeared to result from the abandonment of injurious drugs which the patients had previously been in the habit of taking. In some cases, to test this point, the physician purposely abstained from treating the patients at all, and yet witnessed the same marked amendment. My informant points out to me another natural field of observation in this line, in the numerous patients discharged, cured, or relieved, from hydropathic establishments, almost all of whom carry with them such a horror of drugs that they never have recourse to them, if it can be helped, afterwards. Yet these people recover from their subsequent diseases—even without Hydropathy!

7. Mesmerism, also, we think, must come either within the category of cases illustrating the curative powers of nature, or, at least, the non-necessity of drugs, or both.

8. We may next instance a large and important class of cases, in which some philosophical physicians, in all times, have instituted direct experiments, both publicly and privately, to test the powers of nature, by either withholding all means of treatment, or by prescribing substances totally inert; the result often being the cure of many diseases under such management.

9. Lastly, we must advert to what is, perhaps, the most extensive and valuable source of all—the actual practice of the more scientific physicians of all ages, in the latter part of their career,—men of philosophic minds as well as of much experience. It is well known, from the history of physic, that a large proportion of men of this class have, in their old age, abandoned much of the energetic and perturbing medication of their early practice, and trusted greatly to the remedial powers of nature. The saying of a highly respected and very learned physician of Edinburgh, still living at an advanced age, very happily illustrates this point. On some one boasting before him of the marvellous cures wrought by the small doses of the Homœopathists, he said, "this was no peculiar cause for boasting, as he himself had, for the last two years, been curing his patients with even less, viz., with nothing at all!"

The candid consideration of what precedes will, we hope, go far to satisfy the minds of most men, of the justness of the conclusion previously come to by us—viz. That the curative powers of nature suffice to explain all the triumphs of homœopathy: we think the consideration of some additional influences essentially connected with the exercise of the new system, must entirely remove all doubt on the subject. We will here specify a few of these influences.

1. The abandonment of all previous medication, often, doubtless, of injurious influence on the malady; and the free field thus left for the operation of the *vis medicatrix*:

> "For Nature then has room to work her way;
> And doing nothing often has prevailed,
> When ten physicians have prescribed and failed."

2. A much stricter regulation of the diet and regimen, including the entire omission of vinous and other alcoholic drinks, nervous and other stimulants, as tea, coffee, pepper, &c.

3. The influence of imagination, stimulated by previous belief of the potency of the remedies, and nourished by fervent faith, hope, &c. Of course we know that this argument will not apply to the cases of infants, and that very small number of patients who are not made aware of the nature of the treatment to which they are subjected. As an equivalent, at least, to the latter part of this objection, we can adduce the fact, repeatedly established, of the non-effect of the homœopathic remedies, when experimentally administered by allopathic practitioners, without the knowledge of the patient.

4. The indirect influence of this faith, hope, &c. in inducing patience, so that time is allowed for nature to work the cure in her own way. And here we would remark, that the establishment of this most desirable state of mind—itself, by the way, directly curative—is an event much more likely to occur in homœopathic than in allopathic practice. The conscientious homœopathist, who believes he has selected the proper remedy, must possess a degree of confidence in the result of his medication, which an ordinary practitioner can be but rarely justified in feeling. Acting on the principle of specifics, the former can wait in patience for the event, which is that of the subsidence of the disease simply; while the latter, acting, for the most part, only indirectly on the disease, and obliged to vary his means according to circumstances, is sure, in every long disease, more or less, to lose patience and entertain doubt; and the betrayal of such feelings to the patient (and they can hardly be concealed) has a most unfavorable effect on the cure, by destroying faith, confidence, and hope. It may be added, that these most desirable states of mind are much more likely to arise and be retained where, as in homœopathic practice, *no obvious effect* on the system is seen, or expected to be produced by the medicines, than where, as in ordinary practice, a decided and visible effect is produced, and yet no amendment ensues.

But while we are thus exalting the powers of nature at the expense of homœopathy, are we not, at the same time, laying bare the nakedness of our own cherished allopathy? If it is nature that cures in homœopathy, and if homoeopathy (as we have admitted) does thus cure, in certain cases,

as well as allopathy, do we not, by this admission, inevitably expose ourselves defenceless to the shock of the tremendous inference,—that the treatment of many diseases on the ordinary plan must, at the very best, be useless; while it inflicts on our patients some serious evils that homœopathy is free from, such as, the swallowing of disagreeable and expensive drugs, and the frequently painful and almost always unpleasant effects produced by them during their operation? This inference, and the dilemma it involves, are always held up by the homœopathists *in terrorem* to any allopathist who should think of using the argument of nature's *autocrateia* against their system; and they think the threat too terrible to be encountered with disregard, much less with defiance, by any man in the actual practice of allopathy.

"If the latter be true [the negative quality of homœopathic practice] what a fearful judgment," say Drs. Drysdale and Russel, "must necessarily be formed of the allopathic method! . . . This view is one of momentous consequence to the practitioners of medicine; and we trust it will have its due effect in leading them seriously to reflect on their responsibility—on the awful circumstances in which they voluntarily place themselves." (p. 235.)

"It is often said," adds Dr. Henderson, "that the benefits of homœopathy flow mainly from the omission of medicine altogether, of which the system is supposed by its opponents in reality to consist. This opinion had better be reconsidered, if it lead to the practical inference, as I think it does, that some 80 or 90 per cent. of the patients who employ medical practitioners would be better off without them." (p. 237.)

These threats do not deter us from accepting the horn of the dilemma presented to us; nor do we think it worth while cavilling about the precise amount of the estimate involved in Dr. Henderson's inference. This may or may not be accurate; we believe that it is exaggerated; but be this as it may, we concede at once to him the truth of his general proposition; *and still adhere to* ALLOPATHY. In doing so, we consider that though we are embracing a system extremely imperfect, we are, at least, embracing one which, with all its faults, contains a considerable amount of truth, and a yet greater amount of good; and which, above all, is, or may be made, in its exercise, consonant with the principles of science, and is capable of indefinite improvement; while, in rejecting homœopathy, we consider that we are discarding what is, at once, false and bad—useless to the sufferer and degrading to the physician.

So much for HOMŒOPATHY—for the present at least. It only now remains for us to add a few momentous words on ALLOPATHY—words which have long had their prototypes in our thoughts, but which now find formal utterance for the first time, forced from us, as it were, by the immediate pressure of the important discussion in which we have been engaged. Before entering, however, on this concluding portion of our task, we must be allowed to make an explanatory remark, relating partly to something that precedes as well as to what is to follow. The reader will now understand the precise meaning of an expression we made use of in the commencement of this article, to the effect,—that Homœopathy would probably be the cause of more important fundamental changes in the practice of medicine, than any previous system since the days of Galen. In repeating our belief of the correctness of this statement, we

will add, that in this respect, if in no other, the doctrine of Hahnemann will have conferred an inestimable benefit on the healing art. Regarding Homœopathy, as it will now be seen we do, as a system of medicine, which essentially leaves diseases to the operations of nature, we must consider it as having been the means of instituting a grand natural experiment in therapeutics, which, though of vital importance to our art, could not have been compassed by any other means we know of. From the results of this experiment, conducted as it is on the most extensive scale, and likely to be prolonged through an indefinite period, we have the prospect of obtaining at last, a true natural history of human diseases, and the means of ascertaining the actual powers of nature in relieving or removing them; and, as a corollary of this knowledge, the real powers of art in the same field. We may also hope to learn from the same source, directly or indirectly, the proper occasions for applying and withholding the instruments by which art works, and to discriminate accurately the effects produced by one class of operations from those produced by the other; so as to come, at length, to something like an appreciation of the true powers and actions of remedies, of which, at present, we are lamentably ignorant. In truth, a portion, though only a small portion, of this most important knowledge has been already obtained from the experiment; just enough to show us more clearly than before, the extent of our ignorance,—the first step to knowledge. We hope this will appear from the brief exposition of the present state of Therapeutics which we are now to make.

In finishing our examination of the writings of the Homœopathists, we said, that we did not shrink from admitting and adopting the inferences—however unfavorable to Allopathy — which seemed necessarily to flow from the results of their treatment of diseases. The principal of these inferences have been already stated more than once. It seems necessary, however, to recapitulate the more important of them here. These are:—

1. That in a large proportion of the cases treated by allopathic physicians, the disease is cured by nature, and not by them.

2. That in a lesser, but still not a small proportion, the disease is cured by nature, in spite of them; in other words, their interference opposing, instead of assisting the cure.

3. That, consequently, in a considerable proportion of diseases, it would fare as well, or better, with patients, in the actual condition of the medical art, as more generally practised, if all remedies, at least all active remedies, especially drugs, were abandoned.

We repeat our readiness to admit these inferences as just, and to abide by the consequences of their adoption. We believe they are true. We grieve sincerely to believe them to be so; but so believing, their rejection is no longer in our power; we must receive them as facts, until they are proved not to be so.

Although Homœopathy has brought more signally into the common daylight this lamentable condition of medicine regarded as a practical art, it was one well known before to all philosophical and experienced physicians.

It is, in truth, a fact of such magnitude,—one so palpably evident, that it was impossible for any careful reader of the history of medicine, or any

long observer of the processes of disease, not to be aware of it. What, indeed, is the history of medicine but a history of perpetual changes in the opinions and practice of its professors, respecting the very same subjects—the nature and treatment of diseases? And, amid all these changes, often extreme and directly opposed to one another, do we not find these very diseases, the subject of them, remaining (with some exceptions) still the same in their progress and general event? Sometimes, no doubt, we observe changes in the character and event, obviously depending on the change in the treatment,—and, alas, as often for the worse as for the better; but it holds good as a general rule, that, amid all the changes of the treatment, the proportion of cures and of deaths has remained nearly the same, or, at least, if it has varied, the variation has borne no fixed relation to the difference of treatment.

In making this statement, we are far from denying that practical medicine has made considerable progress since it was first established as an art, or that we do not now cure more diseases and save more lives than our forefathers did. The truth of our assertion,—taken as a general assertion, and when the question is regarded in the only way it ought to be regarded, in an approximative, not in an absolute sense,—is not thereby in any respect invalidated. We do not deny that medicine has made progress, or that it can cure diseases and save life;—we merely assert that the *superiority in the proportion* of the instances in which it does so, in the present day, is most lamentably small, all things considered, when placed side by side with the amount of any former day. In several of our commonest and most important diseases, it is hardly to be questioned that the proportion is little, if at all, on our side, and in others it is manifestly against us.

This comparative powerlessness and positive uncertainty of medicine, is also exhibited in a striking light, when we come to trace the history and fortunes of particular remedies and modes of treatment, and observe the notions of practitioners, at different times, respecting their positive or relative value. What difference of opinion, what an array of alleged facts directly at variance with each other, what contradictions, what opposite results of a like experience, what ups and downs, what glorification and degradation of the same remedy, what confidence now—what despair anon in encountering the same disease with the very same weapons, what horror and intolerance at one time of the very opinions and practices which, previously and subsequently, are cherished and admired!

> "Quod fuit in pretio, fit nullo denique honore;
> Porro aliud succedit, et e contemptibus exit,
> Inque dies magis appetitur, floretque repertum
> Laudibus, et miro 'st mortaleis inter honore."

To be satisfied on this point we need only refer to the history of any one or two of our principal diseases or principal remedies, as, for instance, fever, pneumonia, syphilis; antimony, bloodletting, mercury. Each of these remedies has been, at different times, regarded as almost specific in the cure of the first two diseases; while, at other times, they have been rejected as useless or injurious. What seemed once so unquestionably, so demonstrably true, as that venesection was indispensable for the cure of pneumonia? And what is the conclusion now deducible from the facts already noticed in the present article, (p. 246,) and from the clinical re-

or nearly as well, without it?—Could it have been believed possible by the practitioners of a century since, that syphilis could be safely treated and successfully cured without mercury? or that it could ever be questioned that mercury was not specific in the cure of this disease? And yet what are the opinions and the practices of the surgeons of the present day, and the indubitable facts brought to light during the last thirty years? Are they not, that mercury is not necessary (speaking generally) to the cure of any case, and that it is often most injurious, in place of being beneficial? The medical god, Mercury, however, seems as unwilling to be baulked of his dues, as the mythological; if he has lost the domain of syphilis, he has gained that of inflammation; and many of our best practitioners might possibly be startled and shocked at the supposition, that their successors should renounce allegiance to him in the latter domain, as they themselves had done in the former. And yet such a result is more than probable, seeing that there exists not a shadow of more positive proof (if so much) of the efficacy of the medicine in the latter than in the former case.

The same truth, as to the uncertainty of practical medicine generally, and the utter insufficiency of the ordinary evidence to establish the efficacy of many of our remedies, as was stated above, has been almost always attained to by philosophical physicians of experience in the course of long practice, and has resulted, in general, in a mild, tentative, or expectant mode of practice in their old age, whatever may have been the vigorous or heroic doings of their youth. Who among us, in fact, of any considerable experience, and who has thought somewhat as well as prescribed, but is ready to admit that,—in a large proportion of the cases he treats, whether his practice, in individual instances, be directed by precept and example, by theory, by observation, by experience, by habit, by accident, or by whatsoever principle of action,—he has no positive proof, or rather no proof whatever, often indeed very little probability, that the remedies administered by him exert any beneficial influence over the disease? We often may hope, and frequently believe, and sometimes feel confident, that we do good, even in this class of cases; but the honest, philosophical thinker, the experienced, scientific observer, will hesitate, even in the best cases, ere he commit himself by the positive assertion, that the good done has been done by him. When physicians of this stamp have met in consultation in any doubtful case, and when they have chanced to be startled out of their conventionalities by the bold doubt, or bolder query, of some frank brother of the craft, has not the confession, like the confidence, been mutual?

> "And when his comrade's thought each *doctor* knew,
> 'Twas but his own, suppress'd till now, he found."

From these our free confessions and bold denunciations of the feebleness and uncertainty of therapeutics, it may possibly be inferred, that we are entirely sceptical of the truth of medicine as a science, and think most

* See Louis' Recherches sur les Effets de la Saignée, Paris 1835; or the Review of the work in this Journal, Vol. I, p. 97.

meanly of it as a practical art. And yet this is not so. On the contrary, we look upon medicine, regarded in all its parts and all its bearings, as a noble and glorious profession, even in its present most imperfect state; and we believe it destined to become as truly grand and glorious in actual performance, as it now is in its essence, its aims, and its aspirations.

It is an unquestionable truth that medicine, both as a science and as an art, is, on the whole, progressive; and its progress, compared with that of preceding times, has been immense during the last sixty years. In the fundamental parts of the science, in physiology, pathology, and diagnosis, great and manifold additions have been made to our knowledge, during this period; several positive improvements have been also introduced into the general mode of treating our patients; and we have acquired one or two unequivocal accessions to our stock of certain means of relieving or curing diseases. We believe we may also add, with truth, that the general style of practice in this country has become better, is less guided by theory and tradition, is more discriminating, less confident and bold, less perturbing and meddling. We have learned a good deal at home, and still more, perhaps, from home. The long peace and the general intercourse of nations consequent thereon, have permitted every country to know what every other country possesses. British medicine has thus profited considerably, and most especially by the importation of some of the humbler notions and milder practices of our continental neighbours. In the treatment of acute diseases, we have attained somewhat nearer to the heroic virtue of patience, from an increased knowledge of the morbid processes going on; in chronic disorders we have become more regimenal and less druggish; in all cases, perhaps, we have grown a little more trustful of nature, and a little less trustful of art. Nevertheless, it cannot be denied that the more ordinary proceedings of a large proportion of the practitioners in this country differ from those of their predecessors, much more in their nature than in their effects; and that they are, to a lamentable extent, palpably and egregiously wrong. We doubt, therefore, if we should greatly, if at all, exceed the bounds of truth, if we said, that the progress of Therapeutics, during all the centuries that have elapsed since the days of Hippocrates, has been less than that achieved in the elementary sciences of medicine, during the last fifty years. This department of medicine must, indeed, be regarded as yet in its merest infancy. It would, doubtless, be going far beyond the truth to assert, that there is no certainty in medicinal therapeutics, and that the whole practice of medicine, in as far as this consists in the administration of drugs, is a system of traditionary routine and conventionalism, hap-hazard, and guess-work; but it is not going beyond the truth to assert, that *much* of it is so. In the hands of men of scientific education, men of philosophical views and long experience, and who, from the position they occupy and the confidence they inspire, are enabled to proceed exactly as they think best, Practical Medicine, we readily admit, is, even now, a rational and wise system, rarely productive of evil if it fails to benefit, and often benefiting in the highest degree. But in the hands of those who are differently circumstanced in every respect; who either travel contentedly in the broad highways of tradition, or deviate into still more dangerous paths which they deem rational; who, confounding

cording to fancy,—medicine is a very different art, and its practice productive of very different results.

The foregoing elucidations, it will not be doubted, disclose a lamentable state of things; but it is not a state to be despaired of; much less is it one to be concealed as something disgraceful. It is more our misfortune than our fault that it is as it is; but if it were our fault, still it ought to be made known. Here, as in morals, the more sensibly we feel our defects, the more openly and heartily we confess them, the more likely are we to get rid of them. As thus reflected in our critical mirror, the features of our Ancient Mother assuredly look somewhat unattractive. She seems neither happy nor prosperous; yea, she seems sick, very sick; yet not sick unto death. On the contrary, we believe that she is more vivacious and vigorous than at any preceding time; her countenance is merely "sicklied o'er by the pale cast of thought," from the strength of her inward throes; "the genius and the mortal instruments are now in council, and her state, like to a little kingdom, is suffering the nature of an insurrection." And such, in truth do we believe to be, literally, the condition of physic at this moment. Things have arrived at such a pitch, that they cannot be worse. They must mend or end. We believe they will mend. The springs of life are yet untouched; the constitution retains its rallying power; the vis medicatrix is in action; and we flatter ourselves that there is yet enough of young blood and energy and wisdom in our ranks, to redeem the past, and to achieve that glorious REGENERATION, which has been long announced by infallible signs and portents in these later days. Old as we are, we yet hope to see raised the standard of "YOUNG PHYSIC," though we cannot expect to see it furled, after the destined victory is won.

The course of our subject would now lead us to attempt to show wherein the defects and evils of the present system of practical medicine mainly consist; the causes of these; and the means that seem best calculated to remove them;—with a view to the substituting a better system in its place. But this must be reserved for another occasion, if not for another hand. Our space, for the present is overpast; our time expended; and the reader's patience, we suspect, already sufficiently tried. Had we proceeded in our task, it was our design to show, that the changes in therapeutics contemplated by us had nothing to do with any dogma, or system, or crotchet of our own; but were merely such alterations, or if the word be allowed, such reforms, in the existing state of things, as seemed necessarily to flow from a consideration of the more obvious deficiencies heretofore exposed. Such as it is, our project of reform, is, at the least, as conservative as radical. We are not altogether discontented with the general principles on which medicine, considered as a science, is now studied by many men of education; we do not object greatly to the mode in which we know it to be exercised as an art by many of its professors; but we cannot shut our eyes to the enormous mass of its defects, or to the grievous evils which result, both to the community and the profession, from the way in which it is carried out in detail, by a large proportion of existing practitioners. It is with a view to the removal of these defects and evils—as far as they are removable—that we would fain excite

the interest and claim the attention of our readers, and, through them, of the profession generally.

It would be presumptuous in us, in the present stage of the question, to attempt to give even a formal Outline or Sketch of the Reform in Practical Therapeutics which appears so necessary, and which we believe to be impending. This is a work which can only be the result of mature reflection, and of the labour of many years and many hands. All which we can think of attempting at present, is to set down, almost at random, a few of the various considerations that press upon us, touching the many things to be thought of and done, the manifold evils to be abated, the manifold benefits to be achieved, by the enthusiastic and active spirits whom we have heretofore sportively personified under the name of "YOUNG PHYSIC," and to whom we look with confidence for the consummation of the great REFORMATION which assuredly will come.

In submitting these suggestions to criticism, we would request, that their extemporaneous and undigested character be borne in mind. All of them, we believe, to be true and just; many of them of high importance; and although more mature reflection may prove some of them, at least, to be neither the one nor the other, we shall, nevertheless, not regret having written them. They may excite others to consider this momentous subject, and thus elicit from better minds, thoughts worthier of remembrance and fruitful of greater things.

COGITANDA—EXCOGITANDA—AGENDA.

1. To endeavour to ascertain, much more precisely than has been done hitherto, the natural course and event of diseases, when uninterrupted by artificial interference; in other words, to attempt to establish a true Natural History of human diseases.

2. To reconsider and study afresh the physiological and curative effects of all our therapeutic agents, with a view to obtain more positive results than we now possess.

3. To endeavour to establish, as far as is practicable, what diseases are curable and what are not; what are capable of receiving benefit from medical treatment and what are not; what treatment is the best, the safest, the most agreeable; when it is proper to administer medicine, and when to refrain from administering it; &c. &c.

4. To endeavour to introduce a more philosophical and accurate view of the relations of remedies to the animal economy and to diseases, so as to dissociate in the minds of practitioners the notions of *post hoc* and *propter hoc*.

The general adoption by practitioners in recording their experience, of the system known by the name of the *Numerical Method*, is essential to the attainment of the ends proposed in the preceding paragraphs, as well as in many that are to follow.

5. To endeavour to banish from the treatment of acute and dangerous diseases, at least, the ancient axiom, *melius anceps remedium quam nullum*, and to substitute in its place the safer and wiser dogma—that where we are not certain of an indication, we should give nature the best chance of doing the work herself, by leaving her operations undisturbed by those of art.

6. To endeavour to substitute for the monstrous system of Polyphar-

macy now universally prevalent, one that is, at least, vastly more simple, more intelligible, more agreeable, and, it may be hoped, one more rational, more scientific, more certain, and more beneficial.

7. To direct redoubled attention to hygiene, public and private, with the view of preventing diseases on the large scale, and individually in our sphere of practice. Here the surest and most glorious triumphs of medical science are achieving and to be achieved.

8. To inculcate generally a milder and less energetic mode of practice, both in acute and chronic diseases; to encourage the Expectant preferably to the Heroic system,—at least where the indications of treatment are not manifest.

9. To discountenance all active and powerful medication in the acute exanthemata and fevers of specific type, as small pox, measles, scarlatina, typhus, &c., until we obtain some evidence that the course of these diseases can be beneficially modified by remedies.

10. To discountenance, as much as possible, and eschew the habitual use (without any sufficient reason) of certain powerful medicines in large doses, in a multitude of different diseases, a practice now generally prevalent and fraught with the most baneful consequences.

This is one of the besetting sins of English practice, and originates partly in false theory, and partly in the desire to see manifest and strong effects resulting from the action of medicines. Mercury, iodine, colchicum, antimony, also purgatives in general and bloodletting, are frightfully misused in this manner.

11. To encourage the administration of simple, feeble, or altogether powerless, non-perturbing medicines, in all cases in which drugs are prescribed *pro forma*, for the satisfaction of the patient's mind, and not with the view of producing any direct remedial effect.

One would hardly think such a caution necessary, were it not that every-day observation proves it to be so. The system of giving and also of *taking* drugs capable of producing some obvious effect,—on the sensations, at least, if not on the functions,—has become so inveterate in this country, that even our *placebos* have, in the hands of our modern doctors, lost their original quality of harmlessness, and often please their very patients more by being made unpleasant!

12. To make every effort not merely to destroy the prevalent system of giving a vast quantity and variety of unnecessary and useless drugs, (to say the least of them,) but to encourage extreme simplicity in the prescription of medicines that seem to be requisite.

Our system is here greatly and radically wrong. Our officinal formulæ are already most absurdly and mischievously complex, and our fashion is to double and redouble the existing complexities. This system is a most serious impediment in the way of ascertaining the precise and peculiar powers (if any) of the individual drugs, and thus interferes, in the most important manner, with the progress of therapeutics.

We are aware of the arguments that are adduced in defence of medicinal combinations. We do not deny that some of these combinations are beneficial, and therefore proper; but there cannot be a question as to the enormous evils, speaking generally, resulting from them. Nothing has a greater tendency to dissociate practical medicine from science, and to

stamp it as *a trade*, than this system of pharmaceutical artifice. It takes some years of the student's life to learn the very things which are to block up his path to future knowledge. A very elegant prescriber is seldom a good physician. And no wonder. Tailors, barbers, and dancing-masters, however learned they may be in the externals of gentility, are not expected to be fine gentlemen or men of fashion.

13. To endeavour to break through the routine habit, universally prevalent, of prescribing certain determinate remedies for certain determinate diseases or symptoms of diseases, merely because the prescriber has been taught to do so, and on no better grounds than conventional tradition.

Even when the medicines so prescribed are innocuous, the routine proceeding impedes real knowledge by satisfying the mind, and thus producing inaction. When the drugs are potent, the crime of mischief-making is superadded to the folly of empiricism. In illustration, we need merely notice the usual reference, in this country, of almost all chronic diseases accompanied with derangement of the intestinal functions, to "affection of the liver," and the consequent prescription of *mercury* in some of its forms. We do not hesitate to say, that this theory is as far wrong as the practice founded on it is injurious; we can hardly further enhance the amount of its divarication from the truth.

14. To place in a more prominent point of view the great value and importance of what may be termed the physiological, hygienic, or natural system of curing diseases, especially chronic diseases, in contradistinction to the pharmaceutical or empirical drug-plan generally prevalent. This system, founded as it is on a more comprehensive inquiry into *all* the remote and exciting causes of disease, and on a more thorough appreciation of *all* the discoverable disorders existing in all the organs and functions of the body, does not, of course, exclude the use of drugs, but regards them (generally speaking) as subservient to hygienic, regimenal, and external means,—such as the rigid regulation of the diet, the temperature and purity of the air, clothing, the mental and bodily exercise, &c., baths, friction, change of air, travelling, change of occupation, &c. &c.

15. To endeavour to introduce a more comprehensive and philosophical system of Nosology, at least in chronic diseases, whereby the practitioner may be led less to consider the name of a disease, or some one symptom or some one local affection in a disease, than the disease itself,—that is, *the whole* of the derangements existing in the body, and which it is his object to remove, if possible.

16. To teach teachers to teach the rising generation of medical men, that it is infinitely more *practical* to be master of the elements of medical science, and to know diseases thoroughly, than to know by rote a farrago of receipts, or to be aware that certain doctors, of old or of recent times, have said that certain medicines are good for certain diseases.

17. Also to teach students that no systematic or theoretical classification of diseases, or of therapeutic agents ever yet promulgated, is true, or anything like the truth, and that none can be adopted as a safe guide in practice. It is, however, well that these systems should be known; as most of them involve some pathological truths, and have left some practical good behind them.

18. To endeavour to enlighten the public as to the actual powers of

medicines, with a view to reconciling them to simpler and milder plans of treatment. To teach them the great importance of having their diseases treated in their earliest stages, in order to obtain a speedy and efficient cure; and, by some modification in the relations between the patient and practitioner, to encourage and facilitate this early application for relief.

19. To endeavour to abolish the system of medical practitioners being paid by the amount of medicine sent in to their patients; and even the practice of keeping and preparing medicines in their own houses.

Were a proper system introduced for securing a good education to chemists and druggists, and for examining and licensing them—all of easy adoption—there could be no necessity for continuing even the latter practice; while the former is one so degrading to the medical character, and so frightfully injurious to medicine in a thousand ways, that it ought to be abolished forthwith, utterly and for ever.

20. Lastly, and above all, to bring up the medical mind to the standard necessary for studying, comprehending, appreciating, and exercising the most complex and difficult of the arts that are based on a scientific foundation,—the art of Practical Medicine. And this can only be done by elevating, in a tenfold degree, the preliminary and fundamental education of the Medical Practitioner.

Such are a few of the labours in store for our young Hercules of Physic; a few samples of the varied contents of the stable he is called upon to cleanse; and a few pailfuls, it may be, of the veritable Alpheian he is to work withal:

> "———— Mox in ovilia
> Demisit hostem vividus impetus;
> Nunc in reluctantes dracones
> Egit amor————."

ART. XXII.

An experimental Inquiry into the Pathology and Treatment of Asphyxia. By JOHN E. ERICHSEN.—*Edinburgh*, 1845. pp. 56.

THE author of the present treatise was appointed by the general committee of the British Association for the Advancement of Science, at the meeting held at Manchester in the year 1842, to form, in conjunction with Dr. Sharpey, a commission to make an experimental inquiry into the subject of asphyxia. Professor Sharpey shortly afterwards retired from the commission, and our author continued to prosecute his inquiry alone. His report, which was read at the fourteenth meeting of the British Association, and for which the Royal Humane Society awarded the Fothergillian gold medal, is the work before us.

Mr. Erichsen first considers his subject with reference to its pathology, and then enters into the question of the treatment of asphyxia.

In the first part of the subject, the pathology of asphyxia, Mr. Erichsen examines experimentally the merits of the three leading opinions each of which has, in turn, been received as the fundamental *fact* of asphyxia:

"1st, That the circulation ceases in consequence of the arrest of the respiratory movements; 2d, in consequence of want of power in the heart; 3d, in consequence of an obstruction in the passage of blood through the lungs" (p. 2.)

Each of these opinions has received the investigation of careful, and, in our opinion, in the main, conclusive experiments; though, as we have often had occasion to express ourselves before, so great do we consider the violence done to the system in these experiments on living animals, sufficient of themselves to cause death, that we cannot accept all their results without some reservation.

With regard to the first and second of the above views of the leading cause of the phenomena of asphyxia, as the conclusions of Mr. Erichsen are in unison with those of Dr. Kay, now, we believe, so generally received,—and for a concise and clear expression of which we may refer to a book in every one's hands, the excellent lectures of Dr. Watson (vol. i, p. 45)—we do not deem it needful to follow him through his reasonings and experiments, but shall satisfy ourselves with expressing his conclusions, only adding on our own part, that we consider them borne out by his experiments. We give them in his own words:

"1st. That although the persistence of the respiratory movements has some influence in maintaining the circulation through the lungs, yet that their arrest is not by any means the sole cause of the cessation of the circulation.

"2d. That a diminution in the force and frequency of the contractions of the heart, consequent upon the altered quality and lessened quantity of the blood circulating through its muscular substance, is one of the principal causes of the cessation of the circulation in asphyxia, as is evident from the fact, that, when the force of the heart's contractions is maintained by a supply of arterial blood to its muscular substance, it is enabled to propel black blood through a collapsed lung." (p. 31.)

It is, however, in the investigation of the third opinion, the obstruction of the circulation through the lungs, that Mr. Erichsen finds the main cause of the arrest of the circulation. As on this point, however, we find him somewhat at issue with Dr. Kay, whose opinions on this, as on the other heads, have, we believe, received general assent, we here propose to follow him, in some measure, into the details of his reasoning and experiments. While Dr. Kay holds "that the main cause," we quote the clear and concise account of Dr. Watson,* "of the failure of the circulation seems to be the difficulty with which the non-arterialized blood finds its way though the capillaries of the lungs," Mr. Erichsen, on the other hand, is of opinion that the obstruction is in the passage through—not the capillaries but—the smaller ramifications of the pulmonary veins, believing "that the venous blood which circulates in asphyxia may act as a stimulus to the smaller branches of the arterial system, [by analogy, those of the pulmonary *veins*,] exciting the contractility of these vessels, and that thus the obstruction in the circulation may be occasioned." (p. 26.) Mr. Erichsen dismisses the claim of the action of the capillaries in producing this result, by arguing, as "stated by Dr. Alison, that the stagnation cannot arise from the stimulus of the venous blood, being insufficient to excite the capillaries, as it has not been proved that these vessels are irritable, nor that they possess coats." (p. 26.) But, to proceed from the negative

* Lectures on the Practice of Medicine, vol. i, p. 45.

sides, that every part of the body [he has made an exception in the previous page in favour (?) of the capillaries] is provided with a special sensibility, 'which determines,' to use the words of Bichat, 'the nature of its relations with those foreign bodies which may happen to be in contact with it." (p. 27.) Mr. Erichsen then cites the peculiar physiological relations and actions, towards foreign bodies, of the salivary, bilious, and other ducts, and, reasoning from them towards the relations between venous blood and arterial branches, he remarks, "on reasoning from analogy, therefore, we should be disposed to look upon venous blood as a stimulant to the arterial system, that is, as having a tendency to excite the contractility of these vessels."

"But," continues Mr Erichsen, "we may go a step farther, and prove that it actually possesses this power; causing these vessels to contract distinctly, as I have several times observed, on examining under the microscope, the mesentery of rabbits during and immediately after the process of asphyxia. . . . On asphyxiating a young rabbit, a portion of whose mesentery has been conveniently fixed under a powerful microscope, the following phenomena will be observed to ensue. For about a minute after the struggles of the animal have ceased the circulation appears to be going on with its usual rapidity; it then gradually becomes somewhat slower, the arteries contracting in size, containing less blood, and assuming a lighter and more tawny colour than before; whilst the veins become congested, and evidently fuller, assuming, when viewed by transmitted light, a very beautiful crimson hue As the circulation becomes more languid, the arteries continue contracting, and acquire a lighter colour; the diminution in their size and the difference in the quantity of blood contained in them and in the veins, being most marked. The motion of the blood in the capillaries now becomes oscillatory, the whole mass of the blood being at each impulse of the heart slowly propelled forwards, and then moving backwards. . . . Nor have I ever been able to discover any obstruction in the vessels, in consequence of the adhesion of colourless globules to their sides,—a phenomenon that I especially watched for, and which has by several been supposed to occur. The diminution in the diameter of the smaller arteries, and the proportionate difference between these and the neighbouring veins, was most evident, and was such as could leave no doubt on my mind as to the important part that the contractions of these vessels play, in giving rise to an obstruction to the passage of the blood through them in asphyxia; *in which I have no doubt it is the principal if not the sole agent*." (pp. 28-9.) [The italics are our own.]

To apply these general remarks to the circulation through the lungs, Mr. Erichsen observes:

"The blood having now become perfectly venous, begins to be obstructed in its passage through the ultimate ramifications of the pulmonary veins, and of the arterial system generally, occasioning congestions of the pulmonary artery, of the right cavities of the heart, and of the whole of the venous system; and the quantity sent to the left cavity of the heart becomes materially lessened." (p 30.)

Having thus given our author's opinions and reasonings on this head, we quote his two remaining conclusions, viz.:

"3d. That the obstruction which has been proved to take place in the pulmonary and systemic circulation is due to the venous blood exciting the contractility of the minute divisions of the arteries and pulmonary veins, by acting upon their special sensibility.

"4th That the cause of the stoppage of the circulation in asphyxia is therefore threefold: depending, first, upon the arrest of the pulmonary movements; second, upon the weakening of the heart's action; and third, upon the obstruction offered to the blood (propelled with diminished force) by the refusal of the pulmonary veins and minute arteries to receive venous blood." (pp. 31-2.)

Before quitting this part of our subject we feel tempted to transcribe a table of the average duration and succession of the various phenomena of asphyxia, as noticed by Mr. Erichsen in his numerous experiments. The animals experimented on were dogs.

	Minutes.
"Voluntary movements cease in	1¼
Involuntary movements in	2½
Blood in arteries becomes as black as that in the veins in .	1¾
Occasionally as early as	1¼
Contractions of the ventricles cease in	9½
The earliest that I have observed has been in . .	6½
The latest in adult animals in	14
Twitching and irregular movements continue some time longer.	
The left auricle on an average in . . .	12
I have seen it in an adult continue to contract till the .	37th
The right auricle on an average in . . .	19½
I have seen it in an adult animal continue to contract till the	44th
Pulsations continue in the femoral artery on an average for	7½
In very young animals the time that the contractility of different parts of the muscular system is maintained is very different.	
In puppies four days old movements continued for . .	16
Ventricles continued acting regularly for . . 1 hour	57
But twitchings and irregular movements continued for 3 hours	4
Auricles continued acting for . . . . . 3 hours	25." (p.31.)

Under the head of treatment, Mr. Erichsen submits some interesting considerations. Can the circulation be restored through the lungs after it has become stagnated in that organ? Can the action of the heart be restored after it has ceased pulsating? (p. 41.) Each of these considerations demands some notice, which we shall make as concise as possible.

First, with respect to the renewal of the circulation through the lungs, after it had actually ceased in those parts. Mr. Erichsen gives us the detail of an experiment,—and he informs us he repeated it many times with the same result,—in which, though artificial respiration was not applied till 11½ minutes after the auricles had finally ceased from their irregular movements, and the left side of the heart was emptied, and as much as 37½ minutes after the ventricles had ceased to contract, yet that after commencing artificial respiration, the blood in the pulmonary veins became arterial, and blood returned to the left side of the heart, insomuch that after four minutes the auricles became of a more florid hue; and at the 27½th minute of artificial respiration, he informs us, "on puncturing the left auricle at the point of junction with the pulmonary veins, a very large quantity of perfectly bright arterial blood issued in a small jet a line or two in height. The insufflation being continued, perfectly florid blood

continued to well out each time the lung was distended." (p. 43.) Still, however, there was no renewal of the systemic circulation. We think with our author he has made out his case, that his experiments clearly prove "the possibility of re-establishment of the circulation through the lungs after the heart's action has entirely ceased." (p. 43.)

We now come to consider the second question proposed, viz., the possibility of restoring the heart's action after *it* had ceased; but before expressing our conclusion on this point, we must discuss as preliminary to it, a question on which our author appears to have come to an erroneous conclusion; it is this,—how long after submersion does the heart continue to pulsate? This may vary; syncope may be induced from mental terror, and the circulation, and the consequent changes of the blood being thus considerably retarded, the action of the heart, though extremely feeble, as of necessity it must be in this condition, may not yet be entirely arrested for a much longer period than in other cases, when the violent struggles not only force out the air contained in the lungs, but by quickening the circulation, much sooner reduce the blood to the venous character, and thus secondarily must hasten the period at which the heart ceases to act: the quantity of air which happened to be in the lungs at the time, may also vary; but making allowance for these variations, our author is disposed to consider five minutes as the average period after submersion at which the heart ceases to beat. We confess we think this estimate rather low, when we consider that with all the violence of the operation of exposing the heart, and the unfavorable circumstances for the continuation of organic life, that yet in the dogs he has experimented upon, Mr. Erichsen gives 9½ minutes as the average period at which ventricular action ceases: nor do we think that by any means Mr. Erichsen's facts warrant his conclusions. The facts are mostly on the authority of Mr. Woolley, who has had ample opportunity of making correct observations on this subject; they are these, — that, as a general rule, no person recovers who has been much more than four minutes submerged, and that the vast majority recover who can be brought from the water under that period; and hence Mr. Erichsen concludes, "I should suppose that contractions of the ventricles have most generally ceased before 5 minutes have elapsed after complete submersion." (p. 34.) This conclusion, however, is on the assumption that air is restored to the lungs, and arterial blood to the heart, within *a few seconds* after removal from the water, whereas we hold, the calculation should not be the period of actual submersion, but the average period between the commencement of complete submersion and that at which the first evidences of convulsive respiration have been evinced: in the recovery of *extreme cases*, this period would fall *within* the minimum of the continuance of the pulsation of the left ventricle after asphyxia had commenced; a conclusion we think fully borne out in a quotation which Mr. Erichsen himself brings forward in refutation of a similar error with respect to a similar calculation in Dr. Roesler's experiments on rabbits.*

"'It may be said .... that an obscure respiration might have been going on' from the time the asphyxiating cause was resumed, 'but every one who is

* Edin. Med. and Surg. Journal, vol. xxiii.

materially familiar with the phenomena of suspended animation, must know that this was impossible. After an animal has been asphyxiated so long that its breathing has ceased, the first efforts to renew respiration are never obscure; but they are convulsive gasps attended with spasm of the whole body; such as no one could possibly fail to notice, who was occupied with preparations to resuscitate it.'" (pp. 44-5.) *

So far from the phenomena of asphyxia beginning to cease the instant the body is recovered from the water, the symptoms may be, and probably are yet increasing for some time, while nevertheless means are successfully proceeding towards recovery; for until air actually reach the lungs, and arterialized blood return to the left side of the heart, all the preliminaries for that end cannot prevent the pulsations becoming more and more distinct.

From the conclusion then of our author, that in the human subject the heart's action ceases within 5 minutes after submersion, we certainly must dissent; but we do not despise the facts and observations of Mr. Woolley on which it is founded. They are worth just their average result and no more.

Having considered this question, we may now fairly return to that from which we started, viz., the heart having ceased to pulsate, can its action be restored?

Our author details many experiments on this head, the results of which are briefly as follows: that although artificial respiration with common air will restore the circulation through the lungs, and cause florid blood to return to the heart, yet that it will not restore the pulsations of the ventricles, these having once ceased; but that although the inflation of common air be insufficient for that purpose, yet when oxygen was substituted, the result was perfectly successful in many cases—only however in those in which its use was adopted very quickly afterwards.

"There can be little doubt," observes Mr. Erichsen, "but that the revival of the contractions of the heart in these cases was due to the very rapid arterialization of the blood in the lungs, and its passage into the left side and tissue of the heart, before the irritability of that organ had become so far extinct that it was unable to contract on the application of its usual stimulus; and that the reason why, by the inflation of the lungs with common air, it is very difficult, if not impossible, to excite perfect contractions of the ventricles, although tremulous movements in them, and proper action of the auricles may be set up, is, that blood does not become arterialized soon enough to reach the left ventricle, and to permeate the tissue of the heart in sufficient quantity before that organ has lost its power of contracting on the application of this stimulus." (p. 52.)

From the results of his experiments we need hardly remark that Mr. Erichsen places artificial respiration with oxygen gas as a most important

* An instance of the convulsive movement attendant on returning respiration we had recently an opportunity of observing so low in the animal kingdom as the class *insecta*. We were rearing a larva of the puss moth, *ceruria viscuila*, which we had left feeding on a twig of poplar in a glass of water. On entering our study we found our caterpillar drowned in the glass. We wiped the water from the spiracles on both sides, and held him on the warm hand. After a few minutes a convulsive movement took place in the posterior articulation, with violent throbbing of the corresponding portion of the dorsal artery, very different from the tranquil pulsation which usually exists. This kind of action continued to take place upwards in each consecutive articulation, and our pet was soon restored to his usual functions.

not observe any peculiarity worthy of notice, except the advantage of using it warm—for instance, about 98°. With respect to the other means noticed by Mr. Erichsen, we observe nothing different from those recommended in the several excellent articles which our now numerous cyclopædias contain, except that with regard to the warm bath, he lays great stress on its temperature, being in some proportion regulated by the heat of the water in which the patient had been immersed. He observes that while 98° or 100°—the degree recommended by the Royal Humane Society—may be proper in the summer months when the temperature is between 50° and 60°, it is far too high in the winter when it may be little above 32°. "In this case, I should certainly," observes our author, "recommend that the patient be not at first exposed to a greater degree of heat than from 85 to 90 degrees."

We cannot take leave of Mr. Erichsen without testifying the great pleasure we have experienced in the perusal of his Treatise. We think he has done real service towards our knowledge of the subject he has taken in hand, and we should strongly recommend the perusal of his Treatise, as well to those interested in the physiology and pathology of organic life, as to those who may be so circumstanced as to be likely to have the apparently drowned submitted to their care.

Art. XXIII.

Lectures on the Theory and Practice of Surgery. By the late Abraham Colles, M.D., &c. Edited by Simon M'Coy, Esq., F.R.C.S.I.—*Dublin and London,* 1845. 2 vols. 8vo, pp. 396-360.

These Lectures, by the late respected Professor of Surgery in the Royal College of Surgeons in Ireland, have been reprinted from the 'Dublin Medical Press,' and put into their present more commodious form by Mr. M'Coy, who had the advantage of a personal attendance during their delivery. They have "been compiled from notes of several courses, and carefully collated with the manuscripts of others of Mr. Colles's pupils." They are indeed "eminently practical," and we doubt not must have had great influence in their time. There is no effort at what might be called scientific arrangement, and this we take as proof of Mr. Colles's practical character, for such an attempt would only have involved theoretical disquisition—a thing which seems to have been most carefully avoided by the teacher. The topic of each lecture is clearly stated in simple and unaffected language; the illustrative cases, anecdotes, and occasional witticisms, are all much to the point; and, even without the precious impress of Mr. Colles's name, any practical man looking over these pages would at once perceive that he was reading the doctrines of a master in the art.

We imagine, however, that the chief value of these lectures is derived from the circumstance that they were those of Abraham Colles. We doubt not that they will be received with pleasure by many in Ireland, who cherish with respect and gratitude the memory of one who guided them so judiciously in the early part of their career. Further than this we do

not suppose that they will be accepted; and though we can most conscientiously recommend them as being in every way worthy of consideration as far as they go, we feel equally bound as impartial critics to state, that they are far behind the surgery of the present day. While doctrines, many of which may in a manner be deemed obsolete, are thus published, there is not a word about the modern improvements in surgery. There is not a sentence from Mr. Colles about lithotrity, for example; nor of the hundred little matters of novelty introduced within the last twenty years. Mr. M'Coy, to be sure, gives a foot-note about lithotrity, whereby it might appear, to a person not acquainted with the state of surgery, that this proceeding had actually been alluded to, even in a manner exhibited, in a lecture-room in Dublin. We are gravely told by Mr. M'Coy that "at this part of the lecture, and before the operation (lithotomy) was demonstrated in the dead body, by Mr. Colles, an Italian surgeon, named, I think, Pacheoli, was introduced to the class, one session, to explain the mechanism and *modus operandi* of an instrument he contrived for lithotrity;"— and so the note proceeds, in a like innocent tone, to the end, without one word of what has been done since. Surely Mr. Colles must have taken more notice of such a subject in subsequent courses of his lectures! If so, as "the present publication has been compiled from notes of several courses," it is remarkable that we have no further account of it.

Both teacher and annotator occasionally display a degree of simplicity which makes us smile. Thus we are told in the lecture on hydrophobia, (p. 89,) that Mr. Colles has "been informed that spring is the time dogs are most subject to madness in Scotland, and it is accounted for in this way. A number of cattle are annually left to perish during the severity of the winter, and it is supposed the dogs over-eat themselves, which causes a tendency to the disease," and the worthy professor sagely concludes, "but whether this be correct or not, I cannot say." There was no Rowland Hill in those days, yet "we have been informed" that there was a post between Ireland and Scotland, and imagine that a letter would—after a week or two—have put an end to all doubt in the mind of the worthy teacher.

The omission of many characteristics of modern surgery, however, cannot be deemed a fault in these lectures—for such things were not known when Mr. Colles taught. "The medical practitioners of Ireland of the present day," to whom the preface of the publisher is specially addressed, must not, therefore, expect from these volumes an exposition of the present state of surgery; they must take them, as we have already said, only as Mr. Colles's lectures.

There are only forty-nine lectures in all, and it would therefore be unreasonable to expect that Mr. Colles should have discussed every subject of surgery. We cannot but express our astonishment, however, that out of this small number ten of them should be devoted to the venereal disease. But the subject has seemingly always been a favorite one with the Dublin surgeons.

PART SECOND

Bibliographical Notices.

Art. I.—*Observations on the Nature and Treatment of the more important Diseases of the Nervous System.* By a Physician.—*Bath,* 1845. 12mo, pp. 90.

This pamphlet, termed by its author a "Volume," contains a number of remarkable cases of diseases of the nervous system. A short preface is signed "Edward Blackmore, M.D." and dated "Bath, January, 1845." We are at a loss to know why the name of the author was not placed in the title-page, as is usual with authors who court publicity.

There is nothing in this little pamphlet which, in our opinion, merits lengthened criticism. The pathology of the diseases treated by its author as stated by him, appears to us extremely hypothetical. Of apoplexy, he observes, "the immediate cause of a large majority of the cases is a rush of blood to the brain, occasioned by the vehement action of its arteries, and it is probable that this spasmodic action of the vessels continues until, from the failure of the respiratory functions, black blood is circulating in the head; and indeed, even the condition of venous blood being in the arteries of the brain, does not instantly put a stop to the excessive vascular action." (p. 7.) He further observes, "that it is no disproof of this view of the proximate cause of the fit, that sometimes from the moment of its invasion, the pulsation at the temples is low and the face cold; for the capillary branches of the internal carotids are unquestionably sometimes in high action when the branches of the external carotids are in an opposite condition. This view is easily understood upon hydraulic principles, and is confirmed by the fact that, in lessening the determination of blood to the brain, the circulation in the arteries of the head is seen, in these cases, to become excited!" Venous congestion is not, according to our author's views, "the immediate cause of the apoplectic state. . . . . The suspension of the functions of the brain appears to be referrible to the *pressure* of the accumulated blood; and partly, also, to *the influence of undecarbonized blood.*" (p. 8.) We have always understood venous blood to be undecarbonized blood; but it appears from the preceding extract, that Dr. Blackmore thinks differently, unless he contradicts himself,—the more probable conclusion.

We observe nothing remarkable in his treatment, except that it appears to be of the most heroic kind, and therefore to be eschewed. Two or three cases of spasmodic affection were treated by a deep crucial incision through the scalp, allowing it to bleed freely. There is nothing, however, in the histories of these cases which would induce the judicious practitioner to adopt the plan as recommended by Dr. Blackmore. He has tried it "several

times with gratifying success." "In spasmodic cases of an encephalic origin" he observes, "as well as in hemiplegia and mania, where there has been either a fixed pain in the head or a tender portion of the scalp, so that gentle percussion has induced excessive pain, or a convulsion, or an hysterical fit, my experience, as far as it has gone, has proved that the bleeding from the free incision is a far more powerful remedy than any other mode of depletion." (p. 52.) Two cases of aggravated hysteria were thus treated!

Our note of admiration reminds us of Dr. Blackmore's notes of admiration. They abound, and in conjunction with numerous words in italics mark a succession of medical miracles. "The 26th, *pain in the region of the spine,* the only complaint. *He was then cupped,* and quickly cured. (p. 47.) "The 19th, after the use of the henbane, the left leg became relaxed, *and she could walk, which she had not done for six weeks!*" (p. 59.) "The henbane and purgatives were continued to the 23d, when she was convalescent!" (Ibid.) "The 12th, great and sudden relief after the remedies!" (p. 61.) "In March she was well!" (Ibid.) "— as if an irresistible influence came over her!" (p. 62.) "And sit for some time at the *chaise percée,* without inducing a fit!" (p. 63.) "The uneasy feeling in the head was always relieved on resuming the *horizontal* posture!" (Ibid.) We could fill a page with such quotations. We assure Dr. Blackmore we note these defects with regret; but they are so glaring and exhibit such bad taste as to be altogether inexcusable in a physician.

ART. II.—*Rapport Annuel sur les Travaux de la Société Médicale du Canton de Genève. Pour l'année* 1843. Par le Docteur MARC D'ESPINE, President de la Société.

Annual Report of the Medical Society of the Canton of Geneva. By Dr. MARC D'ESPINE.

WE select from this report one or two points of interest.

M. Morin read a paper to the society, on certain preparations from sweet and bitter almonds. *Amygdalin,* a proximate principle, consisting of oxygen, hydrogen, carbon, and azote, is found only in the bitter almond, while *emulsion* is found both in the bitter and sweet almond. All mixtures of *amygdalin* and *emulsion* in water give rise to the immediate formation of the essence of bitter almonds and prussic acid. The therapeutic properties of laurel-water depend upon its containing a quantity, unfortunately variable, of prussic acid. Liebig has proposed to substitute for the laurel-water an ounce of almond emulsion, mixed with seventeen grains of amygdalin. M. Morin finds this proportion of amygdalin too strong, and proposes only twelve grains of that substance, and asserts that the mixture will yield five grains and a half of the essential oil of bitter almonds, and five grains and a half of medicinal prussic acid, which is equivalent to two-thirds of a grain of anhydrous prussic acid.

Doctor Mayor adds another to the list of successful cases of Cæsarian operation. The female was forty years of age, and pregnant of her first child. Her delivery was rendered impossible by a fibrous tumour of the uterus, which filled the true pelvis. Both mother and child are now in perfect health.

ART. III.—*Das Kindbettfieber in nosologischer, geschichtlicher und therapeutischer Beziehung.* Von Dr. C. T. LITZMANN.—*Halle*, 1844.

The Nature, History, and Treatment of Childbed Fever. By Dr. LITZMANN. —*Halle*, 1844. 8vo, pp. 346.

PUERPERAL fever prevailed epidemically during the winter of 1840, in the Lying-in Hospital at Halle. It broke out in a cold and peculiarly ungenial season, and at a time when influenza was very prevalent, and continued from the end of December until the return of warmer weather at the beginning of the following April. Fourteen persons were attacked by it, eight of whom died, but only three cases occurred out of the hospital, and in each of them the influence of contagion was plainly traceable. In those cases also the patients died. The peritoneum, ovaries, and fallopian tubes were the chief seats of local mischief, and secondarily, the mucous membrane of the stomach and colon were frequently affected. The disease usually commenced on the evening of the second day after delivery, and an extraordinary rapidity of the pulse which beat 130 or 140 times in the minute, though continuing soft and small, was usually its first symptom. The lochial discharge was unaffected, but the secretion of milk was soon suspended, the abdomen was soft, and the pain experienced then at first dull, deep seated, and excited only by pressure. During the night the patients continued restless and feverish with heaviness of the head and uneasiness of the abdomen, but, towards morning the occurrence of a gentle perspiration relieved all the symptoms. Towards evening, however, there was a fresh exacerbation of the disease, the abdomen became distended, and in a short time extremely painful and the patients grew exceedingly restless and anxious. The constipated state of the bowels was suddenly succeeded by profuse watery diarrhea, attended with tenesmus, and pain in the course of the colon, and retching with vomiting of a bilious fluid. The secretion of milk ceased at so early a period after the commencement of the illness, that the child was never suckled for more than a very short time. In no instance, however, did the milk appear to exert any injurious influence upon them, and all the infants but one, were discharged from the hospital quite well.

Dr. Litzmann had the opportunity of observing this epidemic during its whole course, as he then held the appointment of assistant-physician to the hospital. We should not, however, have noticed his work, had it contained nothing more than an account, how good soever that account might have been, of a comparatively unimportant epidemic. But Dr. Litzmann has brought a truly German industry to bear upon the subject, and has compiled an extremely accurate history of every important epidemic of puerperal fever of which we have any record. This historical sketch occupies nearly two hundred pages of the work, and supplies a want long felt in obstetric literature. In another part of his treatise he mentions a fact of considerable interest in the history of the disease; namely, that peritoneal inflammation formed the basis, of nearly all the epidemics of puerperal fever that prevailed in the eighteenth century. It appears to have for the most part run its course in an uncomplicated

form, and to have been comparatively seldom associated with inflammation of other parts of the sexual apparatus. In the present century, on the contrary, peritoneal inflammation seems to have characterized the epidemics only at their commencement and so long as the disease retained a sthenic character, and to have subsequently given way to other varieties. Closely connected with this is the fact that uterine phlebitis has been gradually increasing in frequency, and has constituted the grand characteristic of many of the fatal epidemics that have prevailed in the hospitals of Paris, London, and Vienna, during the past twenty years. The historical facts adduced by Dr. Litzmann show that this apparent change in the character of puerperal fever, cannot be by any means satisfactorily explained on the supposition that former epidemics were imperfectly observed; but that a real alteration in the *genius epidemicus* must have taken place. It would be an interesting task, but one on which we cannot now pretend to enter, to trace the revolutions which have taken place in the epidemic constitution within the past quarter of a century.

The reasons that have led most eminent recent observers to consider puerperal fever as a blood disease, are ably expounded by Dr. Litzmann who himself advocates that opinion. He does not adduce any new arguments on the subject, but relates the different observations and experiments of others with great precision. The account of the morbid anatomy and symptoms of the disease, the description of its various forms, and, indeed, the whole of the remainder of the work present the same proofs of sound and extensive learning, and great good sense which raise it far above the level of ordinary compilations, while the historical notices that it contains, and which might be vainly sought for elsewhere, will render it an invaluable aid to all teachers of midwifery who are masters of the German language.

ART. IV.—*An Act for the Regulation of the Care and Treatment of Lunatics; with explanatory Notes and Comments.* Edited by FORBES WINSLOW, M.D.—*London,* 1845. 8vo, pp. 174.

THIS little work contains, (1) a history of the legislation on the subject of lunacy; (2) general observations on the recent act; (3) an elaborate and minute analysis of the act itself; and (4) an account of the present condition of lunacy in England and Wales, based upon the recent reports of the commissioners in lunacy. The appendix contains lists of all the public and private asylums for the insane in England and Wales, with the number of inmates in each, and various other matters of interest relating to the general subject. The work is unsusceptible of analysis, and requires no comment. It is indispensable to every one charged with the care of lunatics in any way, and will afford much valuable information to all who take an interest in the subject of lunacy or in lunatics.

London, 1845. 18mo, pp. 112.

It will not be expected that we are going to give any particular account, much less to offer any opinion of the merits of this little book. The subject of it, however, has of late years excited so much interest among the members of the medical profession, that we think it right that they should be made aware of the general nature of its contents. These are the following:

"I. Alexis. (Two Exhibitions.)
"II. Adolphe. (Trials A, B, C, D.)
"III. Lady-Somnambulist. (Trials E, F, G.)
"IV. Fraulein von Gönnern.) (Trials H, I. Remarks.)
"V. George Goble. (First day—four experiments.)
(Second day—three experiments.)
(Third day—two experiments.)
"VI. Miss Martineau's J.
"1. Miss Martineau's statement.
"2. Dr. Forbes's letter to the editor of the Athenæum.
"3. Dr. Brown's letter to Dr. Forbes.
"4. Extract from Mr. Greenhow's statement.
"5. Attested statement of Barbara Cole."

The whole consists of a republication of papers that originally appeared in the 'Medical Gazette' and Athenæum. The subjoined extract from the preface will show the object of the publication.

"How far the anticipations of my friends are correct, as to the more general interest likely to be taken in these Papers,—remains to be seen; but, slight as they are, I am disposed to believe that they may be of some benefit not merely to the public, but to the Mesmerists themselves. If received simply as specimens or illustrations of the sort of things which mesmeric professors daily hold forth to the world, and which the world receives, as marvels of the highest order and as truths admitting of no question, they must surely give rise to reflections that may lead to some beneficial results.

"If the professors, on further consideration of the subject, do not condescend to supply the public with evidence of a more satisfactory kind, the public must cease to be satisfied with the kind of evidence they do supply.

"Every one who has paid attention to the proceedings of professed mesmerists,—even those of the highest class, the members of the medical profession,—must now be thoroughly convinced of the absolute necessity of their changing the plan they have hitherto pursued, if they expect to see mesmerism regarded as a branch of human knowledge deserving the attention of scientific men. So long as they refuse to adopt the rigid system of observation required in the sciences, and repudiate all the ordinary rules of induction and rational inference deemed essential to establish facts in other departments of knowledge, they have no right to quarrel with those who persist in disbelieving—or who, at least, refuse to admit as truths,—things, most marvellous in themselves, which, if true, are, to say the least, in nowise proved to be so, and which, for the most part, have no other evidence in their favour than the bare assertions of ignorant, interested, and, it may be, very unprincipled persons. No one conversant with these proceedings, as hitherto conducted, can deny that few, if any, of the greatest marvels recorded by the mesmerists, and promulgated as unquestionable facts, repose on more sound foundations than, before trial, seemed to support those which the investigations detailed in the following pages proved to be utterly baseless and false. As *all*, then, *may* be untrue, are we not authorised to demand a new course of inquiry, or a new series of evidences, before we are called upon to admit the truth of Clairvoyance and the other transcendental phenomena of mesmerism? Are we not

justified, for the future, in refusing to receive from the mesmerists marvellous statements as truths and facts, unless it is, at the same time, proved to be *impossible* to explain or account for them, on other, ordinary, or what may be called natural principles?

"It is also hoped that the perusal of the exposures contained in this little book, may teach a useful lesson to those numerous unscientific persons, who are accustomed to attend mesmeric exhibitions, public or private, from motives of rational curiosity, or with the commendable object of investigating what seem important truths. Such persons, it is believed, must now feel convinced that no reliance whatever is to be placed on the results presented at such exhibitions, as evidencing the truth and powers of mesmerism. As these results are witnessed by the ordinary visitor, it is quite impossible to discriminate the true from the false. The coarsest jugglery may pass with the honest spectator, seated at a distance from the scene of action, for mysterious and awful truths. If Herr Döbler and Monsieur Phillippe can puzzle and perplex a whole theatre, surely George Goble may bamboozle the erudite captain and the six ladies on Mr. Vernon's back seats!" (Preface, pp. vi-ix.)

Art. VI.—*Notice sur les Variations du Poids des Prisonniers soumis au Régime Pénitentiaire.* Par le Docteur Marc d'Espine, Médecin des Prisons et Membre du Conseil de Santé du Canton de Genève, &c. &c.

On the Variations in the Weight of Prisoners submitted to the Penitentiary System. By Dr. Marc d'Espine, &c. &c. Extracted from 'The Annales d'Hygiène publique et de Médecine légale.' Vol. xxxii, Part I.

We refer our readers to this essay for a species of information which is not to be found elsewhere. The second Report of the Prisons of England contains, it is true, a statement of the weight of prisoners on their entrance into and discharge from the house of correction at Devizes; but M. Marc d'Espine shows that these results are wanting in accuracy, from defective arrangement of the facts on which they are founded. The subject is well reasoned out in his own essay, though the facts are scarcely sufficiently numerous. The system pursued is a modification of the American system. The prisoners labour in common and in silence during the day, and occupy separate cells at night. The peculiarity is one well worthy of imitation. The prisoners are divided into four classes, and a promotion from the worst to the next one above it is attended with some slight indulgence, which offers a motive to good conduct and holds out a hope of improvement. The inmates are not, as in the worst of our penal settlements, cut off from all hope. The effect of this wise arrangement displays itself in the improved health of those who have reached the two upper classes, as tested by an increase of weight. On the average of all the prisoners there is no material gain or loss of weight; but those who are placed in the most favorable circumstances have also the best health, and are found, on the average, to increase in weight. The following are the author's own conclusions:—1. That the number of prisoners emaciated by the penitentiary system is a little larger than that of those who have increased in weight. 2. That this difference, less than we should imagine, *à priori*, is remarkable only in the divisions in which the regimen is most severe. 3. That the loss of flesh does not occur in the first month of confinement, but much later. 4. That summer or winter, cold or heat, do not seem to exert a special influence on the degree of emaciation.

Discorso Storico-critico del Dottor GIUSEPPE GIROLAMI, Medico in Civita-Vecchia.—*Napoli*, 1844.

A Critical and Historical Discourse regarding Gentili da Fuligno, a celebrated Physician of the 14th century. By Dr. G. GIROLAMI, &c. 8vo, pp. 60.

ITALY was full of popular cities, and abounded in all the knowledge and arts which emanate from popular cities at a period when the rest of Europe was but half civilized. Amongst those learned men, who constituted the avant-couriers of modern philosophy, Gentili da Fuligno was conspicuous. He was the son of a celebrated physician and writer of Fuligno, (from which city the family surname was derived), who was born there in 1230, and died at Bologna in 1310. Gentili was educated at Bologna, the pupil of the celebrated Taddeo di Fiorenza, who was professor there from 1260 to 1295, and an enthusiastic student of the Hippocratic books. At Bologna, Gentili became distinguished for his learning, and was chosen to fill the chair of medicine at Bologna and Perugia. Both these cities conferred upon him the right of citizenship, and Perugia, in addition, presented him with a house situate near the church of the Augustines. He was also professor at Padua during the years 1337 to 1345. Ubertino da Carrara, lord of Padua, having fallen sick, sent for him to Padua, and that he might have Gentili near him, presented him with the professorship. It is supposed Gentili was also physician to that same Pope John 22d, who had Petrarch for his secretary, and by him was loaded with wealth and honour.

In the year 1340, the black death devastated Italy. The contagion was brought by a Genoese vessel to Genoa, from whence it spread to the adjoining towns, arriving at Perugia in April. It there broke out with all the symptoms of the Levant plague. It is not certain whether Gentili went expressly to that city to assist its inhabitants in grateful return for the honour and property they had bestowed upon him, or whether he was already there occupied with the duties of his professorship. Dr. Girolami inclines to the latter opinion. Be this as it may, Gentili devoted himself most assiduously to the duties of his calling. He immediately wrote his "Consilium de Peste," for the instruction of the citizens; he recommended and carried out such measures of public hygiene as he thought necessary; and at last, wearied with incessant attendance on the sick, was himself attacked with the plague, and died in six days. The manuscript copy of his "Consilium de Peste," found in the Malatesta Library at Cesena, has the following annotation by a pupil at its close:

"Et postea Gentilis infirmatus est ex nimia requisitione infirmorum, et hoc fuit 12 die Junii, et vixit sex diebus, et mortuus est, cujus anima requiescat in pace. Hoc fuit MCCCXLVIII. Et ego Franciscus de Fulgineo interfui ægritudini ejus, et numquam dimisi eum usque ad mortem, et sepultus fuit Foligini in loco Eremitarum." (p. 10)

There is here a charming touch of human kindness. In the midst of despair and death, amongst the arrows that flew at noonday, and the pestilence that walked in darkness, the master of the medical art, led on

the van of his small and feeble array. He sinks, overpowered; but his pupil, regardless of the danger, is with him to the end, and on the dusty MS. of his master records the fact, "I, Francis of Fuligo, attended him in his sickness, and left him not until he breathed his last." And when the tumult of pestilential destruction was sunk into the stillness of death, the same faithful hand removed his remains to his native soil.

Gentili wrote several works, &c. A comment on the first three canons of Avicenna; Consilium de Peste; De Febribus; Consilia Peregregia, or a Collection of Cases; a tract on Hernia; Recepta; De Balneis; Quæstiones subtilissimæ in Artem parvam Galeni; De Proportionibus Medicinarum, &c.; Expositio cum Commento Mag. Egidii Monachi Benedictini, lib. i.; (an example of a monk-physician;) Judiciorum de Urinis; De Lepra Tractatus; De Balneis Tractatus. The scientific bibliographer will find the titles and editions of these books with other information regarding them in Dr. Girolami's pamphlet. There is also an account of the social, political, and literary condition of Italy, at the time in which Gentili flourished,—a topic always pleasing to the Italian,—for the contemplation of the past glory of his country affords him hope and solace when he views its present abasement.

ART. VIII.—*Beiträge zur Erkenntniss und Heilung der Spinal-Neurosen.* Von Dr. GEORG HIRSCH in Königsberg.—*Königsberg,* 1843.

Contributions to the Diagnosis and Treatment of Spinal Neuroses. By Dr. G. HIRSCH, &c. *Königsberg,* 1843. 8vo, pp. 466.

THIS is an elaborate monograph on what has been usually termed spinal irritation. It is characterized by the research and minuteness usually found in German works, and will form a good book of reference, although the author has not made himself acquainted with all the views that have been broached on the subject. He confines himself to the old beaten track through the *anatomical* relations of the spinal cord, and omits to notice the more recent views propounded by Dr. Laycock as to the influence of the physiological relations of organs, or, in other words, their sympathies in determining the organ, or class of organs, in which the phenomena of spinal irritation appear. The consequence is, that the higher or cerebral phenomena of hysteria, and spinal irritation, as insane secretiveness, abhorrence or love of colours, and the like, find no place in his Contributions. Yet it is certain that these are analogous in their origin to the symptoms which indicate irritation of the spinal cord only. As, however, we have, on more than one occasion, lately brought this subject before our readers, we must content ourselves with this hint to Dr. Hirsch, and others of his class, as to the propriety of adopting more comprehensive principles in laying down their pathology. It may be convenient for a systematic arrangement to distinguish the symptoms of spinal affections from those of cerebral affections, just as it is convenient to distinguish one artery from another. But when the general pathology of the nerves, or vascular system, is under discussion, it is well to get rid of these mere aids to arrangement, and let the mind take in the whole, unembarrassed by details of minor and sometimes doubtful value.

On a Gland, hitherto not accurately described, in the interior of the tip of the Tongue. By A. Nühn, M.D., &c.—*Mannheim*, 1845. 4to, pp. 8.

Many less modest authors would have made a larger book than this, to proclaim the discovery of an organ, visible to the naked eye, in the human body. We have found Dr. Nühn's description of these glands exactly true to nature; and they are so easily dissected, that all who are interested in anatomy will, with a few helps, find them for themselves. When the mucous membrane is removed from a slightly elevated elongated oval space which is always seen about two lines from the median line, on the under surface of the apex of the tongue, a layer of longitudinal muscular fibres is at once exposed. If, next, these, which form part of the inferior lingualis and of the anterior fibres of the styloglossus, be reflected or removed, the gland is seen just under the canine artery, outside the expanding branches of the lingual (5th) nerve, just anterior to the edge of the hypoglossus. The gland measures from seven to ten lines in length, from three to four and a half in breadth, and from one and a half to two and a half in thickness. In structure and general aspect it agrees with the larger labial, lingual, and other similar glands. It has many ducts which open through the mucous membrane over it, by irregularly placed orifices, each of which is surrounded by a slightly elevated ring. The author supposes it to be a mucous gland; and that it may be in some way subservient to speech, for he finds it in no animal, except man, and the orang, though he has searched for it in very many species. In the orang it is rather larger than in man.

Art. X.—*A Clinical Introduction to the Practice of Auscultation, and other Modes of Physical Diagnosis: intended to simplify the Study of the Diseases of the Lungs and Heart*. By H. M. Hughes, M.D. Fellow of the Royal College of Physicians, Assistant Physician to Guy's Hospital.—*London*, 1845. 8vo, pp. 246.

This is a very excellent work, and cannot fail to be of great use to the attentive student. In it he will find every information requisite to direct his proceedings in the clinical study of auscultation. The author does not pretend to any originality, but every page shows that he has learnt what he teaches from the great book of nature, as well as from the writings of his predecessors. While Dr. Walshe's admirable treatise 'On the Physical Diagnosis of Diseases of the Lungs' (noticed by us vol. XV. p. 223) exists, we cannot admit, with Dr. Hughes, "that a work was wanted in which should be simply explained to the student of auscultation, not merely the origin, character, and diagnostic value of certain physical signs, but also the manner in which he should proceed to elicit them;" (Pref. p. ix;) because we believe that Dr. Walshe's treatise supplies all this in the most unexceptionable manner, and with singular clearness, conciseness, and completeness. At the same time, we are far from quarrelling with Dr. Hughes for having written and published his book. Although inferior to Dr. Walshe's in originality, in comprehensiveness, and in clearness of arrangement, and although exhibiting less of refined observation and critical

sagacity, it is well calculated to fulfil the object for which it has been written. We are not sure but its very inferiority to Dr. Walshe's treatise may be a cause of its greater usefulness. There is a numerous class of students to whom the aphoristic conciseness of Dr. Walshe will be much less welcome, and even less intelligible, than the plain continuous narrative style and frequent repetitions of Dr. Hughes. And the same remark applies to the arrangement of the subjects adopted by the two authors. Dr. Walshe's plan is unquestionably much more philosophical, and is even clearer and more practical than that of Dr. Hughes; and yet this very plan,—involving as it does a greater subdivision of subjects, and thus rendering it necessary, in the working out of any one point, to refer to three or four different parts of the volume,—is likely to be a cause of puzzle and annoyance to some readers. We are therefore pleased to think that by the publication of Dr. Hughes's volume, this class of students will be conciliated, and the progress of auscultation practically extended. To the student of auscultation we would indeed say, possess yourself of both works—both will be found useful, neither superfluous. It is but justice to Dr. Hughes to add, that his work includes the subject of diseases of the heart, while Dr. Walshe's is confined to that of diseases of the lungs. Dr. Hughes's book is written with great modesty, and everywhere exhibits a becoming distrust of his own powers, and the powers of his art. He gives ample evidence of possessing that amount of knowledge which can afford to confess its deficiencies.

ART. XI.—*The History of the Middlesex Hospital during the first Century of it existence.* By ERASMUS WILSON, F.R.S.

This is a work most creditable to the author. It is almost unique of its kind; at least we have never seen any account of a British hospital at all to be compared with it in fulness and accuracy of detail. If in addition to the economical history, Mr. Wilson could have given us the medical history of the hospital—that is, a complete report of the diseases admitted, with their events, &c., during the same period, he would have left us nothing to desire. We entirely assent to the author's estimate of the probable influence of the present work on those into whose hands it may fall.

"Certainly, if a picture of the admirable management of a truly benovolent institution be calculated to make an impression upon the human mind,—if the testimony of an upright and conscientious spirit of action have any weight with the charitable and the humane, the conduct of the Governors of the Middlesex Hospital, during a century of usefulness, cannot be otherwise than favorably estimated." (p. x.)

ART. XII.—*Elementa Physiologiæ specialis Corporis Humani.* Auctore AUGUSTO ARNOLDO SEBASTIAN, Ordinis Leonis Neerlandici Equite, &c. Editio altera. *Groningæ*, 1842. pp. 356.

THIS work is dedicated to his class by Professor Sebastian. The plan he has adopted is on the model of the older writers. The work is divided into numbered aphorisms, each concisely stating a fact, or a principle. There is nothing original in the work. It is a good compendium, and a work suitable to students.

Art. XIII.—*An Inaugural Address, delivered at the Opening of the Norfolk and Norwich Hospital Museum,* Sept. 10, 1845. By J. C. Crosse, M.D., F.R.S., the Senior Surgeon to the Hospital. *Norwich,* 1845. 8vo, pp. 28.

We can pay this little book no higher compliment than to say, it is a fitting memorial of the event it celebrates. Its author is one of the principal founders of the splendid Institution which now enriches and adorns the Medical School at Norwich; and the character of this Address proves that its literary inauguration could not have been intrusted to an abler pen. Dr. Crosse gives a delightful account of the many great men of our profession who have illustrated Norwich, including the names of Kaye or Caius, Sir Thomas Browne, Howman, Benjamin Gooch, William Donne, Alderson, Yelloly, Wright, Rigby, Martineau, and Dalrymple—the last alone surviving. We doubt not that those who heard this eloquent Address, felt that there were yet giants in the land; and all who know Dr. Crosse's writings are aware that the future historian of Norwich will have to place his name not last in the roll of the medical worthies who give lustre to that ancient city. We regret that our circumscribed space prevents us doing justice to Dr. Crosse's Prolusion. We can only introduce, by way of specimen, a fragment of the sketch he has given of Mr. Dalrymple.

"All unbecoming competition Mr. Dalrymple was a stranger to, for his ardent love of the calling he had embraced, and so successfully pursued, was the powerful and the only needed stimulus to his exertions. Whilst practising in this city, he constantly showed himself more jealous of his reputation than of his purse; he left the public to find out his qualifications, and required that it should seek to reward him, being not in the pursuit of wealth, like one possessed of mediocrity of talent, but in pursuit of the science of his profession and of his own continual improvement in that science. It appears that Mr. Dalrymple was many years resident in this city ere his talents were adequately appreciated, and longer before he became connected with this Hospital. His rise was the slower, but perhaps the more secure, from his delicacy on certain professional points, and his deference to certain customs which help to regulate our profession, and which if young men do not respect, they have no longer any guide but their passions. People are led so much more by their prejudices and feelings, than by reason and good judgment, that the art of rising in practice consists as much in *knowing how to please,* as *how to cure diseases;* it is not surprising that Mr. Dalrymple, who so well understood *both* these, should have ultimately gained the most extensive employment and the highest reputation in his profession; no man knew better how to unlock the affections of those patients whom he once attended, and he opened all the avenues to their understandings by the most refined eloquence. . . . Who is there that knew much of him, and did not applaud that great sensitiveness for his patients, which led him to feel interested in their comforts and well-doing, to his own distress and injury? or has not been captivated by his eloquence, his ready wit, and his extensive information, available upon all the general topics that engage the attention of the most polished society, and which imparted a charm to his conversation that raised him above all ordinary men? Amidst the honorable strife inseparable from our calling and under circumstances which always placed me in unequal competition, no living being has ever heard from my lips a word respecting Mr. Dalrymple, that was not in strict accordance with this public statement; I have no winged words of flattery for this occasion, and there is no fear of my language surpassing the subject of my discourse, as regards his professional abilities and character."

BOOKS RECEIVED FOR REVIEW.

1. Spinal Affections, and the prone system of treating them. By James Coles, Surgeon.—London, 1845. 8vo, pp. 320.

2. On the Periodical Maturation and Extrusion of Ova. By Professor Bischoff. Translated by Henry Smith, Surgeon. (From the Lond. Med. Gazette.) 1845. 8vo, pp. 34

3. The Cold-water Cure, its Use and Nature examined. By Herbert Mayo, M.D. F.R.S.—London, 1845. 8vo, pp. 84.

4. Prize Clinical Reports of Surgical Cases treated at the Queen's Hospital Birmingham. By John Moore.—London, 1845. 8vo, pp. 66.

5. The Irish Watering Places; their climate, scenery, and accommodations; including analyses of the principal Mineral Springs by Dr. R. Kane. By A. Knox, M.D.—Dublin, 1845. 8vo, pp. 332.

6. Fruits and Farinacea, the proper Food of Man. By John Smith.—Lond. 1845. 8vo.

7. On the Nature, Causes, Prevention, and Treatment of Acute Hydrocephalus. By Thomas Smith, A.M. M.D.—London, 1845. 8vo, pp. 108.

8. Introductory (Lecture) for 1844-5, On the present position of some of the most important of the modern Operations of Surgery. By J. D. Mütter, M.D.—Philadelphia, 1845. 8vo, pp. 34.

9. Some Observations on Organic Alterations of the Heart; and particularly on the beneficial Employment of Iron in the Treatment of such Cases. By S. Scott Alison, M.D.—London, 1845. 12mo, pp. 62.

10. Examination of the Views adopted by Liebig on the Nutrition of Plants. By W. Seller, M.D.—Edinburgh, 1845. 8vo, pp. 22.

11. Anecdota Sydenhamiana; Medical Notes and Observations of Thomas Sydenham, M.D., hitherto unpublished. — Oxford, 1845. 18mo, pp. 80.

12. Cosmos; a Summary of the Physical History of the Universe. By A. von Humboldt. —London, 1845. 8vo.

13. The Half-yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D. Vol. I. Jan.—June, 1845.—London, 1845. 8vo, pp. 390.

14. A Practical Treatise on Inflammation, Ulceration, and Induration of the Neck of the Uterus. By J. H. Bennet, M.D. &c.—London, 1845. 8vo, pp. 212.

15. The Pharmaceutical Latin Grammar; being an easy Introduction to Medical Latin. By A. J. Cooley.—London, 1845. 8vo pp. 132.

16. A view of the Formation, Discipline, and Economy of Armies. By the late Robert Jackson, M.D. Inspector-general of Army Hospitals. The Third Edition, revised, with a Memoir of his Life and Services.—London, 1845. 8vo, pp. 426.

17. Medical and Physiological Problems. By W. Griffin, M.D. and D. Griffin, M.D.—London, 1845. 8vo, pp. 356.

18. The Nature and Treatment of Gout. By W. H. Robartson, M.D.—Lond. 1845. 8vo, pp. 372.

19. A Dictionary of Practical Medicine. By James Copland, M.D. F.R.S. &c. Part X. (*Palate Pestilence.*) 8vo, pp. 144.

20. The History of the Middlesex Hospital during the first century of its existence. By E. Wilson, F.R.S.—London, 1845. 8vo, pp. 296.

21. Notes on a microscopical examination of Chalk and Flint. By G. A. Mantell, LL.D. F.R.S. London, 1845. 8vo, pp. 24.

22. The Ocean Flower; a Poem, preceded by an Historical and Descriptive account of the Island of Madeira. By F. M. Hughes. London, 1845. 8vo, pp. 309.

23. Urologie. Des Angusties, ou Retrecissements de l'Urètre et de leur Traitment rationnel. Par le Dr. Leroy-d'Etiolles. Paris, 1845. 8vo, pp. 488.

24. Elements of Chemical Analysis, Qualitative and Quantitative. By E. A. Parnell. New edition, enlarged. London, 1845. 8vo, pp. 520.

25. Nuovi Elementi Fisio-Patologici di Medicina Eclettica del Dottore Niccolo Celle. Pisa, 1842. 8vo, pp. 486.

26. Metastasi riprovate dalla struttura dei Tessuti a dalla Funzioni dei Medesimi. Trattato di G. D. Bellini, M.D. Firenze, 1845. 4 fasciculi.

27. Sull' Acopuntura. Del Dr. G. B. Bellini. Fano, 1844.

28. Sui vantaggi dell' incisione laterale della bocca dell' utero nelle isterorragie per distacco di placenta, gli ultimi mesi di gravidanza. Di Dr. G. B. Bellini. 1841-45.

29. Opere di Maurizio Bufalini, Professore della Clinica Medica nelle Scuole Mediche-Chirurgiche di complimento a perfezionamento dell' Universita di Pisa in Firenze. Firenze, 1844-5. 2 vols. 8vo, pp. 378, 378.

30. Saggio Illustrativo le Tavole della Statistica Medica delle Maremme Toscane compilata. da Antonio Salvagnoli Marchetti. Firenze, 1844. Fol. pp. 86.

31. Sui Pregi e Doveri del Medico, del Professore Roberto Sava. Milano, 1845. 8vo, pp. 216.

32. Della Mortalitá e dimora Media dei Malatti nello Spedále Maggiore di Milano dal 1811 al 1844 ed in quello dei RR. Fatebenefratelli dal 1604 al 1844 coi prospetti del calcolo complessivo sopra 784,000 infermi. Memoria del Medico-Statista Dottore Giuseppe Ferrario. Milano, 1845. 8vo, pp. 16.

33. The Modern Treatment of Syphilitic Diseases, both Primary and Secondary. By Langston Parker, Surgeon to the Queen's Hospital, Birmingham. Second Edition. London, 1845. 8vo, pp. 228.

34. First Steps to Anatomy. By J. L. Drummond, M.D. London, 1845. 8vo, pp. 201.

35. Homœopathic Domestic Medicine. By J. Laurie, M.D. Third Edition. London, 1846. 8vo, pp. 576.

36. On Scarlatina, and its successful treatment by the Acidum Aceticum dilutum of the Pharmacopœia. By I. B. Brown. London, 1846. 8vo, pp. 66.

37. Lectures on Natural and Difficult Parturition. By E. W. Murphy, A.M., M.D., Professor of Midwifery, University College. London, 1845. 8vo, pp. 263.

THE

BRITISH AND FOREIGN

MEDICAL REVIEW,

FOR APRIL, 1846.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

Die Operative Chirurgie von Johann Friedrich Dieffenbach. Erster Band.—*Leipzig*, 1845.

The Operative Surgery of J. F. Dieffenbach. First Volume. *Leipsic*, 1845. 8vo, pp. 856.

There is a great difference between authority in matters of opinion, and authority in matters of fact. Combating as we have continually done against the evil effects of the former in impeding the progress of inquiry, we have been equally willing to yield a ready deference to the latter. Medical authority and surgical authority, also, must be regarded in a different light, even in practical matters. The effects of medicines are obscured by the natural progress of the disease; by changes of weather, and by moral and other influences often inappreciable to the physician, so that some scepticism becomes natural to the critical inquirer after truth, when comparing the results of opposite modes of treatment in internal diseases. But in surgery the nature of the cure is plain, the end attained by an operation evident, and when a man whose practice is open to the scrutiny of a public hospital comes forward and says, "I have performed this operation a thousand times, and prefer this plan," every prudent surgeon must bow to such authority, and feel grateful to the writer who thus enables him to correct or modify the impressions derived from his own more limited experience. Regarded in this light the work before us is one rather for analysis than criticism. The author states that he has written his book "to communicate what he has found useful in surgery to those who have seen less than himself,"—like Richter, desiring not to write a book for the learned, but one containing no page in which a practical surgeon may not learn something calculated to prove useful at the bedside. He says he has seen almost everything that can happen to the sick, and has observed with his own eyes, and never through "coloured

or strange glasses," and in all cases has preferred expressing his opinion freely and openly, to taking a "half-and-half course between yes and no." The work is no survey or retrospect of an anxious and busy life, no pensive contemplation in the evening of existence, but the record of daily observations made with all the ardour of youth in the presence of passing events. It is replete with valuable practical matter, the more important part of which we now proceed to lay before our readers.

After a few introductory pages, and a very hasty glance at the history of operative surgery, with some general remarks on surgical instruments, to the end that they should be of the best material, and simple in construction, the operator working and not the tool—our author enters upon the first division of his work, a description of the "operations which are performed on the various parts of the body." The whole of the volume before us is occupied by this division, extending to sixty-seven chapters, which we now proceed to examine in detail. The First is on the extraction of foreign bodies, and is divided into four sections: 1. On the removal of foreign bodies fixed to, or surrounding projecting parts of the body, as rings over the fingers or penis. 2. On their extraction from the natural passages, nares, antrum, meatus auditorius, eyelids, air-passages, pharynx, and œsophagus, stomach and intestines and rectum, and genital organs of the female. 3. On their extraction from the substances of the body, as balls and shot, parts of knives, nails, glass, &c. 4. Method of removing foreign bodies which remain after the healing of the wound caused by this introduction: here we have a curious case of a girl who had been subject to epileptic fits, which ceased after the removal of several fragments of glass from the hand by incision, which had remained several years, she having broken a glass in her hand. The chapter is closed with remarks on the removal of fragments of dead bone, and is useful and practical, without however containing anything particularly new, or peculiar to the author.

The second chapter is on *the surgical knot*, or the union of wounds by suture, comprehending the treatment of wounds by the common suture with the assistance of adhesive plaster, &c., the various applications of the twisted suture, and a comparison of the advantages of the two methods. The latter is a great favorite with Dieffenbach in plastic operations, and in all cases where very accurate union is required to obviate ugly cicatrices. He employs the small spring-hardened insect needles, manufactured at Carlsbad. The use of the subcutaneous suture is then recommended in cases of obstinate salivary, urinary, and other fistulæ. The thread is introduced beneath the skin at some distance from the fistulous opening, and the canal is surrounded by the ligature in the same manner that an artery is tied. The ligature may be left from four to fourteen days, according to circumstances; it is better to withdraw it too late than too early. It is to be employed if the twisted suture have failed, and the author states that he has often used it with success in cases which had resisted every other mode of treatment. The chapter is concluded by some remarks on the knots of the olden surgeons. The author does not appear to be acquainted with the platinum suture, so successfully employed at Guy's Hospital by Mr. Morgan.

In the third chapter *the potential and actual cautery* are described, their uses and modes of application. The chapter is long, but the author

means are applicable, and states that this practice has much declined in Germany.

The application of leeches is the subject of the fourth chapter, venesection of the fifth, and arteriotomy of the sixth. They are all good, but do not require remark from us, nor do the two following on infusion and transfusion. In the former the methods of injecting various saline and other solutions into the veins are described, and in the latter the blood of one person into the veins of another.

The ninth chapter treats on *the ligature of arteries.* The general remarks on the application and effects of the ligature to suppress bleeding from wounds and obliterate the trunk in states of disease, are concise and practical, but somewhat commonplace. The operations upon particular arteries are then described, namely, the common carotid, superior thyroid, lingual, temporal, occipital, posterior, auricular, innominate, subclavian and axillary in various situations, inferior thyroid, internal mammary, brachial, ulnar, radial, abdominal aorta, with its subdivisions, and the arteries of the lower extremities. In a work from the head of the German school of operative surgery, professedly a complete system of this department of medical science, one would naturally expect to find such an important subject as thel igature of arteries fully and completely treated, or, to say the least, that some results of the personal experience of the writer would be given for the guidance of his readers in their operations upon the principal arterial trunks. He states that what the discovery of printing was to science, of gunpowder to war, of railroads for the communication of one people with another, the discovery of the ligature of arteries was to surgery; yet he describes the methods of tying all the arteries in the body separately in eighteen pages. The innominata occupies half a page, the internal and external iliacs the same space, the aorta a little more. We should not quarrel with this if he had condensed what was known on the subject into this space, or if he had given a clear account of what he had found to be the best mode of operating; but instead of this we have a hasty and imperfect sketch of the anatomy of the parts concerned, and of the different methods adopted by several surgeons, without appreciation of their respective merits. The chapter is altogether unsatisfactory, and we suppose does not come within the writer's *spécialité.*

In the following chapter on *the torsion of arteries*, Dieffenbach informs us, that, like other surgeons, he has found it useful in checking the hemorrhage from small vessels bleeding upon an open surface, as after the removal of tumours. With regard to torsion in the continuity of an artery, instead of the ligature to produce its obliteration, he well remarks that, however interesting as a physiological experiment, it is quite useless to the practical surgeon.

The eleventh chapter treats of the *operation for aneurism* after the methods of Antyllus, Hunter, and Brasdor, the history of these methods and the cases in which they are severally applicable. The author considers that of Antyllus the best in cases of false aneurism following wounds of arteries, as in the elbow after unskilful venesection. We may remind our readers that Antyllus's plan is that of tying the vessel both above and below the aneurism, and then opening the sac. In all cases of true aneurism this is to be discarded for either Hunter's or Brasdor's operation. The

operation for false aneurism is first treated in general, and then when situated in the bend of the left elbow. This is minutely and well described, but appears to have been far from successful, as the author states that he has very frequently performed it, after wounds from venesection, and notwithstanding the most careful operating and after-treatment, at least a fourth have died. He goes on to the consideration of popliteal aneurism and its treatment by ligature of the femoral artery, and then to the operation of Brasdor. This he considers of great value in cases where it is impossible to place the ligature between the aneurismal sac and the heart. The ligature being placed beyond the sac, the blood cannot flow from the latter; it stagnates there, coagulates, afterwards becomes absorbed, and the sac is necessarily obliterated. The ligature must not be placed too near the sac, or the diseased coats of the artery will give way, and secondary hemorrhage be the result. This is of course all well known, but it is well to know the present opinion in Germany on so important a subject.

The operations for varicose tumours. These are considered in the twelfth chapter. Exposure of the tumour by incision without opening it, and filling the wound with charpie, which heals by cicatrization, is not dangerous, but very seldom successful. The same may be said of puncture with the lancet, and compression afterwards by plaster and bandage. Incision is more dangerous without being more successful. Extirpation has been recommended by exposure of the varix and division of the veins leading to it, above and below. It is also dangerous. Ligature of the vein in any way is highly dangerous. Ligature by needles and twisted suture, as recommended by Velpeau, Dieffenbach says he has several times seen followed by great danger in Paris. The carrying a needle and thread through the tumour, and leaving the latter for two days, has been followed by obliteration of the veins, but also by severe phlegmonoid inflammation. Cauterization of the varix is described as excessively dangerous. On the whole, the author is of opinion that no patient should be recommended to put his life in danger by undergoing any of these operations, or any others which have been proposed, in order to get rid of what is seldom more than an inconvenience. The disease is generally a constitutional one, and operation is no more to be recommended for a single varix, than for an external scirrhous tumour, when the same disease exists in other parts of the system. We believe that this is the conclusion to which the experience of the last twenty years must bring all prudent surgeons. We have seen the application of caustic potash along the course of the vein above the varix extensively used. The vein participates in the inflammation of the surrounding parts, and generally just enough so to cause its obliteration without being followed by diffuse phlebitis. A succession of forty successful cases, without a single symptom of dangerous consequences, had led us to believe that at length the true method of treating varix was discovered, when two fatal cases in succession dispelled the delusion, and we believe, as we have just stated, that it is better to diminish the inconvenience of varices by laced stockings or elastic bandages, than to recommend any operative proceeding whatever.

The author scarcely touches upon the mechanical treatment of varix; yet much more than palliation of distressing symptoms may be obtained

by its judicious application; and we believe in stating the result of our observation of the result of simple compression in the treatment of varicocele, we impart a practical fact of great value to our readers. The unsatisfactory results of all the known methods of treating this disease, with the exception of the ligature of the veins, which though often successful, is often followed by dangerous phlebitis, led us to reflect on the effects of the ligature and the means of safely fulfilling the same indications. What is the effect of the ligature when successfully applied? Simply obliteration of the vein, preventing the gravitation and pressure of the column of blood upon its dilated portions, and necessitating the return of the blood by some other channel. Now came the question, how could compression be applied to fulfil the same purpose? At first sight it appeared that anything like compression between the varix and heart would inevitably increase the tumour by preventing the veins from returning their blood to the heart; but still a ligature does so, and if it does not kill the patient, often cures the disease. We determined then to try the effects of compression upon the spermatic veins as they pass through the inguinal canal, and at first used linen compresses and bandages, with some encouraging result, but a little experience proved that a common truss was by far the most convenient means of applying pressure. We have notes of 24 cases of greater or less severity in which wearing a truss from 6 to 18 months has completely cured varicocele of long standing. The patient should be kept in bed or on the sofa, with the scrotum elevated during the first week of its application, and the pad applied immediately above the pubes, so as to press upon the whole of the inguinal canal. The pad must not be too large and flat, or it will not answer the desired purpose; nor too conical, or it will not only cause inconvenience and pain, but also, by separating the fibrinous insertions of the oblique muscle, reduce their resistance to herniary protrusion. The common spring-truss, well fitted, answers much better than the ball-and-socket pad, and has not the disadvantage of the pad behind. Immediate relief follows the application; the column of blood is prevented from bearing upon the dilated veins; they contract, and the portions subjected to pressure become either greatly thickened in their coats, as their hardness evinces to the finger, or perhaps altogether obliterated. We have not hitherto had an opportunity of examining the state of the veins after death; therefore do not enlarge upon the anatomical effects of this mode of treatment, but strongly recommended it as a simple, safe, and successful substitute for former difficult, hazardous, and doubtful proceedings.

In the following nine chapters, vaccination, cupping, scarification, the formation of issues and setons, the opening of acute and chronic abscesses, simple and electro-galvanic acupuncture, and the dilatation of wounds, are successively treated. We have found nothing on any of these subjects peculiar to the author or to German surgery, and therefore pass to the 22d chapter.

The operations upon cicatrices. Operation upon a cicatrix may be required,—1, when it has disfigured the form of any part; 2, when it interferes with the function of a part; 3, when its condition is morbid.

The operations are four—1, subcutaneous division; 2, division from without; 3, extirpation; and 4, incision or extirpation, with transplantation of skin.

Subcutaneous division is described as often successful in removing contractions of cicatrices about the face, neck, arms, and fingers. It is performed with a fine, narrow, sickle-shaped knife. At any convenient point of the cicatrix, the neighbouring healthy skin is punctured; the knife is passed beneath the base of the cicatrix, which is then divided as freely as possible without injuring the skin. In cases where the skin and subcutaneous tissue have become adherent to subjacent periosteum, the knife is passed flatly as near the bone as possible, and division effected to any extent which may be necessary. If the cicatrix be very large, it may be necessary to make more than one puncture in the skin. The cicatrix only should be divided, not any of the surrounding healthy cellular tissue. If bleeding beneath the skin come on, charpie and compresses must be applied, and the part surrounded by a bandage. This general description of the operation will apply to all cases in which it may be necessary to remedy distortions of the mouth, nose, eyelids, neck, fingers, &c.

The division of a cicatrix from without is seldom necessary to remove disfiguration of a part, but often to restore the functions of a limb. The operation consists either in a simple transverse division, in repeated transverse separation or notching, or in oblique division. The first plan is often successful in sharp band-like cicatrices in the neck and flexures of the limbs; the second, when the cicatrix consists of indurated cellular tissue, firmly adherent to surrounding parts, and particularly when it is connected with a tendon; the third is most useful after wounds in the inside of the hand and fingers, and is generally successful. Dieffenbach has completely restored the use of the finger after it had been so drawn into the palm of the hand as to fix the nail in the skin, and cause the patient to pray for amputation.

The *extirpation* of cicatrices is particularly required in young females to diminish disfiguration, and in other cases where its texture is morbid; when the wound was produced by broken glass, china, &c.; in cases of hypertrophy of the cicatrix, or chronic inflammation, with re-opening, which resists other treatment. Incisions are made in the healthy skin, including the whole extent of the cicatrix, which is then raised with forceps and dissected away, and the edges of the wound brought into close apposition by small needles and the twisted suture. If, however, the cicatrix be so large that the union of the edges of the wound would be impossible, repeated partial extirpation is the best practice. A long slip, with pointed edges, should first be excised from the middle, and when the wound so produced has healed, after some weeks, or, better still, some months, another piece is excised, and so on until the whole has been removed.

In some of the most difficult cases it is necessary to divide the cicatrix in its centre, and transplant skin from a neighbouring part between the edges of the wound. The manner of doing this will be explained in the division of Plastic Surgery, and we may refer to Dr. Müller's case, given in our 19th vol., p. 411-12. In all cases, after these operations, warm applications are far preferable to cold, as chronic inflammation, lividity and death of the edges of the wound frequently follow the latter.

Operations on nævi. In this chapter the author speaks highly of the effects of the pure liquor plumbi, and of the solution of alum, in flat nævi up to the size of a crown-piece. Lint steeped in the lead solution is

cations of the lead, without frequent removal of the lint. After days, or weeks, the swelling becomes whiter, flatter, and firmer; soon after, little firm white spots form on the surface, and the cure is certain. By means of the solution of alum and compression, Dieffenbach has cured nævi so large that extirpation would have been impossible. It may be necessary to keep it constantly applied for six months.

Compression is often useful, but often also dangerous, and frequently inapplicable from the situation of the nævus.

Irritation by blisters, vaccination, &c., has never proved successful in the experience of the author.

Tattooing with three needles, bound together, and dipped in a coloured solution, which is also rubbed in, has been said to alter the colour of the spots. The author does not speak of its effects from personal experience, but says it may be employed on the faces of young females, where other means are inapplicable. He does not give the composition of any of the coloured fluids.

Transfixing the tumour in various directions with needles is useless in the opinion of our author: it is very uncertain even when threads are carried by the needles, and then tied. The seton is not to be recommended.

The ligature is to be applied when the tumour is pediculated, or when situated in the mouth, neck of the uterus, &c. If the base be broad, needles may be passed through it, and the threads knotted in opposite directions.

Caustics and cauteries are generally more painful, and less useful than excision.

Ligature of the principal artery leading to the tumour is not to be recommended, as its result is very uncertain.

Extirpation of the tumour and union of the edges of the wound by needles and twisted suture, is the best method of all, when the lead or alum has failed. Extirpation is to be either total or partial, according to the size of the tumour. If partial, it is done in the same way as partial excision of a cicatrix. Several interesting cases are given as examples of the success of this method: the chapter is a practical and interesting one.

Operations on tumours. We now come to the operations for the removal of tumours. These are,—1, extirpation by the knife; 2, ligature; and 3, chemical and mechanical means for their destruction. Of the two former methods we have nothing new in the work before us, but several cases are recorded, in which large encysted tumours, containing fluid, were successfully treated by trocar puncture, and carrying a seton through the tumour. This practice is particularly recommended in large watery tumours with thin sacs, lying between muscles, and especially between the muscles of the back, and beneath the scapula. The trocar is introduced, and the seton passed through the canula, which is then withdrawn. The seton is smeared with the cantharides ointment, and several times daily it is partially withdrawn, the wet portion cut off, and all discharge squeezed from the sac of the tumour. When the discharge becomes thicker the seton is withdrawn, the openings only kept open, and compression applied to the other part. Caustics are occasionally useful to destroy parts of cysts which cannot be removed by the knife.

In chapter 25 we find the operations for *ganglia* described. Dieffenbach finds the best treatment is to burst them by a smart blow with a hammer. This would frighten most patients a great deal more than the plan usually followed in this country, of striking with the back of a book. We should think it also very easy to miss the aim with a hammer, and probably strike the wrist instead of the ganglion.

Tumours of nerves are to be removed by incision, and exposure of the nerve above and below the tumour as far as its size is increased. Where it is seen of the natural caliber, it is to be divided, and the enlarged part, with the tumour, dissected away together.

We have now a very long chapter on *polypi*. Various operations are required, according to their nature. Caustics are principally applicable to carcinomatous polypi; extraction to polypi of mucous membranes; ligature to fleshy vascular polypi; and excision to those of a fibrous texture.

The rules for the treatment of nasal polypi do not differ from those laid down in most surgical works. Several interesting cases are recorded in which large fibrous polypi of the fauces were removed by excision, division of the velum palati being necessary before they could be reached or removed. The fissure in the velum was afterwards healed by suture.

The various modes of operating upon polypi in all situations are fully described in a sound practical manner. The chapter is a useful one, but does not add much to our former knowledge of the subject.

PLASTIC SURGERY. We now arrive at the principal subject of this volume, and favorite study of its author—Plastic Surgery, which is treated at great length, occupying upwards of seven hundred pages. It is an amplification of his former work on the Restoration of Lost Parts, which we noticed in our Seventh Volume (pp. 386-416), in conjunction with the treatises of Blandin, Zeis, and Liston. The barrenness of our own literature in this department of surgery we endeavoured in some measure to supply by the article just referred to, and in a more recent one on the works of Von Ammon, Baumgarten, Serre, and Mütter (vol. XIX, p. 396-427); but it induces us again to enter at some length into the description of curative measures, which certainly have not received their due share of attention from surgeons at large. We do so the more willingly, as eleven years have now elapsed since the former work of Dieffenbach was published, and during this period he has enjoyed extensive opportunities of testing the merits of his former proceedings, of modifying them, or substituting others which may be preferable. Our remarks must be regarded rather as an analysis or condensation of the statements of the author, than a critical examination of his respective operations, which must necessarily be more fully tested here, before such criticism would be of much practical value. We proceed, therefore, simply to give an account of his present views and operations, merely omitting those points which have been already discussed in our former articles, to which we would recommend the student again to refer.

Plastic surgery is defined as consisting in "the replacement of a lost, or the restoration of the form of a mutilated (verstümmelten) part of the human body." If "imperfect" had been added to mutilated, this definition would have been more correct, as many of the applications of plastic surgery are required for the relief of congenital deformities, which can scarcely be said to be mutilations.

The history of plastic operations is fully entered into, but our first article renders it unnecessary here to follow our author. He considers that the French remain far behind the Germans, and describes Blandin's *Autoplastie* as "an empty compilation, in which the author appropriates foreign property to himself." (p. 317.) Serre's work is not much better, and many important things are unknown to him, while Labat's contains much interesting matter. It is well to know what one great surgeon thinks of another; but we cannot admire the uncourteousness which we remark, not only here, but frequently in the writings of the German when speaking of his contemporaries. Very little ceremony is used with opponents or rivals, while flattery of a somewhat coarse description throws a questionable halo around friends and supporters.

The basis of all plastic operations is, that separated portions of integument unite to fresh surfaces of wounds of other parts, when, by means of a small band, the nervous and vascular communications are kept up, or when the separated portion retains for itself a high degree of vitality. Having before described the consecutive conditions of the flap after its elevation and adjustment, we extract some important remarks upon the various forms and changes in its texture, with respect to various modes of operating:

"The principal efforts of the new formed part are directed towards its limitation or isolation. The transplanted superficial flap contracts concentrically, drawing the borders together, and resembles a rounded hillock. The degree of elevation of the centre and furrowing round the borders depends upon the rigidity or yielding nature of the surrounding parts. The elevation is even level in cases of transplantation into fixed skin; when into the yielding skin of the eyelids, it almost resembles a ball; and remains perfectly flat when into a thin skin, which extends over a flat bone. When the flap is fixed in a raised position, so as to resemble a sort of roof over an opening, its under surface does not become covered by skin, but the two parts either adhere together, or approach each other, and granulations fill up the intervening space; the nose also becomes massive, and its openings only remain open, when the skin falls inwards.

"When the flap is applied upon a superficial fresh wound, it unites both with the borders and the base.

"When the flap is applied to a part covered by skin, as to the stump of a nose, so that the wounded cellular tissue of the flap lies upon the sound epidermis, after the union of the edges the following changes occur: The cellular surface is not covered by skin, but becomes smooth, pale, and receives a fine polished transparent exhaling membrane; the opposed epidermis of the stump receives a similar moist separate covering, and the two surfaces remain for a long time like the two opposed surfaces of a serous membrane—the pleura of the lungs and thorax, for example—separated by the exhalation. When the access of air is prevented, no suppuration occurs, but the cellular surface and that of the epidermis unite together as soon as they perfectly resemble each other. The improvements I have lately undertaken upon noses formed by art, have given me numerous opportunities for pursuing these inquiries.

"If two surfaces of epidermis are brought together, and they are deprived of air, they begin to exhale, and become like serous membranes, uniting, however, afterwards.

"When a serous membrane is fixed outwards, exposed to the air, it is placed in highly irritating circumstances, and changes with difficulty into a dry membrane like epidermis.

"When a mucous membrane is turned from within outwards, it often, in a few weeks, assumes the nature of the external skin, and becomes pale and dry.

"When a flap is perfectly reversed, so that the cellular surface is turned outwards and the epidermis towards the face of the wound, and the edges unite, the following changes happen. At first the flap suppurates, but languidly and thin, (the discharge?) the surface becomes smaller, the borders contract together, it becomes rolled inwards, like an encysted tumour, both the internal surfaces go on as before described, exhale, and remain a long time separated, until the cellular and epidermic surfaces become united together. A contracting process of cicatrization covers the external wounds and draws the surrounding borders so strongly together, that the cicatrix has scarcely a quarter of the circumference of the wound, in the depth of which the flap lies like a ball. But when a flat bone lies beneath the wound, the cellular surface arches itself strongly outwards, and appears like a lump (buckel) covered by skin.

"The most common property of transplanted parts is, that they appear harder, almost cartilaginous, and afterwards become softer, and often decay, a peculiarity which is brought about by changes in the cicatrix." (pp. 322-4.)

For further remarks on the general principles of these operations, Dieffenbach refers to his former work, and we to our former articles, and follow his account of the rhinoplastic operations. His first chapter is on the formation of the nose from the skin of the forehead, and, as our former articles do not contain a detailed account of this, the grand operation of plastic surgery, we think it well, at the risk of being a little diffuse, to give at full length the directions of one who probably speaks after greater practical experience than any living surgeon can boast of.

THE NOSE. The operation for the formation of an entire nose is a great modification of the Indian operation, and of that of Tagliacozzi, Carpue, &c., because, in all their cases a stump remained, and the operations were undertaken to restore the cartilaginous part of the nose. It is described by Dieffenbach as follows:

"*Preparation.* A nose is to be cut out of a piece of leather, the inner side of which is to be covered by a thin layer of plaster; the form of this leather flap must be almost triangular, and under its middle a quadrangular flap is applied, an inch in length, and three quarters of an inch in breadth. This latter part is the model of the septum. The model, particularly when the skin of the forehead is thin, must be almost a fourth part larger than the nose which is intended to be formed. Several small, delicate-bladed scalpels, hooked forceps, a pair of pincers to nip off the ends of the needles, fine insect needles, curved needles threaded with waxed silk, two measures (Spanne) of long thick cotton threads, slips of plaster, small Indian rubber tubes, soft sponge, &c., must be ready. Several practised assistants surround the operator.

"Before the operation is begun the leather model must be placed on the part, and its borders attached around the stump (by the plaster). If its form is satisfactory, the model is so extended upon the forehead, that the part which is to form the septum is laid close to the hair, which has been previously shaved, in order to prevent its introduction into the wounds.

"At the operation the patient is seated, the back of his head supported against the breast of an assistant, who also supports the side of the head with the palm of his hand.

"1. *The wound of the stump of the nose.* The nose is entirely wanting; a round hole occupies its place, and above this the skin lies flat. The knife is inserted near the right internal angle of the eye, and drawn downwards, and somewhat outwards, towards the commencement of the upper lip, where the ala is placed. A second similar incision is made on the opposite side, which passes obliquely outside the root of the nose in the first descending cut. This is exactly the line from which the nose formerly rose. A third penetrating incision is made

across the upper border of the lip, quite down to the bones. The skin between this and the descending incision remains undivided. These three incisions isolate the flat remains of the old nose and the hole in the middle, and remain under the new nose. All the external borders of skin with which the flap of the forehead is to be united must be loosened to the extent of several lines, that they may be readily adapted to the flap.

"2. *Incision of the skin of the forehead.* The knife is inserted near the point of the right side of the model, and, following this, the skin is divided by gently drawing the knife from above downwards, until it arrives at the uppermost point of the right incision. Then the incision on the opposite side is made, but this must terminate a full finger's breadth earlier, so that the skin between the incision on the opposite side of the stump remains undivided, in order to form the bridge. Then the septum is to be cut from the highest part of the forehead, making first the lateral incisions, and lastly the upper transverse one. If this incision were made first the flowing blood would interfere with the formation of the flap. During these incisions the assistant stretches the skin of the forehead by drawing the skin of the temples towards himself. All the corners and angles of the flap must be divided with great care, and before this is effected the loosening of the flap must not be commenced. The upper border of the skin destined to form the septum is fixed by hooked forceps, and separated from the pericranium by drawing the knife freely with its flat surface towards the forehead; the body of the large flap is to be separated in the same manner; and lastly, the bridge is loosened underneath, in order that it may be readily turned.

"3. *Union of the wound of the forehead.* This must take place before the fastening of the flap, partly for the protection of the denuded frontal bone, partly to give time for the troublesome bleeding to cease. The thick edges of the wound between the eyebrows are to be united by several strong points of interrupted suture, proceeding upwards, until stretching of the skin of the forehead commences. Then no more sutures are to be inserted. Then the angles which were produced by the formation of the septum are to be pierced once or twice, and a stitch also placed in each corner between the flaps of the nose and septum. The opening which then remains must be filled with charpie, and strips of plaster brought across the forehead from one temple to the other, to support the sutures.

"4. *Fastening of the flap.* This is effected by means of a number of small insect needles, the ends of which are cut short off. The union must be quite exact in every point. *Some* sutures are also placed between the needles, but *many* are apt to be followed by eversion of the edges and gaping of the intervening space. Lastly, the septum is to be fastened to the upper lip by three very thick sutures, of which the middle one is to be the first applied. The wounded edge of the upper lip is to be curved outwards by means of the index finger placed within the mouth; both edges are to be transfixed, and brought in direct apposition with each other by connecting knots. By this means surfaces of epidermis are prevented from remaining in a corner, and separating the wounded surfaces from each other. The needles are to be snipped short off. The careful pressing together of the partition is now of particular importance, and this is effected by means of a wick formed of twelve threads of thick cotton laid together, which is passed in at one nostril and out at the other, and then generally tied with thick knots. Formerly I stitched the edges of the septum together, and then passed a long strip of plaster around it, but the wick answers better on account of its equable and gentle pressure.

"After the operation is terminated, the nose is to be syringed and cleansed from blood, and light charpie is to be carried under its upper part by means of forceps, in order to prevent any sinking inwards; thick quills with charpie wound round them, or the useful elastic tubes of Zeis, are then to be introduced. The after-treatment will be presently considered.

"*Modifications.* Various complications which are observed when the nose is

totally deficient, render a particular kind of incision of its bone necessary. The lip is occasionally drawn by the process of cicatrization into the large hole in the middle of the face, so as to form two oblique limbs (schenkeln) over the bare and dry alveolar process, part of which is often destroyed by necrosis, and sometimes it is so completely turned up as to resemble a total ectropium. With this there may be great loss of the soft parts of the middle of the face, the hole surrounded for a considerable breadth by fine red cicatrices; and to complete the evil, both the under eyelids may be drawn down to the hole by cicatrices and turned outwards.

"In order to replace such a loss of substance, almost the whole integument of the forehead would be required, and the danger to life after the operation would be greatly increased. The nose itself also would always be too small, even with the largest possible flap; and the flap, from the distance of the sound skin of the cheeks would only resemble a flat roof over the hole.

"In such cases I have adopted a proceeding by which the operation is much diminished, while the secondary ectropion of the upper lip and of the eyelid is removed, or the subsequent healing at any rate effected. Loosening of the skin of the face does this.

"After the incisions for the nose flap have been made in the healthy skin of the face, the upper lip is fixed, and freed from the bone until it can be freely drawn downwards; then the knife is carried upwards under the skin of the face, and this is loosened (demaskirt)." (pp. 327-30.)

In this way, by dividing the subcutaneous tissue, large flaps may be obtained, which may be drawn in almost any direction, and fixed by needle and suture. The mode of their application will be described when speaking of the restoration of lips and eyelids.

When the skin of the centre of the forehead has been destroyed by necrosis of the subjacent bone, the flap must be formed from the side, or one flap from one side and one from the other; or if no healthy skin remain on the forehead, then the skin of the arm must be employed.

Our space does not permit us to follow the author in his descriptions of the various circumstances which may give rise to slight modifications in the operation, and indeed it is scarcely necessary to do so, as no two cases exactly resemble each other, and any surgeon who was thoroughly acquainted with the general principles of the operation would be able to adapt slight changes to the nature of the case. It is well, however, to know that Dieffenbach has succeeded in forming satisfactory noses from the hairy scalp, the connecting bridge being a long narrow strip of the skin of the forehead. The advantages of this method are that very little scar of the forehead remains; the scalp is so firm that it does not contract, and thus a smaller flap is necessary, and the nose remains firmer. The hair is prevented from growing by solution of bichloride of mercury. Without stating anything, however, against this method, the author says he only practices it when the other operations are impracticable.

Partial rhinoplastic operations, or restoration of a part of the defective nose, is effected by transplantation either of the skin of the forehead, or of that of the eyelids, cheeks, upper lip, or of the nose itself. The chief difficulty is in adapting the flap exactly to suit the various forms of defect, and also from the want of correspondence between the new and the old parts. When the ridge of the nose is entirely absent, a furrow occupying its place from destruction of bone, the edges are pared and a proper sized flap cut from the forehead and adapted as before. When the upper part

will apply. When the bony part of the nose is covered with sound skin, the alæ alone being wanting, the forehead-flap must be as broad as in the operation for total restoration, and the septum must be also of the same breadth. The connecting bridge must of course be very long. The model is cut, fixed to the forehead, incisions made and flap separated, and then the bridge is to be carefully separated in the neighbourhood of the corner of the left eye; the union of the wound of the forehead, and adaptation of the flap as before described. When the alæ have been drawn inwards by cicatrices, they may be raised after division of the latter, and afford great assistance in the formation of the nose, as, instead of adapting the forehead-flap to the furrow made in the face by incision, it is merely necessary to pare the edges of the alæ and bring them into apposition with the edges of the flap. When the septum alone is defective, it may be restored by transplanting a long flap, the breadth of a finger, from the centre of the forehead, or it may be formed from the upper lip. As the latter operation is not described in our former articles, we extract the remarks of our author:

"*First method.* Flap taken directly below, and mucous membrane turned outwards. The septum is entirely wanting. The middle of the upper lip is twice cut through, and the small isolated strip, from which the red portion of lip is removed, is united by insect needles with the edges of the wound of the point of the nose. Von Ammon, Liston, and Fricke operate in this way. This method has the great advantage that the flap undergoes no twisting, as it is carried directly upwards. I can prove the great value of this method from personal observation. The everted mucous membrane becomes dry, and assumes the nature of epidermis.

"*Second method.* Flap taken from below and turned. A strip of skin is cut from the whole thickness of the lip. The incision on the right side divides the right nostril in order to facilitate the turning of the flap; this is then loosened from the bones, turned back, and united by means of three insect needles with the wounded point of the nose, after removal of the red edge. The division of the lip is to be previously united by three strong insect needles. . . .

"When the cartilaginous septum remains, the external part alone being absent, I first pare away the edges, and wound the point of the nose; then I cut a perpendicular strip from the corium of the upper lip, and fasten the upper border, the red edge being previously removed, to the point of the nose with insect needles. This method generally succeeds perfectly.

"When large strong cicatrices existed on the lip I have excised them, and formed a septum from the substance of the cicatrix, which restored the form perfectly.

"*Third method.* Septum formed by oblique divisions of the upper lip. In cases of thin long upper lip and small orifice of the mouth, I have endeavoured to avoid further lessening of the mouth by taking the strip of skin in an oblique diverging direction beneath the nostril, loosening it from its attachment, turning and uniting with the point of the nose by sutures." (pp. 345-7.)

Death of the flap is apt to occur in this operation unless it have been so well loosened that the bridge is not much pressed in twisting. It is better even to leave a little gaping at first, and complete perfect union afterwards by division of the bridge.

"*Fourth method.* Immediate union of the point of the nose with the upper lip. I make two perpendicular descending incisions through the whole thickness of

the lip, from the two nostrils to the middle of the upper lip, and at the highest point. These are united by a transverse incision. The upper border of the wound is fixed with forceps, the skin loosened and carried forward as the bridge; then the anterior and inferior border of the point of the nose is wounded, and the flap being drawn upwards, is fastened to the nose by three insect needles. No union of the wounds of the upper lip should be attempted." (p. 347.)

We have given a literal translation of the description of this fourth method, but must confess that we do not perfectly comprehend it. How deep is the upper transverse incision to be? If the two perpendicular incisions commence immediately beneath the nostril, and the transverse incision is at their highest point, what bridge can there be above the upper border of the wound? Conciseness of description is always to be recommended, but it may become a fault; and when carried too far, needlessly puzzle the student.

The septum may also be formed from the ridge either of the old or new nose, bringing down a flap cut from the ridge, and uniting it, after turning, with the point of the nose and the upper lip by needles, and uniting the edges of the wound of the ridge after removal of the flap. This operation is described in our Seventh Volume, p. 403.

Some interesting observations follow *On the means of closing openings in the bony structure of the nose.* In these, as in all the other plastic operations of Dieffenbach, he states he has given not studies upon the dead body, but "real portraits for which living men have sat."

1. Suppose the opening in the nasal bones is of the size of a large pea, the surrounding skin thick, healthy, and inverted. At the upper and lower part of the opening a small wedge is cut out to give the opening the form of a pointed oval, an incision is made around the edges of the opening, and the outer borders separated from the bone all round to the extent of five or six lines; they may then be brought together and united by insect needles and twisted suture.

2. If necessary, incisions may be made at the side of the opening after paring the edges; this facilitates their approximation, and after their union the healing of the side-wounds is effected, scarcely any scar remaining.

3. In case of a large hole in the upper part of the root, of the circumference of a bean, with the edges towards the corner of the eye, and surrounded upwards and outwards by an elevated swollen margin, the thin part of the border is to be surrounded by a semicircular incision; then the opposite border is also surrounded by a convex incision, which runs outwards about four lines from the edge of the opening. The bone of the flap thus formed is separated, so that it may be easily turned over the opening, and its convex border united by the twisted suture with the concave one on the other side of the opening.

4. A somewhat larger opening in the same situation may be closed by transplantation from the forehead. At some little distance from the opening it is surrounded by an incision, and the ring removed only when it is diseased. The skin around is loosened, and a flap, the shape of which is determined by a plaster model, cut from the lower part of the forehead, and adapted to the wound around the opening.

5. The skin of the cheek should only be taken to close these openings

when they are in its immediate vicinity; otherwise it is better to take the flap from the forehead. After the incisions of the edges, a round flap is taken from the cheek; a little external to the ala nasi is the most convenient spot; the flap is fixed and the edges of the wound of the cheek approximated, by drawing forwards the skin of the side of the face.

6. In some very rare cases, as when a very large opening exists close to the eyelid, and this is drawn towards the opening by cicatrices, the skin of the eyelid may be used as the flap, and at the same time ectropion consequent upon its traction towards the opening be removed.

7. The flap may be formed from the substance of the lip. A large hole may exist in the under part of the root of the nose, and beneath the edge of the orbit, so that the internal structure of the nose may be seen. Cicatrization has drawn the upper lip, and often the whole corner of the mouth into the opening, the mouth being thus nearly doubled in length. This must be rectified before closing the nasal defect, in order not to disfigure the mouth more for the benefit of the nose. This operation is described in successive steps by division of both lips, their reunion forming a new angle of the mouth, and then the remaining part of the upper lip used as a flap for closing the opening in the nose; but the wording is so careless and contradictory, that we have in vain endeavoured to comprehend exactly how the operator is desired to proceed.

The elevation of a nose which has sunk in may be attempted when the bony parts have been destroyed, without injury to the soft parts, on their inner surface. The latter must not be drawn completely inwards, nor their under support be entirely wanting, or the nose would resink after its elevation. The operation is commenced by dividing the nose throughout its whole length by four incisions. Two of these incisions, which commence at the internal corner of the eyes, and pass to the posterior inferior border of the alæ, divide the skin and cartilage at the limit between the side of the nose and the cheek. The other incisions have the ridge of the nose between them; the point of the septum remains hanging together. When the base of the nose has been loosened from the bones, the inner border of the edges of the upper incisions are to be slightly pared, and the outer borders of the under, to assist in the proper replacement of the nose. The two upper incisions are then united by the twisted, and the two under by the interrupted suture, two tubes being placed in the nostrils. The neighbouring skin of the cheek is then, by means of two long needles carried across it and two small leather straps, pressed together, and the nose thus more pressed forward. The sutures are afterwards removed, the inner covering of the nose cauterized for a long time, and elastic tubes worn until the nose has no tendency to sink.

In cases of *flattening of the nose* after total destruction of the bones, one or more flaps may be united one over the other at different times.

When flattening occurs from relaxation of the skin surrounding the base of the nose, portions may be removed, and the reunion of the edges remedies the defect.

Elevation of the flattened ridge may also be effected by making an incision from between the eyebrows to the point of the nose in a direct line, loosening the edges, and bringing a flap half an inch broad, cut from the middle of the forehead, which is united by needles with the edges of the

nasal wound. The flap must be well supported underneath, or it will sink inwards.

A singular method of elevating the whole nose is described as very useful and often practised. Two small splints of stiff leather are placed at each side of the nose, and these, with the nose also, are transfixed by long thick insect needles. The heads of these on the left side are drawn to the splint, and on the right the points are cut off to leave about a quarter of an inch of the needle projecting. These are then surrounded by threads to prevent their escape, and press the splints together. After some days, when the punctures suppurate, the needles are bent to the shape of a ring, and being fixed by pincers, the nose is thus pressed more outwards: afterwards the needles are still more bent. When they have remained two or three weeks and produced much surrounding swelling, the needles are removed easily by clipping off their heads. Then the skin is cleaned, new splints applied, and the nose transfixed in two or three other spots. In the mean time the first punctures heal, and their orifices are drawn a little inwards, and the nose is held together by the contraction of the thickened cellular tissue, into which their canal is converted.

Small defects of the point of the nose, and separation of the integumental portion of the septum, may be easily remedied by paring the edges, loosening the surrounding skin, and then bringing the edges together and uniting by twisted suture.

Defects of the alæ nasi are very common, particularly in young persons. When slight, paring and subsequent approximation of the edges of the cleft or fissure, is all that is necessary. If the symmetry be thus destroyed, a similar operation may be performed on the sound side; and then if the ridge be rendered too prominent, a small portion of the cartilage may be removed where it unites with the integumental portion of the septum.

Some modifications of this operation, to suit particular cases, are described, but our space will be better occupied by more important matter.

Various *deformities of the nose* may be removed by subcutaneous operation, as division of cicatrices, which draw the nose inwards; this is easily done by a small scalpel inserted up the nostril. Depressions of the cartilaginous ridge of the nose are also treated by inserting the knife into the nostril, loosening the skin around the depression, and dividing the cartilage transversely with a tendon-knife, filling the space up afterwards with charpie. Depressions of the alæ also may often be remedied by similar or by crucial incision of the cartilage, without injuring the external skin, keeping the nostril stuffed with charpie until the parts have healed. When the nose is crooked from obliquity of the septum, the deformity may be remedied by the removal of a long oval portion of the cartilage of the overhanging part of the septum. When the obliquity of the nose is congenital, or arises from wounds or disease, it is remedied by raising the skin of the ridge at the point between the bones and cartilage, transfixing it, and with a small concave knife dividing both cartilages quite down to the cheek, where they join the bone and the cartilaginous septum also—of course beneath the skin. Charpie is then placed within the nose, and the nose is drawn to the side necessary to straighten it by means of straps of plaster passing from the temple over the nose to the lower jaw. This

is to remain long undisturbed, and replaced in the same manner after removal. Generally the healing and removal of the deformity are rapid, and the author has never seen any ill effect follow this operation. It certainly appears to be a simple method of removing a great personal defect.

The after-treatment. Generally speaking a local and constitutional antiphlogistic treatment is now found most successful after plastic operations; contrary to the formerly received opinion, that a stimulating treatment was necessary to elevate the diminished vitality of the part. This is only necessary in weak persons, when the parts remain flaccid, cold, and deficient in blood; warm aromatic applications and a covering of flannel may then be used to sustain the heat of the parts. More often a congestion of the flap leads to its death, the blood entering more easily than it can escape. Cold applications must then be used, and leeches applied upon the bridge, the surrounding parts, and even the flap itself. The bleeding is encouraged by fomentations, and sometimes a small portion of the edge of the wound is removed, and free bleeding generally follows. General bleeding is advisable if there be much constitutional excitement. If nevertheless gangrene should follow, stimulating and aromatic applications must be used, granulation of the inner surface of the nose encouraged; and when the healing is completed, the defect must be again remedied after the methods before described.

Some *after-operation* is almost always required, even when the first has been most successful, in order to perfect the produce of art. Small prominences must be pared off; small depressions elevated; puffiness removed by superficial cauterization; central elevation of the flap lowered by excision of a small flap from its highest part; the connecting bridge, as soon as the nutrition of the flap is secured, is to be excised by two elliptical incisions, and the edges united by fine insect needles. In this manner all bridges of transplanted flaps are to be excised, unless the portion of skin is required to cover some other defect.

An operation is described as the favorite of the author, in lengthening and improving the form of new noses, by making two diverging incisions from the root, where both commence, along the sides of the ridge. The long tongue-shaped wedge thus formed is loosened from above, so that it only remains attached below. The skin of the side of the nose is then loosened, and the edges brought together over the ridge. The wedge is pressed downwards to the point of the nose, which is then long pressed outwards, and the wedge united with sutures.

The frontal cicatrix is best thickened or condensed by the application of lead lotion and a compress. Ointments thin the epidermis, and do harm. It is often advisable to lessen the cicatrix by excision of long portions of the shape of a myrtle leaf, or entirely to extirpate it, only taking away so much at a time that the edges can be reunited. "In young girls I have generally removed it completely by repeated operations, so that no trace of any operation having been performed on their forehead could be discovered." (p. 372.)

Dieffenbach has only once attempted the support of a sunken nose by a metallic plate, and in that case without permanent success, having been obliged to remove it, and transplant a flap from the forehead.

As we have already, (vol. VII, p. 390-3,) when commenting on the operations of Taliacotius, and Von Gräfe, described in general terms the method of restoring the nose by a flap taken from the arm, we do not think it necessary to enlarge here upon this operation; but it may be interesting to know the opinion of the author, after his enlarged experience, upon the relative value of the two modes of forming the flap, the one from the forehead, the other from the arm. He says, each have their advantages and disadvantages, but generally the former is far preferable, and the latter only to be adopted when the former is impracticable.

The advantages of the forehead flap are, that it is firmer and thicker, and the nose consequently is less liable to shrivel—the greater proximity of the forehead renders the union of the flap more certain—the operation is more easily performed—the patient remains much more comfortable during the healing process—and the healing is more rapid. As a *per contra* it must be allowed that the cicatrix on the forehead is a disadvantage, and after the removal of this, the alteration in the countenance produced by the approximation of the eyebrows. Some danger also may be apprehended from the denudation of the forehead when a very large flap is required.

The advantages of the other operation are, that the cicatrix of the forehead is avoided—the countenance is unaltered—and its applicability when the skin of the forehead is diseased. The disadvantages are, the greater difficulty of its performance—the tormenting situation of the patient after the operation, during the progress of union between the arm and face—the annoyance of pus flowing over the face, and drying by evaporation, when the flap is applied immediately after its elevation from the arm—the limitation of the method to the restoration of the anterior part of the nose—the diversity in colour and thickness between the skin of the face and that of the arm—the frequent failure in the union of the flap—and, lastly, the diminution and withering of the nose from resorption of the subcutaneous layer of fat.

The chapter on nasal plastic operation is excluded by a detail of the case we have noticed, (vol. XIX, p. 399-400,) and some anecdotes descriptive of the joy and gratitude of patients who have been restored to their friends and society after years of solitude and distress. One, some years after the operation, wrote, "I have lost the whole of my large fortune, but I am happy, *for I have a nose!*" Lavater says, a beautiful nose is worth a kingdom, and probably no one knows the value of his own until he has lost it.

The thirtieth chapter is occupied by the methods of *dilating and perforating the nostrils*, when they are closed or adherent. The evil may be congenital, or the result of disease, and may be present in natural as well as artificial noses. Simple or conical incision; or removal of the cicatrix or other substance closing the nostril by excision; the application of caustic; and wearing metallic or elastic tubes, are all that is required.

THE EAR. Otoplastic operations are described in chapter 31. Complicated wounds or lacerations of the external ear are common. The edges are to be equalized if fresh, and pared if they have become covered with cuticle, brought into apposition and fixed by suture. The replacement

of an entire ear cannot be effected, the new ear remaining in a formless mass, but parts are successfully restored as the upper ear, the helix, and the lobule.

The edges of the defective part are pared, a flap of the requisite size raised from neighbouring skin, and fastened by suture, oiled lint being placed beneath the flap behind the ear. When the flap is united, and cicatrization of the edges commences, a rather larger strip of skin than the defect requires is cut from the skin of the head, which gives a round form to the border. Interposed lint prevents adhesion.

When the restoration of the lobule has been required, it has generally been lost by burning, and the remains united to the cheek by a common cicatrix. The method by sliding or displacement is here practised—a descending incision is made from the free border of the ear, and then a second on the opposite side. The two inclose between them a portion of skin, one third broader than the healthy lobule, and are united below by a curved incision of the form of the lobule, and then the flap thus formed is separated from its under attachments. The healing of the wound is effected by loosening the edges, and bringing them together, and this is facilitated by cutting out small pieces of integument, so as to make the ends of the wound pointed. The wound heals by the first intention, and the flap becomes covered by cuticle. Small irregularities are removed by superficial paring, and the lobule generally proves to be a good one.

THE MOUTH. We are now introduced to the operations *Chiloplastik* and *Stomatopœsis*, or those performed for the restoration of the lips and mouth when destroyed, or the removal of their deformities. The general remarks on the union of the wounded lips are worthy of attention, and we extract them accordingly.

"In no wounded part of the body is the twisted suture so necessary as in wounds of the lips, where it especially offers three great advantages, 1st, It brings the edges of the wound and the substance between the needles also into close contact;—2dly, it suppresses the most violent bleeding;—3dly, it so fixes the parts during the healing process, that eating and speaking do not do much harm. The interrupted suture, on the contrary, allows after-bleeding, between the edges, and swelling and separation from each other of the interstitical substances, and does not fix the lips.

"The union is effected by insect needles and thick cotton threads; the larger the wound, the stronger the binding; when there is great loss of substance, the longer and stronger must be the needles, in order that in their application they may reach from one wounded border to the other. Here they must be rather thick, in order not to bend from the tension of the edges. The needles are applied during the most violent bleeding from the edges, as a means of suppressing it; whereas frequently in other situations it is necessary to wait until the wound bleeds no longer. The coronary artery is never to be tied, because the ligature prevents union, and an insect needle and twisted suture, applied to the spot, instantly suppress the bleeding." (p. 398-9.)

The needles are passed three or four lines from the edges of the wound, and close to the mucous membrane. We have described this little operation at length, as the twisted suture, in many of its applications, is regarded by Dieffenbach as a *specialité* of his.

In the following chapter, the 33d, we find the operation for *Harelip.*

Our author has operated upon a *thousand* cases, and says that while union has taken place at every age, from a few days after birth into extreme old age, it is better to wait until dentition is accomplished, as when performed very early, the cicatrix is apt to yield as growth advances. It is not necessary to follow the details of this operation, and its modifications to the different forms of harelip, as we find nothing not generally known and practised here. Dieffenbach prefers the scissors to the scalpel for paring the edges, and uses much thinner needles than are in fashion here, as he says failure frequently follows the inflammation and suppuration set up by thick needles. Instead of the waxed silk often employed, he uses thick soft cotton thread, as he says the former frequently causes the needles to break through the skin, and leave ugly cicatrices, like pock marks, upon the lip.

The operation for *ectropium of the lips* is one of the most difficult, but successful, of plastic surgery. The defect is always connected with contraction of the external skin, and generally is the result of burns; sometimes of severe herpes.

In a simple case of ectropium of the under lip, where a small portion only of the lip is turned downwards, the operation resembles that of cancer of the lip, a wedge being cut from its middle, with the point directed to the chin, loosening of the edges of the wound, and application of needles. The external skin is of course left uninjured here, (although the author does not say so,) the wedge only including the mucous membrane and submucous tissues.

In the more advanced stage the lip is no longer a screen covered by skin and mucous membrane, but merely a surface of mucous membrane, drawn down to the chin, and covering it like a "red half moon," leaving the teeth and under jaw exposed. By incisions from one corner of the mouth to the other, which descend to the chin, and loosening from the bones, a portion of the everted lip is extirpated of a rounded-triangular shape, like the lower half of *the spade* of cards cut across. Then from the two upper corners horizontal incisions are made and the sides loosened. They are then brought together, and united by insect needles. If the surrounding skin be very firm, lateral incisions require to be made to the edge of the jaw, to facilitate the sliding or displacement of the integument.

Sometimes, by cicatrices extending from the breast, the lip is everted, and brought below the point of the chin. In such a case, Dieffenbach has operated successfully as follows:—

"First, the cutting of a half-moon shaped flap from the red skin. Commencement and termination at the angles of the upper lip. Loosening and turning up of this flap. Excision of two small wedges from the present corner of the mouth, and union by sutures. Carrying over of a cross slip of plaster, by which the outer surface of the lip flap is pressed against the inner.

"Then two narrow incisions through the hard cord-like cicatrix, from without downwards to the sternum. Loosening of the borders. Bending of the head backwards. Direct approximation of the edges is thus effected. This pyramid, cut from the cicatrix, moves upwards, and is fastened to the side edges, and the inferior point of the wound can also occasionally be directly closed. Strips are afterwards taken from the centre of the pyramid, and the edges united, and thus greater elevation obtained." (p. 414.)

We have given this description in the author's own words, because the operation appears to be a valuable one, and also because the directions are so concise that some misconception might easily take place.

Ectropium of the upper lip arises from similar causes, and operations upon similar principles remove it.

When the corner of the mouth is turned outwards, a wedge of the mucous membrane is excised, the base at the corner of the mouth, the apex towards the cheek, and the edges are united by twisted suture.

In a case of total ectropium of both lips and both corners of the mouth, the operation just described is performed upon the corners, and then the upper and lower lips are replaced by the usual methods.

The double lip is remedied by excision of the false inner lip, with fine scissors, and union of the wound by a number of small sutures.

In our Nineteenth Volume, (p. 404-8,) and Seventh, (p. 407-8,) we have entered at some length upon the subject of operations for the restoration of both upper and lower lips, in the former commenting particularly upon the methods of M. Serre, and in the latter upon those of Dieffenbach himself. It is not necessary, therefore, to retrace our steps, especially as our author does not appear to have modified his former proceedings.

For a description of the means of enlarging a contracted mouth, we can also refer to vol. VIII, p. 409-10, and vol. IX, p. 402-3. Since his former publication, the author has repeatedly performed this, *his own* operation, with success, and upon persons from the most distant countries.

THE CHEEK. The thirty-seventh chapter contains the meloplastic operation, or that by which a greater or less portion of lost cheek is renewed. We must again refer to the interesting case of Dr. Mütter, detailed vol. XIX, p. 403-4. In vol. VII, p. 410, will be also found a notice of Roux's operation. It is necessary, however, to recur to this subject.

Dieffenbach observes that the generality of so-called meloplastic operations which have been recorded, do not deserve this title, being merely restoration of a part of the cheek by displacement of surrounding skin, or closure of openings between the cheek and nose, or in the nose itself. He says—

"When the middle part of the cheek is defective, the mouth is generally drawn obliquely outwards to double its length, and its corner is close to the malar bone. Here, as described when treating on the restoration of the lips, both lips are divided perpendicularly at the spot where the corner of the mouth is to be made; the wounded edges of the proper mouth are brought together by sutures, the displaced part is loosened round about from the bones, the mucous membrane taken away in order to obtain bleeding edges, and insect needles applied to effect union.

"When the defect of the cheek is of greater extent, but the corner of the mouth less drawn, the mouth is first restored, and its corner made smaller by excision of the borders. Then the borders of the defect are excised, and stitched with thick needles and thread; at the border of the nose a deep incision is made, and a second, in the form of a half-moon, on the opposite side, both being of such a depth that the middle can be united.

"When the corner of the mouth is drawn upwards into a great defect, and the under eyelid is drawn downwards as a total ectropium, I have made use of the superfluous part of the lip to close the under part of the defect; I have

ing the borders of the cheek, and stretching them greatly after the lateral incisions, and several times I have turned the whole everted lower eyelid, covered at the time with healthy epidermis, into a wedge-shaped flap, have brought this down, and so completely replaced the lost substance. The eyelid was then restored by integument from the temple.

"In other cases where the defect was nearer to the nose, and where three fingers could be passed through it into the mouth, when the nose was large, and the defective ala bounded the opening, I have loosened broad flaps from the whole side of the nose, and have obtained substance for the most external part by sliding the flaps, or by lateral incisions, and have united the nose and cheek flaps by the twisted suture, and completely replaced the nasal defect from the forehead.

"I have frequently closed large openings, particularly in the central part of the cheek, by circular incisions at a considerable distance from the borders of the opening and loosening of the flaps, which commence by obtuse points, and terminate in a broad base. These strips are bound spirally with each other, and with the surrounding parts, upon the surface, in the form of a small circle." (pp. 431-2.)

The author has transplanted the soft parts from beneath the lower jaw and chin, to restore these defects, but has never used the skin of the neck for this purpose, as the danger of denuding the neck is too great, and the distance is as great as from the forehead. The integument of the arm could be used, but only when it could not be elsewhere obtained.

Most of these operations require to be performed in successive steps, and often some months must be allowed to intervene between each operation, and no new one attempted until cicatrization is perfectly accomplished. This is especially the case in the extreme deformities following malignant ulceration, where not only the half of both lips, but the *ala nasi*, on the affected side, are destroyed, with the whole cheek. The alveoli are also wanting, so that the tongue is freely exposed. The border is thin, and adherent to the bones; and often ectropium of the under eyelid is also present, the jaw being also fixed by contraction of the cicatrices, and of the masseters, or perhaps from true anchylosis. In such cases, by repeated operations, after months or years, first freeing the jaw by division of the masseters, &c., and then replacing part by part, after the methods we have described, successful results have been obtained.

THE PALATE. We now come to the 36th chapter, on the palatine suture, (Gaumennaht,) but after the lengthened notice we gave in our former article on the operations of Mr. Mütter and Mr. Fergusson, (vol. XIX, pp. 412-16,) it is not necessary to return at any length to this subject. We would, however, remind our readers of the valuable paper lately read at the Medico-Chirurgical Society, by Mr. Nasmyth, on the mechanical means of relieving palatine fissures. In some cases the mechanist can do more than the surgeon, in others the reverse holds good, while each might assist the other more frequently than they do.

In cases of small holes, or openings in both the soft and hard palate, Dieffenbach employs with great success a concentrated tincture of cantharides, with which the borders of the opening are pencilled several times daily. Inflammation and granulation of the edges are followed by union, while if the potassa pura be used, a portion of substance is lost, and the

granulation is not sufficient to close the opening, which remains larger than before. Larger openings are, of course, treated by paring the edges, and union by suture.

In closing fissures of the soft palate, leaden wire is said to be far preferable to silk, and much more easily applied. It can be drawn sufficiently tight to keep the wounded edges close together, while silk, if so drawn, would cut through those delicate textures.

Adhesion of the velum palati to the posterior wall of the pharynx, causes great suffering from stopping the communication between the nares and air-passages, deafness from closure of the eustachian tube, &c., and therefore, although the operation is very difficult, the adhesions must be freed by means of a long scalpel, making a transverse incision, about half an inch below the adherent border of the velum. The edge is fixed by a hook, and drawn from the wall of the pharynx. Then a lancet-formed knife, the flat surface of which is curved, is used and directed upwards, to loosen the velum, the separation of which is completed by scissors, also curved upon their flat surface. The upper adhesions are destroyed by passing a blunt curved iron instrument, like a very small spatula, along the inferior nares. This operation would be rendered unsuccessful by a fresh adhesion of the parts together, unless sutures were applied. A ligature is prepared, with a small curved needle at each end, and with one of the needles the velum is transfixed a few lines from its edge, and the needle brought out at a high point, on the anterior surface of the palate. The other needle is used in the same manner, the ligature being passed a short distance from the side of the other. Then the ends of the thread are tied together, taking care that the edge of the velum is left about half an inch distant from the palate.

As we have referred to Mr. Nasmyth's paper on the mechanical treatment of palatine fissure, it may be well to state that Dieffenbach considers that, in general, all mechanical means for closing openings, or fissures of the velum are not only useless, but injurious and dangerous. With regard to those of the hard palate, wearing anything between the edges of the opening, gives relief for the time, but generally causes enlargement of the opening, so that if the size, or other circumstances, render an operation unadvisable, it is better to cover the palate with a gold plate fixed to the teeth. Of course this would be left to the dentist, but if no such person be in the neighbourhood, any mechanic could make such a plate if the surgeon took a model of the palate in soft wax, harden this in cold water, and upon this make a cast in sulphur, or plaster of Paris. The gold plate, formed upon this cast, would form the artificial palate, and be fixed by gold wire around the back teeth.

In cases of holes in the palate, the edges of which are so callous that an operation would be unsuccessful or impossible, the opening may be stopped by wearing a double piece of Indian rubber, without the danger of its enlargement. Two pieces of Indian rubber, of the thickness of thin pasteboard, are cut about four or five lines larger than the opening, and between them, in the middle, a small round piece of the same thickness is laid, and these three layers are transfixed, and sewed together with waxed thread. One plate thus is made to lie on the anterior, the other on the posterior side of the palate, and the small

middle strip in the openings, with the edges of which it is not in contact, as it is smaller than the opening. When the patient wishes to insert this obturator, he softens it in warm water, squeezes its layers together with a pair of forceps, and passes it through the opening, standing with widely open mouth before a looking-glass. It is removed once a week to clean it, or to apply a new one. When the opening is so small that some hope of a closure remains, the edges should be pencilled with tincture of cantharides. We have lately applied this elastic obturator with most satisfactory result upon a gentleman who had undergone three unsuccessful operations, by paring the edges and applying sutures. We took a model of the palate in wax, and upon this made a plaster cast. This saves a great deal of unpleasant manipulation in the patient's mouth, and an exact fit may be easily obtained. Our friend was quite delighted with the result, and his wife amuses herself by making the plugs, so that with the cast they are quite independent. It is really surprising how much happiness may be conferred upon a family by so simple a contrivance. How, then, can a man practise with a clear conscience who does not keep himself on a par with the knowledge of his age? We think it is Dr. Baillie who says, "In other professions ignorance may be *folly;* in ours it is *crime.*"

EYELIDS. The blepharoplastic operations, or those upon the eyelids, occupy the following chapter. In our Fourth Volume, (p. 483,) Von Ammon's operation is described and illustrated by a woodcut. Some of Dieffenbach's operations, particularly for the cure of ectropium, are referred tó in vol. VII, pp. 404-7, and also illustrated by cuts. There are, however, some points in his present volume which require further notice.

These are the most difficult of the operations of plastic surgery. A nose may be considered a good one when it is like a nose, although it be too large or too small—a mouth is a good one when the lips cover the teeth and the saliva does not escape; but the eyelid must be exactly like the lost one: it must be moveable, and close the opening between the lids and cover the eyeball, when the operation is considered to be of any value.

In *wounds of the eyelids* Dieffenbach insists strongly on the advantages of his fine insect needles in producing union by the first intention. A sufficient number must be applied to effect exact apposition. If the edges do not correspond, they must be made to do so by the use of fine scissors. If a large piece of skin be lost, and the edges cannot be united, subsequent ectropium is avoided by making an incision a quarter of an inch from the edge of the wound, which allows the edges then to be united. Opening of the lids during the healing process to be prevented by a strap of plaster, carried over them both. About the third or fourth day the needles are to be carefully removed, and the adherent wound supported by strips of plaster, to prevent reopening. If the needles are properly applied, no scar remains.

The operation for coloboma—a perpendicular cleft in the eyelids, with hard edges—is the harelip operation in miniature. Exact approximation of the edges by careful application of the needles insures the cure. Cold lotions must be used after the operation.

Tarsorhaphie, or the operation to diminish the size of the opening

between the eyelids, is required when this is enlarged by surrounding cicatrices. Von Walther first proposed it. It consists in the union of the unnaturally elongated part of the opening, the edges of which are fixed by hooked forceps, and cut away to the breadth of half a line with a small scalpel, the two incisions meeting over the commissure towards the temple. Union is effected by insect needles. The most anterior suture, the nearest to the eyeball, should be a fine stitch. If cicatrices have caused the disease, they must be first removed, as before directed, and then tarsorrhaphie performed.

Lagophthalmus, or hare's eye, is a condition in which the eyelids cannot be perfectly closed, so that a part of the eyeball is seen through the opening. If produced by small superficial cicatrices, these must be excised, and the edges of the wound left united, as before directed. When the cicatrices are transverse, he employs subcutaneous division in several spots of the whole upper lid and its cartilage, which is then drawn down and fixed during healing by strips of plaster. A long hard perpendicular cicatrix which shortens the middle of the lid is excised by long elliptical incisions (), and the union by needles draws the lid down. Division of the levator palpebræ is sometimes required.

Blepharoptosis, or falling of the upper eyelid, depends upon a loss of balance between the actions of the orbicularis palpebrarum and the levator palpebræ superioris. It is removed either by a subcutaneous division of the orbicularis, or by shortening the depending lid by the excision of a horizontal portion of its integument. The wound may be united by sutures, or, if the skin be very lax, by granulation.

Ankyloblepharon, or the union of the edges of the lids with each other, and *Symblepharon*, or the union of the lids with the eyeball, or a complication of the two may be congenital, or the result of inflammation. We need not describe the mode of dividing these adhesions; the operation in the first case is generally successful, in the second only when a part of the ball is adherent, and the success in both cases depends principally upon preventing subsequent reunion.

We find nothing new with regard to entropium, and as we have before, as just stated, given the author's operation for ectropium, we pass to the *blepharoplastic operations proper*, and extract the author's present mode of forming the under eyelid from the integument of the temple:

"1. *Wounding of the base.* First a semicircular incision is made between the defect and the eyeball, through the free conjunctiva; the edge is fixed by fine hooked forceps, and loosened as far as the fold (or spot where it is reflected). Then a small pointed scalpel is inserted at the internal canthus, and carried obliquely downwards until it reaches below the malar bone. A perfectly similar incision is made from the external canthus to the same point. The flap of skin thus isolated has the form of an inverted pyramid, the base of which is beneath the eyeball. The upper edge is fixed by the forceps, smoothly separated (abpräparirt) from above downwards, and afterwards carefully applied. If the inferior portion of the orbicular muscle is not destroyed, as in ectropium, it is to be carefully preserved.

"2. *Formation of the flap from the temple.* The skin close to the external commissure is divided, and the incision carried almost horizontally, but at the same time somewhat obliquely downwards to the temple, so that the flap above may be a fourth part wider than the upper part of the defect; the knife is then brought

obliquely downwards, approaching somewhat more to the border of the defect. The incision ends below, opposite the point of the pyramid. Then the flap is loosened from above to below (holding it with forceps) by flat and even strokes of the knife, so that no inequalities may be made. As soon as the bleeding has perfectly ceased we commence with—

"3. *The laying down and stitching of the flap.* This is pushed from its situation to its new seat, and the pyramidical wound covered with it; the whole anterior border of the flap is then united from below upwards, with the wounded border of the facial integument, by a considerable number of insect needles and twisted suture, the flap being thus somewhat drawn up, so that its upper border may jut outwards: the highest corner must be exactly united by a stitch with the internal canthus.

"4. *Fastening the conjunctiva.* The halfmoon-shaped cut border of the conjunctiva is then so united with the upper border of the flap by a quantity of fine interrupted sutures, that the knots may lie upon the outer skin." (p. 496-7.)

The dressing consists in filling the side wound with fine charpie, and then carrying long slips of plaster over the side of the face to the opposite side of face and head, preventing stress upon the sutures by previously applying to each side of the united wounds fine linen, folded six times into a long compress, a few lines broad. Antiphlogistic after-treatment. The dressing very carefully removed, commencing about the third or fourth day by removing here and there a needle which presses, or is surrounded by redness, or where the union is complete. The next day all is removed, and fresh strips of plaster applied. The conjunctiva sutures must often be removed before the others.

Elevation of the flap, which occurs a few days after the operation is performed, is counteracted by the combined cicatrization of the wounds of the temple and face, the contraction flattening the flap. If the orbicularis remained intact, the lid is moveable, and the eyelashes alone are wanting.

The upper eyelid is formed precisely in the same way as the under, carefully avoiding the supra orbital nerve.

Partial is very much more simple than total restoration. It is the simplest application of the operation, by sliding or displacement.

Transplantation of the eyelashes. It has been established by experiment that strong hairs, freshly plucked, take root when they are inserted into small oblique punctures, and protected by slips of plaster. The hair must be strong and young, not just about to fall away. Grayness of the hair is of no consequence, as it is the age of the hair, not of the individual, that is to be considered. Dzondi was the first to apply these physiological facts in practical surgery, planting a row of ciliæ upon an artificial eyelid. Dieffenbach does not appear to have followed his example, but he says, to increase the satisfaction of the patient in a case of successful blepharoplasty, the necessary quantity of ciliæ might be plucked from the other eye by forceps, and inserted in small oblique punctures, half a line in depth, made along the border of the lid, the parts being then covered by fine strips of plaster.

Lachrymal fistulæ. Here is the title of the 48th chapter:

"DACRYOCYSTOSYRINGOKATAKLEISIS"

or healing lachrymal fistulæ by transplantation! This operation simply consists in paring the edges of the fistula, loosening the borders, and

assisting the necessary integumental displacement by lateral incisions. Insect needles and twisted suture of course complete the operation. We have seen many severe and obstinate cases of lachrymal fistulæ, but never one which did not close soon after the patency of the nasal duct was re-established. Cauterization or simple paring of the indented edges of the facial opening is occasionally required, but when once the natural passage of the tears is established, little else is necessary, as spontaneous closure invariably follows. This leads us to make a few observations on the means of reöpening the nasal duct, as nothing can be more unscientific than the proceedings still adopted by the vast majority, both of English and continental surgeons. What is the disease? Simply a stricture or closure of the nasal duct, causing a retention of tears in the lachrymal sac. The consequences are, the flowing of tears over the face, with subsequent irritation and excoriation; or inflammation and suppuration of the sac, and an opening on the face, through which the pus and tears escape. Now, putting aside palliative measures, the only means of curing either of these conditions is by keeping the nasal duct freely and permanently open, and nineteen out of twenty surgeons, at least, do this by passing a style through the sac into the duct; to do this, making an opening in the face, if the disease had not done so. The patient is obliged to wear the instrument constantly, and a most unsightly thing it is on the face of a lady. The whole process is unscientific and unsurgical, not to say barbarous. Let us compare the case with one of stricture of urethra, causing retention of urine. Would any man in his senses relieve such a case by cutting through the perineum as long as there was any possibility of opening the natural passage? And if perineal abscess and fistula had formed, would he attempt to remove the stricture of the urethra which had produced it, by making the patient wear a style in the perineum? Yet this is done every day on the face, with the additional disadvantage of the style being visible, and an annoying disfigurement. Any one with a little practice can open the nasal duct through the natural orifice, as easily as he can pass a catheter along the urethra. The profession is under great obligation to Mr. Morgan, of Guy's Hospital, for introducing this plan of treatment. We have repeatedly practised it with invariable success, and believe that it only requires to be more generally known to be almost universally adopted.

The annexed cut shows the curve of the sound and catheter we are in the habit of using. It differs from that described in Mr. Morgan's work, and if any one will take a skull and the two instruments, he will find that the one here described passes with much less catching against the walls of the passage. We also find this advantage hold good upon the living subject. Of course some little variation of the curve will be required according to the formation of the passage in different patients; but, as a general rule, we prefer the curve here described. The instruments are of silver.

1 Lateral view. 2 Shape of handle.

In an acute case, as a general rule, the opening on the face need never be made. Leeches, &c., are applied, and the passage freed by the probe and injection of warm water by the catheter. In a chronic case of fistula the same mode of keeping the duct open must be employed, either for the cure of the fistula, or, if the patient have worn a style, to enable him to dispense with it. The sound is first passed along the floor of the nose, its point directed outwards, until it lies fairly below the inferior turbinated bone. By turning the flat handle the point is then directed directly upwards, and moved gently backwards and forwards along the inferior surface of the turbinated bone, until a little cartilaginous ridge is felt. This is the edge of the orifice of the duct. The point is glided over it, and then, by depressing the handle, the instrument readily glides along the duct. No force must be used, as the bony structures around are very delicate. If much resistance be met with from an old stricture, the point only must be passed into the stricture, and allowed to remain a few minutes. By repeating this daily, the obstruction is gradually, if slowly, overcome. In one case we found it necessary to pass the instrument daily for upwards of a month. Assistance is also derived from cleaning the passage by the injection of warm water through a catheter of the same size and curvature as the sound. When once the passage is free, the tears descend, and all danger of a return of the disease is prevented by teaching the patients to pass the instrument for themselves. This they soon do with a little practice before a looking glass. The sound may be used once or twice a week, and the catheter daily. It is seldom safe to omit their use altogether, as collections of viscid mucus, or recontraction of the duct, are very liable to occur, and the short time occupied at the dressing table is a very slight set-off against the relief obtained by leaving off the style, or avoiding its insertion altogether. We do not hesitate to say that any surgeon who once practices the nasal method of dilatation, will for ever banish the style from his instrument case. But we must return to the work before us, the succeeding chapter of which is on the closure of openings in the air-passages.

TRACHEA. Small openings in the trachea are treated on the same principles as other fistulæ; larger ones either by paring and loosening of the edges, and union by insect needles and twisted suture. Sometimes lateral incisions, three fourths of an inch from the edge of the opening, are necessary to assist the sliding of the integument. Interrupted sutures are better than the needles when the surrounding skin is very thin; and in this case the lateral incisions must be two inches long. Transplantation of a flap is not to be recommended, as the wound is then larger and more complicated; and the flap, after its separation, falls together between the fingers like a piece of wet paper, unites with difficulty around the thin wounded edges, probably dies, or contracts upon itself, and remains like a little ball at one side of the opening.

SCROTUM. *Oscheoplastice,* or restoration of the scrotum, is seldom required, but it is occasionally from the consequences of extensive burns, or of infiltration of urine. The cases in which Dieffenbach has operated, have been where one side of the scrotum was quite destroyed, and the testicle lying free and uncovered, or more or less covered by cicatrices. The skin of the scrotum is divided around the hard edges of the defect,

avoiding the spermatic cord and the opposite tunica vaginalis; then the hard cicatrix is completely removed, the cellular tissue of the under border of the scrotum loosened, and it must be seen if this be sufficient for the complete covering of the testis; if so, the cicatrized surface of the testis is pared with the knife to the thickness of a sheet of paper, "as one would pare a lemon." Then after the bleeding has ceased, the edges of the skin are drawn over the testis, and the wounds united by a quantity of insect needles. Cold applications are used, then lead lotions; the needles remain until the fifth or sixth day, and then plaster is applied.

In two cases of total destruction of the scrotum, Bürger and Labat have restored it by flaps formed partly from the abdomen, partly from the inner side of the thigh. Dieffenbach does not seem to think the operation likely to be often successful, but says he would rather do it than remove a testicle.

PREPUCE. Seven pages follow on the formation of the prepuce, *posthioplastice,* which we do not think it necessary to analyse. We are more in the habit of removing than of making prepuces in this country. The latter operation can be scarcely ever *necessary,* and we imagine few Englishmen would desire its performance as a *Schönheitsoperation*—Anglice, for ornament. The Germans, however, apparently have peculiar notions on this point, as we have a chapter on the means of improving the form of the glans penis, to which we would refer those of our readers who are curious on the subject, and pass to the more important plastic operations upon the urinary passages.

URETHRA. We must refer to our Seventh Volume for the operations upon urethral fistulæ, where, at p. 399, will be found woodcuts and a description of the treatment of suture; and at pp. 413-14, of that by sliding or displacement, also with illustrations. Seven other operations are described in the present work as means of closing large defects of the urethra by transplantation of skin. In the first, the skin is taken from the scrotum; a large catheter is passed, and the edges of the opening so cut that a square opening remains, the points of which look towards each side of the penis; then a fold of the scrotum behind the opening is raised and cut through to the extent of two inches in length, and the wide bridge thus formed between the incision and the opening is loosened from its base. It is then drawn forwards, and its anterior border united with the wounded edge of the skin of the penis by twisted suture. A bougie is worn to prevent infiltration of urine.

The second operation is not to be recommended. The penis is kept near the left inguinal region, and a flap taken from thence and fixed as before.

The third is an annular transplantation of the prepuce, over openings in the anterior part of the urethra. This explains itself; the outer lamella of the prepuce is alone used.

Fourthly, we have annular transplantation of the skin of the penis, over openings in the urethra, close behind the glans. Dieffenbach has performed this three times with success, once having failed from subsequent erections.

Fifthly, the prepuce may be transplanted over openings of the urethra behind the glans. This is the best operation when the under surface of

the glans has been destroyed by chancres, and the urethra opened, the contraction of the cicatrices having drawn the openings beneath the prepuce.

The two following operations we need not describe: one consists in dividing the prepuce at each side, and using the inferior segment to replace defects of the glans and urethra; the other, in bringing the skin of the back of the penis to its under surface, to close extensive openings in the urethra. One of the peculiarities of Dieffenbach's practice, however, must not be passed over. In order to insure the success of extensive urethroplastic operations, to prevent infiltration of urine, and procure union by first intention, he recommends opening the urethra in its posterior part, and drawing off the urine from this opening by means of an elastic catheter passed into the bladder. It is often seen that when two openings exist in the urethra, one before and the other behind, that the anterior one readily heals by cauterization or suture, provided the posterior one is sufficiently large to allow the escape of all the urine. This is still more certain when a catheter is passed into the bladder. When the posterior opening is large and behind the scrotum, the anterior part of the urethra remains naturally perfectly dry, and the opening then, even if it be an inch long, is readily healed. This points out the means of obviating the difficulty afforded in these operations by the urine; but the opening should never be made in case of small fistulæ, as thus we should heal a small fistula and probably produce a large one. It is only to be done in cases of great loss of substance, to ensure the success of the reparative operation, as the advantage is then on one side, the large opening being closed and a small one formed.

The artificial opening heals most readily when it is made by transverse incision. A large elastic catheter is passed; skin and cellular tissue divided; the under half of the urethra and somewhat more than the half of the catheter divided transversely; the catheter held with forceps, divided; the anterior portion withdrawn; the posterior drawn about an inch exterior to the wound; threads passed through its edges, and by them it is fixed to the soft parts with adhesive plaster. The orifice is stopped by a plug, and the urine allowed to escape every two or three hours.

When the cicatrization of the plastic operation is completed, the artificial fistula must be healed; and this is generally done without difficulty by keeping a catheter in the bladder, passed along the whole length of the urethra, and frequently cauterizing the opening.

The operations for the cure of hypospadias and epispadias, of course resemble those for the cure of urethral fistula, provided the canal be pervious; if not, a canal may be formed by transplantation.

"A fold of skin is seized at each side of the penis, and drawn together over the spot where the urethra is to be formed; then the borders of the fold are drawn together along the whole length by means of transfixion with a straight needle and waxed thread; an incision is then made at each side of the penis, through the skin, to prevent tension; and lastly, the borders of the folds which were tied together are cut away over the sutures, one after the other, with a pair of sharp scissors, and the wounded edges brought exactly together by means of a continued suture, or a quantity of insect needles, so that epidermis is in contact with epidermis. That the new canal may not be exposed to the urine, this is drawn from the old opening by a catheter. When the formation of the canal has

succeeded, and it is also internally covered with epidermis, it must be opened forwards, and continued through the glans. If the glans have a cleft or fissure, this must be healed by the suture, and then the canal, which is still closed anteriorly, must be opened by a sound introduced from behind, and a bougie carried through the glans. The external opening between the glans and canal is closed by pencilling it with tincture of cantharides. Should the glans be imperforate, a small trocar must be passed through it into the urethra; then by means of a leaden sound the internal skinning over of the passage must be sought for.

"The closing the opening through which the urine has passed, by caustics and other means, concludes the treatment. This is impossible without introducing a catheter twice a day from the glans into the bladder." (pp. 540-41.)

VAGINA. This brings us to the chapter on *Vesico-vaginal fistula.* Did our space permit, we should gladly extract the graphic description our author gives of the miserable situation of a poor woman subject to this condition; but as we confine ourselves to his practical and novel observations, we pass over this, and his remarks upon its causes also.

These fistulæ vary greatly in form, size, and situation. Small ones can only be detected by the speculum. The quantity of urine which passes by the fistula varies with its size, and the power of retaining the urine in the bladder with its height. Should the opening not be detected by the speculum, the vagina is to be plugged by a roll of linen, and water, coloured by boiling black cloth in it, is to be injected into the bladder. A black spot on the linen plug will then point out the situation of the opening.

The difficulty in treating these cases arises from the feebleness of the plastic process. The wounded edges of the opening very soon become re-covered by a thin skin, and the vegetative process of agglutination, or even the production of granulations, is seldom established. This is the great cause of the non-success of simple cauterization; and it must be observed that unless the opening is *perfectly* closed, no good has been effected, as the urine dribbles through a small opening in sufficient quantity to keep up the miserable condition of the patient. Thus the numerous cases recorded where a large fistula is said to be cured, only a very small opening remaining, *which is expected soon to close,* are not cures, as the really difficult part of the cure remains to be accomplished.

If *cauterization* be frequently repeated, the parts surrounding the opening become often converted into a hard callous substance, and the subsequent healing by ligature is rendered more difficult. If milder caustics are used still oftener, no change is effected, as the reaction very soon subsides. Should cauterization be obstinately continued in the hopes of exciting granulation, loss of substance follows, and the opening becomes much larger than before.

Sometimes the *suture* is unsuccessful; the threads cut through one edge from the second to the fifth day, and all hang by the other; or some cut through one edge, some the other; the former is more usual. The small fissures made by the sutures do not unite. Sometimes the failure arises from excessive inflammation around the sutures, followed by mortification, and the opening is then also larger than before.

The stories of cures of these fistulæ by vaginal plugging and keeping a catheter in the bladder, are of doubtful authority. Dieffenbach has tried the plan, and found that the catheter does not prevent the plug becoming

soaked with urine, and irritating the parts still more. A catheter cannot prevent the passage of urine through the fistula, and this is the great impediment to union of the pared edges by the first intention. The edges may be perfectly approximated and lymph effused, but its organization is stopped, and the edges are soon seen to be covered by a fine ash-coloured coat of mortified cellular tissue. A single drop of urine between the edges is enough to prevent their union.

A natural cure is sometimes attempted by prolapse or protrusion of the mucous membrane of the bladder. In one case, where this had formed a plug to the opening, but was only partially adherent, Dieffenbach completed the adhesion by the application of tincture of cantharides to the ununited portion.

Whatever operation may be decided upon, the surgeon must first make himself thoroughly acquainted with the situation, &c., of the opening in every position of the patient, as well as if he could have it always before his eyes. A castor-oil laxative is given the day before the operation, and shortly before it an enema of warm water, and the vagina and bladder are to be thoroughly washed by repeated injections of large quantities of warm water. The most convenient position for the patient (and the surgeon) is lying upon the back, and not upon the abdomen, as has been recommended. She lies near the edge of a table, which is placed close to a window; the upper part of the body only moderately elevated; the buttocks project over the edge of a hard horsehair mattress, so far that the patient would slip off if she were not held. The operator sits on a stool between the widely-opened thighs; the knees are bent, and the whole limbs raised and bent back as far as possible by an assistant on each side. Pieces of sponge of the size of a walnut are prepared before hand, and by means of straight forceps the urine is thus repeatedly absorbed. After a long account of all the instruments used by different surgeons in these operations, Dieffenbach quaintly remarks that they "only require clockwork to operate by themselves." The various modes of operating which have been recommended are very clearly detailed, and this chapter may be considered as a complete treatise on the subject; but as our present object is to give an account of the author's practice only, we shall do so as shortly as possible, without following his remarks upon the practice of others.

1. *Operation by common suture.* The fistula of moderate size in the anterior part of the vagina; patient prepared and placed as described; three blunt hooks are introduced, and used one on each side to separate the labiæ, the third to draw the commissure backwards; a sharp double hook is then fixed in the anterior part of the fundus of the vagina, the rugæ drawn level, and the part of the vagina in which the opening exists is drawn into sight. The external border of the opening is probably smooth and hard, and the internal covered by a fine red edging of the mucous membrane of the bladder.

"While an assistant holds the hook which draws the vagina into view, the edge of the opening is seized by a conjunctiva hook; it is transfixed with the point of a fine scalpel half a line broad, and cut all round, so that a little more is taken away from the external than from the internal edge, and the wound has a wider external than internal opening, and is somewhat pointed from before backwards.

The internal circular line runs round the fixed point of the mucous membrane, none of which is to be cut away. The part of the ring now first hooked is loosened in a quarter circle; then the hook is again fixed in the neighbourhood, incision carried on further, and this is repeated all round, and the edge resembles a fine circle of skin. The fixing always answers better with the hook than with forceps; it is only when a strip of the ring is to be removed that this is fixed with forceps, and the point of the knife carried round the opening. Then cold water is injected into the vagina, and this is dried again with pieces of sponge, introduced by polypus forceps." (p. 557.)

Three or four sutures are necessary; curved needles are used. The left edge of the opening is first transfixed at the distance of two or three lines, the needle carried close to the mucous membrane of the bladder, and drawn by forceps so far that the middle of the thread arrives at the opening; then the needle is carried across the opening, the other lip of which is pierced close to the vesical mucous membrane, and the needle brought out so that the two threads hang at the same length out of the orifice of the vagina. The wound is again cleaned and dried, and a brush carried over it wet with diluted tincture of cantharides, to obviate the influence of the urine, and the threads are then tied together tolerably tight with a double knot. The ends of the suture are taken in the left hand, and the tied part drawn lightly forwards, so as to project a little, and then the approximated lips of the wound are both transfixed at the same time, the thread passed and tied as before. This is the middle suture; the posterior is made in the same manner; the threads are then cut short off close to the knots, leaving one end of the anterior suture only, which must be cut off close to the labia. The vagina is again syringed with cold water and dried with sponge, and then plugged with soft charpie, which is to be saturated with wine by placing the point of a syringe in the charpie. A large male catheter is passed into the urethra, the patient laid upon her back, and the open catheter connected with a receptacle placed between the thighs in order to keep the bladder empty. Several times daily the bladder must be syringed out with cold water, which is immediately allowed to run off.

Three days afterwards the charpie is to be withdrawn, and the vagina syringed out with warm water, and the sutures examined by careful separation of the labia. Should they not be visible on account of swelling of the parts, and still the vagina is free from urine and has no unpleasant odour, nothing more is to be done than introduce soft dry charpie into the vagina. The following day the sutures must be brought into view by placing the patient on an operation table and separating the labia, and then the short ends of the two posterior sutures are seized with forceps, divided, and removed; the anterior knot is left till the following day, and then removed; charpie is introduced into the vagina, and then saturated by syringing with infusion of camomile. The urine passes some days longer by the catheter, and about the eighth day its voluntary emission may commence; then the patient goes about and is cured.

"This is the progress of a successful case; but when it does not go on well, and one or more sutures have cut through, which generally happens about the third or fourth day, the nose tells before the eyes what has happened; instead of wine, the urine is smelt, and when the charpie is withdrawn, the urine is detected by its intolerable odour. The vagina is to be cleaned by injections of warm in-

fusion of camomile, the seat of operation examined, and the sutures which lie loose, or have cut through one edge, are to be removed. Sometimes all three sutures have cut through, and must be removed, and the opening appears as large as at the time of operation. If partial union of the opening has taken place by means of one or two sutures, these must be left a couple of days, and the edges irritated, as will be explained when speaking of cauterization. No charpie must be introduced, as it would become moist with urine, but the vagina is to be frequently syringed with the camomile tea, and the urine passes off by the catheter. If the remaining opening does not close by cauterization, the operation may be repeated after the complete recovery of the patient from the first." (pp. 558-9.)

In cases where the fistula is situated very far forwards in the vagina, the sutures are better passed from before backwards, than from right to left. In all other particulars the operation is the same as that just described.

When the fistula is deeply situated, for instance, in the middle of the fundus of the vagina, the difficulty of the operation is of course greatly increased. It is however possible; either—1, by strong traction forwards of the upper part of the vagina; 2, by operating by the guide of the sense of touch; or 3, by the speculum. The first is preferable, if practicable, as the presence of the speculum considerably increases the difficulty of paring the edges and passing the sutures. The operation unassisted by the eyes is very difficult: "it requires much practice and great imagination; one must think himself blind, and have eyes on the points of his fingers." It is not to be attempted if either of the other modes can be employed. The general plan of the operation is that just described; slight modifications are of course necessary, but these are so evident that we need not describe them.

Union of fistula by running suture. This is to be recommended when the fistula is small, the edges soft and yielding, and particularly when it is situated in the anterior and middle part of the fundus vaginæ.

The patient is placed and vagina kept open as before, and its upper wall brought far forwards by a double hook, and the edges of the opening pared to the thickness of a sheet of paper; and the day before inflammation must have been set up in them by concentrated tincture of cantharides. A small strong curved needle armed with waxed silk is passed two or three lines from the edge all round the opening, passing it inwards and outwards, and in and out again, until the opening is stitched all round at the same distance from the edge, and the needle with the thread comes out again at the point where it was first inserted. The thread must lie between the mucous membrane of the vagina and that of the bladder. The edges are then smeared over with tincture of cantharides, and the thread tied. One end is cut short off, and the other brought out of the vagina. After the operation the vagina is plugged with dry charpie.

The advantage of this suture is that it allows neither the entrance nor passage of urine so long as it has not cut through the parts, because the edges of the opening are completely bound together, and even when the operation entirely fails the opening is always diminished in size. It is also very safe, and may be frequently repeated, and the hope of success increases with every repetition, which is not the case with other methods, as after their failure the case is rendered worse than before.

Union by twisted suture. It is much to be regretted that this is only

more readily brought together, and if success does not follow, they are less injured than by the needles. But in large fistulæ of the urethra or bladder in the anterior part of the vagina it is the most successful. The spot must be seen, as the needles cannot be applied by the feeling, or through a speculum.

The patient is placed as usual, and the upper vaginal wall drawn as far forwards as possible with a sharp hook. The edges of the opening are fixed by the hook and pared with a scalpel. A strong insect needle, from which a part of the blunt end has been snipped off, is fixed in the needle-holder at right angles, and by this carried through both the wounded edges of the anterior angle of the wound. The points of puncture and exit of the needle are four or five lines from the edges. Then a thick pitched (gepichten) double thread is twisted round the needle once, the needle is a little curved, then the twisting is repeated a few times, and both ends of the needle are snipped off a few lines from the thread. Then the ends of the thread are given over to an assistant, who therewith draws the fistula a little forward, in order to further approximate the edges of the wound, and facilitate the application of the second needle, which must be placed three lines from the first. This is then surrounded by thread like the first, and as many more needles are then passed as are necessary for the closure of the opening. The ends of the most anterior needle only are left a little projecting, the others are all cut off close to the knots. At the close of the operation a double pitched thread is twisted around the collection of sutures, so as to form a sort of ligature similar to that of a small prolapsus around the part occupied by the suture, and thus retain the urine from the inner surface of the wound. The vagina is then cleared and plugged with charpie, which is left unmoistened in young persons; in strong powerful persons it must not be filled, but several times daily cold water is to be injected into the bladder, and the catheter left there constantly.

The examination of the parts must be carefully undertaken after some days, and the needles withdrawn by forceps. If one needle has completely cut through, it is removed and the others left; but if it have only become loose, a new thread must be twisted round it. If all the needles have become loose, and the urine again passes through the opening, they must all be removed, as no union can be expected from this operation; and by the needles lying longer, and being again surrounded by the threads, the borders of the opening would be destroyed. If only a small opening remains, it must be treated on the principles before described. If the operation entirely fails, the hope of better results at a subsequent period must support us.

Union after cauterization. We need not describe this operation. The hot iron is preferable to any other caustic except the cases to which the tincture of cantharides is applicable. In general terms, for the reasons before stated, Dieffenbach prefers the other modes of operating, but says that the remedy is "a great one, and that in the most difficult cases." In two cases of fistula, close to the neck of the uterus, through one of which fistulæ the little finger, and through the other a thick catheter

could be passed into the bladder, union followed one cauterization. "I scarcely trusted my nose, my eyes, or my fingers, when I found the opening perfectly closed. In a *tolerable number* of cases, perfect healing followed the long-continued application of cantharides ointment, or a repetition of the cauterization." (p. 572.)

Transplantation has also been practised: 1, from the wall of the vagina; 2, from the neck of the uterus; 3, from the bladder; 4, from the labia; and 5, from the skin over the glutei.

Transplantation of part of the vaginal wall can scarcely ever give much prospect of success, and is principally employed to diminish openings which are afterwards cured by other methods. The neighbouring part of the vagina is loosened and drawn over the opening like a bridge, and then a row of sutures of lead wire are passed with palate needles, drawing the needles from behind forwards with forceps. Then the sutures are drawn together until some tension commences. Then incisions at each side are made through the vaginal parietes somewhat larger than the opening, and the sutures are again drawn more closely together. When tension is again produced, the side flaps are loosened as far as necessary to allow of union. Then the sutures are cut off a quarter of an inch from the ring. The after-treatment is the same as after other operations. Probably the large opening is converted into a fissure, and this is to be treated by the suture or cauterization.

Dieffenbach has never followed the example of Horner and Le Roy d'Etiolles by transplanting portions of the neck of the uterus.

Portions of the bladder can only be used when they prolapse through the opening. In this case, as before stated, their union may be completed by cantharides.

Transplantation from the labia was practised by Jobert: his paper will be found in our Second Volume, p. 561.

Dieffenbach has never transplanted the externa integument. Recorded cases have been generally unsuccessful.

Complications. Vesico-vaginal fistula may be complicated by—1, rupture of the neck of the uterus; 2, of the perineum; and 3, of the rectum. In some cases all these evils are combined.

1. When a part of the neck of the uterus is torn, and is connected with one border of the fistula, the union of this part must be attempted by the suture or cauterization. If its whole length is split, and the fissure is continued into the bladder, or into the vagina also, and the urine escapes by the whole opening, the suture is less useful than the cautery. In applying the iron the os uteri must not be cauterized, or its adhesion and closure would follow. The suture is not to be recommended, as its application is very difficult in this situation, and very free granulation follows cauterization of the cervix uteri.

2. When the perineum is ruptured, a vesico-vaginal fistula is generally very large, as a very severe labour has probably been their cause. Prolapsus of a portion of the bladder is then frequent, and the uterus sinks and afterwards prolapses. Operation upon the fistula is only to be attempted when it is small enough to afford reasonable hope of success, but if it is so large that prolapse of the bladder has taken place, then the surgeon must content himself by healing the perineal rupture. The sunken uterus and

bladder must be supported during the healing of the fissure after the application of the suture, and cold water frequently injected to prevent the irritating effects of the urine. If both operations are to be performed, that upon the vesical opening must be first performed, and when that has succeeded the perineum must be attended to.

3. When the perineum and rectum are both ruptured, the case is most melancholy. The contemporaneous rupture of the bladder, vagina, perineum, and the inferior part of the rectum forms a large cloaca where urine and excrement mix. Both bladder and uterus are sometimes filled with excrement. The treatment is exactly the same as when the perineum only is ruptured.

Closure of the os vaginæ has been recommended by Vidal de Cassis in cases of large incurable vesico-vaginal fistulæ. The anterior part of the vagina is made to adhere, and the canal changed into a urinary reservoir. M. Vidal has been much blamed for his proposal. Dieffenbach thinks without reason, as the condition of the patient is ameliorated, and especially if the perineum and rectum be also ruptured, the uterus prolapsed, and we are thus enabled to heal the whole enormous fissure.

"The closure of the vagina is only applicable to the largest perforations, when no healing can be expected. It has the advantage of supporting the bladder, and preventing the continual escape of urine. The fear that the urine retained in the vagina will cause inflammation there is altogether groundless. It is seen in every case of vesico-vaginal fistula that the vagina, moistened with urine, appears pale, and never irritated, while the bladder seems red and inflamed by the air. It is only upon the outer surface of the labia, particularly upon the skin over the glutæi and that of the inner side of the thighs, that the urine irritates and keeps up a painful chronic inflammation." (p. 597.)

The fears of the passage of the urine through the uterus and tubes into the peritoneal cavity, and the formation of a vaginal calculus are equally futile. The operation is not to be regarded as a means of *cure*, but as an artistical amelioration of the unfortunate condition of the patient.

The operation is exactly that of ruptured perineum, except that lateral incisions of the labia must be carried up to the height of the clitoris. When the application of the needles and twisted suture is accomplished, a catheter is passed by the urethra, or by the small opening left free, and cold water copiously injected, and again allowed to run off. The catheter remains for the passage of the urine, and the application of cold lotions. The sutures are gradually removed, commencing with those which have cut through one edge. If the suppurating edges completely separate from each other, an irritating ointment is applied and, when they are cicatrized, granulation is to be sought by cauterizing. The running suture may then be used to diminish the opening. If partial union follows the first operation, the portions remaining open are to be cauterized or irritated by tincture of cantharides or red precipitate, until union is perfect.

In one case, in which Dieffenbach thus closed the vagina, great advantage was obtained. A small opening for the urine was left, which the patient could close by a small plug, and thus retain the urine. On removing the plug she had the power of voiding a large portion of her urine. The formerly irritated and fiery red nates and thighs were covered with healthy skin, and no trace of any injurious influence of the urine upon

the bladder or uterus was observed. But this is the only successful case on record. Vidal himself did not complete the cure. Velpeau, Lenoir, and others, have also failed.

Dieffenbach is of opinion that no mechanical recipient for the urine is of any use. Still, patients wear useless and often injurious contrivances for years, satisfying themselves because *something is done.*

Recto-vaginal fistula is closed with greater ease than the vesico-vaginal opening, but still much difficulty arises; 1, from the thinness of the ruptured parts; 2, from the double coating of mucous membrane; 3, from the passage of excrement and flatus from the rectum. The causes are difficult labour, foreign bodies in the rectum or vagina, abscesses, &c.

The modes of operation are, 1, suture after removal of the edges; 2, suture after cauterization of the edges; 3, cauterization alone.

If the fistula be a species of large fissure the interrupted suture is the best. It is necessary to have small strong curved needles, silk thread, a fine small hook, a thin pair of hooked forceps, and a needle-holder. The rectum is cleared by enema, and then well washed by injection of cold water, and the patient placed in the same position as described for the operation upon vesical openings. An assistant with his finger, or a sort of leather bougie in the rectum, presses the gut and vaginal wall downwards and forwards. The edge of the opening is then fixed by the hook, and pared with the point of the scalpel, separating the firm connexion between the rectum and vagina. The needle is held at right angles in the needle-holder, the inferior pointed part of the opening drawn forward with the hook, the needle carried through both edges, and the suture tied tolerably tight. The ends of the thread are drawn a little down, and then the second suture is passed and tied, and so on until the highest one is tied. Then all the ends of the threads are cut off near the knots, except the lowest, which is only shortened, and allowed to hang between the labia. The inferior portions of the rectum and vagina are to be stuffed with soft charpie, particularly the former, to keep away the excrement from the sutures.

After the operation but little food is to be given, and constipation is to be obtained by doses of opium. If the charpie in the vagina is moist it is changed the following day, while that in the rectum is allowed to remain. The sutures are removed, one one day, another the following, and so on; and the charpie kept as long as possible in the rectum. When necessary it is removed with forceps, and then warm camomile tea is injected through a thick elastic catheter, and allowed to pass off again. If the operation has partially or entirely failed, the sutures are removed, the edges cauterized to some extent with the nitrate of silver, and the vagina and rectum plugged with charpie, which is changed daily. Small openings are thus closed; if larger, a repetition of the sutures becomes necessary.

In small fistulæ the running suture after cauterization of the edges is the best practice.

"The nitrate of silver is passed through a small speculum in the rectum, and the border of the fistula and the surrounding parts for a quarter of an inch in extent are touched with it. On the vaginal side inflammation is excited by a ball of charpie moistened with tincture of cantharides, which is prevented from touching

the other parts of the vaginal walls by plugging with dry charpie. If on the following day the edge and its circumference to the extent of half an inch is properly inflamed, the epidermis is raised by forceps and rubbed off with dry charpie, and then the running suture is applied by means of a fine curved needle and a needle-holder in the usual manner, carrying the needle all round the opening between the surface of the rectum and vagina, and tying the threads moderately tight together. The rectum and vagina are plugged with dry charpie. If union is accomplished the suture is divided and removed on the fifth or sixth day; if the operation has this time failed, the opening after cicatrization will appear much smaller than before, and by one or more repetitions of the operation will probably be closed." (p. 605.)

By one or other of these methods, Dieffenbach has cured many of these fistulæ. Some were an inch in length. Others remained after union of ruptured perineum when the rectum had been torn at the same time. In one case the opening, although not completely closed, was so small that no particle of excrement, only flatus, could pass.

The result of simple cauterization is very doubtful.

PERINEUM. We now pass to one of Dieffenbach's particular studies, Union of ruptured perineum. We may refer to an abstract of some of his cases in a former number, (Vol. VI, pp. 536-7) and to some remarks on the same subject, (Vol. XI, p. 195) and now proceed to detail his practice a little more fully.

In old cases of perineal rupture union is effected by suture after paring the edges; in recent cases the paring is not necessary. The only contra-indications to the operation are approaching menstruation, pregnancy, profuse leucorrhœa, diarrhœa, disease of the general system, and severe local inflammation.

Dieffenbach was formerly of opinion that it was better to wait some time after delivery before attempting this union, as the parts were generally much bruised and torn, the patient's system depressed, &c.; but experience has now taught him that it cannot be done too soon; that the first twenty-four hours after delivery must be chosen, and this especially when the rectum is also torn. When the suture is immediately applied union occurs in the majority of simple cases, and if the rectum be also torn, a recto-vaginal fistula only remains, which is treated on general principles.

The necessary instruments are thick strong needles, bent to a three-quarter circle, and threaded with waxed silk doubled twice; insect needles of various thickness and length, and cotton threads; a strong needle clipper; strong forceps hooked and smooth; pincers, knives, scissors, water, sponge, charpie, and syringes; the patient prepared as for vesico-vaginal operations.

Partial recent rupture. Suppose the anterior half of the perineum be torn through, and the wounded edges are even, the left edge is held with the thumb and finger of the left hand, and the curved threaded needle is inserted a few lines from the posterior angle of the wound, and about a third of an inch from the edge; it must pass straight, and not obliquely, through the wounded edge close to the vaginal mucous membrane, and be drawn forward with forceps until the middle of the thread arrives in the wound; then the right edge is seized, everted a little, and transfixed at the point exactly opposite the passage of the needle through the left edge; the threads are then tied tolerably tight with a double knot, and

the ends cut off; then a second suture is applied, and lastly the most anterior one, so that the commissure may be exactly united, and the edges of the mucous membrane be in exact apposition. Three sutures are generally sufficient for a perineum torn half through; but if after their application the space between them gapes, a couple of fine sutures may be applied, which merely go through the skin.

Total rupture. When the perineum is torn completely through to the anus, the edges must be made even, if torn muscle or cellular tissue render it necessary, and then the suture passed as before; but the most posterior suture must be formed of four or six strong waxed silk threads, passed a full half inch from the edge, and through the whole thickness of the wound, the vaginal mucous membrane excepted. Five other sutures must be applied, the middle being the strongest, and their distance from the edge of the wound gradually diminishing to one third and one fourth of an inch; union is thus more exact. The edges of the mucous membrane at the commissure are united by five sutures, and a couple are also applied on the internal surface.

Partial complicated rupture. The posterior or middle part may be torn, the commissure remaining; to these the preceding directions apply. The rupture may not be in the median line, but at one side, and complicated by injury of the labia, ecchymosis, &c.: in such cases the edges must be carefully made even by scissors before applying the sutures.

In total rupture, complicated by rupture of the anus and inferior part of the rectum, union must be attempted as soon as possible, or the large wound will become inflamed by the urine, lochia, and excrement; the cellular tissue sloughs, and unhealthy suppuration comes on; and if granulation and cicatrization succeed, much substance is lost, and the operation is rendered more difficult and painful. In such a case union of the rectum is first attempted by fine needles, and fine single waxed silk ligature, forming the *un*interrupted suture. The ends of the thread are brought out by the anus, then the perineum is united as before.

Old total rupture. The operation here differs, inasmuch as the cicatrized edges have to be removed before the sutures are applied. If much difficulty occurs, from contraction of the parts, in approximating the edges a lateral incision may be made on each side.

If there be also rupture of the rectum, the excrement passes through the large fissure, and, on examination, it appears as if there were no perineum whatever, for its halves are drawn to the surrounding skin, and very often scarcely any cicatrix is detected in their room. Sometimes, instead of a perineal fissure, some cicatrized folds remain on each side, as the remnants of the perineum. Here the operation will vary according to circumstances: either 1, union of the fissure; 2, the same with lateral incisions; 3, transplantation of neighbouring skin. We need not recur to the first and second method. The formation of a new perineum is only to be undertaken in large old fissures, reaching deep into the rectum, where the external skin is cicatrized and deeply wrinkled. The edges of rectum, anus, and vagina are pared. "Then in the depth of the furrow two parallel incisions are made of the required breadth of the perineum; an oblique incision an inch long is carried from their termination outwards, one both before and behind to each incision. Thus two oblong

quadrangular flaps are formed, which reach from the labia to the anus, and which are then loosened from their under surface." (p. 626.)

This sort of bridge is united by sutures, and if there is tension, this must be relieved by lateral incisions.

The after-treatment is of the greatest possible importance with regard to the success of these operations. The best position is upon the back, thighs moderately open, and knees bound together with a thick compress between them. If they are bound together without the compress, the united parts are pressed too much together, and this, in wounds united by suture, is almost more injurious than the opposite fault. For the same reason the position on the side is not to be recommended. In fresh ruptures the parts are often washed by warm-water injections, and dried with sponge. If there is much collection of moisture in the vagina, charpie must be introduced and often changed. If much inflammation follow the sutures, leeches must be applied, and antiphlogistic general treatment adopted. In old cases, if the woman be strong, cold applications are used, and often changed; if she be old or weak, lead lotions. The urine is never to be passed voluntarily, but always drawn off by the catheter, until the wound is firmly cicatrized. After three days a ligature may be removed, a couple on the fourth, the middle and posterior being left the longest. The internal sutures of the mucous membrane are withdrawn, when it can be done without stretching the parts; the uninterrupted suture of the rectum must be left to come away of itself, and sometimes requires a fortnight. Generally speaking, after the third day, lead lotions are useful. Everything must be done to prevent the patient having a motion for six or eight days after the operation: spare diet, and a quarter of a grain of opium twice daily. When the union is firm, the rectum is cleared by injecting warm barley water through an elastic tube, assisting the passage of scybala by a scoop; better diet is then given.

If union follow around some of the sutures, and a fissure remain in other spots, charpie, wetted with infusion of camomile, is placed in the fissure, and often changed. If the edges are indolent, nitrate of silver and dry lint are used. If a recto-vaginal fistula remain, it must be treated as before directed. If union does not follow the application of nitrate of silver or resinous dressing, a second operation must be performed after cicatrization and restoration of the general health. After the most successful operations, the vagina must long be kept well cleansed with warm water, and a mild ointment applied to soften the cicatrix. Very often the hardness entirely disappears, and after some years, scarcely any trace of the operation remains. T bandages, sponge, or pessaries in the vagina, daily evacuation of the bowels, which have all been recommended, are excessively dangerous and injurious.

The manner in which this subject is treated is most creditable to the author, and the results of his practice are most encouraging. He says, "Several women upon whom I have operated had suffered the rupture in the first labour, ten or fifteen years before, and had since remained childless; after the union of the perineum they conceived and bore children, and in no case did a new rupture again take place from the labour." (p. 637.)

Is not this a triumph of modern surgery? Is not this sufficient to

reconcile the most sceptical to endure all the uncertainties and failures of the art?

UTERUS. The operations for the cure of *prolapsus of the uterus and vagina* are contained in the 57th chapter. Palliative treatment by pessaries, or sponges impregnated with astringent fluid, is of use when the patients are very weak and there is great relaxation of the parts, but in the majority of cases mechanical support does much more harm than good, and the radical cure must be attempted, unless the party is advanced in life, very weak, or suffering from incurable disease of the uterus or vagina.

Sometimes a natural process of radical cure is set up by inflammation, ulceration, and subsequent granulation of the vagina, with thickening of the submucous cellular tissue, and contraction of the cicatrices. Prolapse of the uterus is thus prevented, and one of the methods of radical cure is an imitation of this process.

There are two principal methods of obtaining a radical cure. The first, *elytrorrhaphie,* consists in producing an artificial stricture of the vagina, by excision of parts of the mucous membrane of the projecting parts, or of that of the vagina. An elliptical portion three inches long and two broad, the points of which are above and behind, and below and before, is removed from each side of the vagina, and if the parts are very lax, a small segment may be also removed from the anterior wall; but generally it is better not to interfere with the anterior wall, on account of the danger of injuring the bladder, and it is better to employ subsequent cauterization if necessary. After the removal of the portions of mucous membrane, the edges of the wounds are brought together by sutures, and the uterus replaced; the parts are cleansed and the vagina plugged with charpie. The sutures are removed when the speculum shows that union has taken place; but even if union by the first intention does not follow, and the wounds suppurate, nothing is lost, as the prolapsus is often better supported after healing by granulation and cicatrization, than if the wounds had at once united.

A second method consists in paring long strips of mucous membrane from the vagina along its whole length, with the express purpose of producing strong superficial cicatrices.

After these operations the patient is kept recumbent, that the falling uterus may not interfere with the wounded parts; the catheter is at first kept in the bladder, and mild enemata are administered. Cold applications are injurious, either bringing on erysipelatous inflammation, or retarding and rendering torpid the granulating process. Injections of warm camomile tea are useful, and after some time charpie or sponge, moistened with red wine, may be kept in the vagina; and lastly, alum and other astringent lotions are used.

Dieffenbach, however, prefers cauterization of the vagina to either of these proceedings, as he says relapses are apt to follow them. When the parts are so large that the uterus immediately falls on the patient's standing erect, six lines of cauterization with a conical iron, passed slowly from the internal surface of the labia to the neck of the uterus, are required; if the parts are less lax, three or four lines are sufficient; then the parts are covered with cotton wadding, and the separation of the sloughs waited

When the uterus prolapses only after long standing or walking, or on going to stool, it is better to apply the iron by means of a speculum, than to draw the uterus down, as in the method just described. Cotton wool is introduced, after some days it is removed, warm milk or barley water injected, and cicatrization assisted by subsequent injections of lead lotion.

The results of cauterization are most satisfactory. Our author has thus cured a great number of cases, and enabled the sufferers to dispense with their pessaries. In some cases the parts were so much enlarged, that even the largest species of mechanical apparatus could not be supported, but fell out. Some of these persons after the cure conceived, and their labours were unattended by difficulty. In one case, after labour, the uterus again sunk, and a second slight cauterization was required. Cauterization is also much more safe than any of the cutting operations, and the iron is far preferable to the nitrate of silver, potassa fusa, or chloride of zinc, as these are seldom successful—either not acting to sufficient depth, or destroying the parts to an extent which is not desired.

This radical cure is complete. Fricke has employed a method of half cure, by causing adhesion of the labia, by paring their edges, and uniting by suture, *episiorrhaphie*. The plan is easy and safe, and prevents prolapse externally. It is thus a palliative rather than a radical cure, and has the disadvantage of rendering the patient unhappy; and if conception take place, redivision has been necessary to allow of the birth of the child.

Prolapsus of the vagina is the condition in which flaps or folds hang from the genitals externally: the uterus may or may not be sunk. The methods of operation are the same as those for prolapsed uterus, but here excision is more useful than cauterization.

The *gynoplastic* operation is treated in the next chapter,—the operation for opening or dilating the closed or contracted genital organs of the female. This is required in cases of total or partial adhesion of the labia or nymphæ, imperforation or fleshy condition of the hymen, membranous closure of the vagina high up, partial or total adhesion of the vaginal walls to each other, and in closure of the os uteri.

Adhesion of the labia and nymphæ in the first months of life are separated by mere pressure with the fingers, forcing them apart, and then keeping a piece of card between them. In adult girls, unless the union is so great as to impede the escape of urine and menstrual fluid, it is better not to interfere before marriage; but if the union be complete, or so great as to cause more or less retention of these fluids, division with the scalpel must be effected. The more superficial the union, the less must be the extent of incision.

When the adhesion is very extensive, simple division is sure to be followed by recontraction, and transplantation of skin or mucous membrane alone affords means of radical cure. Separation is effected by the scalpel, the mucous membrane of the posterior border of the wounded surface loosened, and then drawn forward and united by suture with the anterior border; or the external skin may be used. Incisions of this form are made from the posterior commissure towards the perineum ┌┴┐. The flap

been previously effected, it is drawn inwards, and united by sutures with the wounded border of the mucous membrane.

We need not describe the mode of perforating or excising the hymen.

Membranous closure of the vagina, if any opening exist in the membrane, is treated by the probe-pointed bistoury; if the borders are very callous, five or six notches may be made. The parts are plugged with charpie, and sometimes it is necessary to wear elastic bougies for some time.

The operations, when there is total adhesion of the vaginal walls, are the same as those just noticed, but of course more difficult. The greatest care must be taken in the direction of the incisions, as Dieffenbach has seen incurable vesical and rectal fistulæ follow unskilful practice.

When closure of the os uteri is membranous, and the membrane is thrust forward by the menstrual fluid, the operation is without difficulty—division with a pair of scissors. If the lips of the os uteri are adherent, and the canal of the cervix also, perforation with the pharyngotome is followed by dilatation of the puncture with a probe-pointed bistoury. When the os uteri is closed by cancer, or in any way so that its opening is impracticable, and the patient is pregnant, if rupture of the uterus is feared, hysterotomy must be performed through the vaginal portion.

Rectum. We have now a chapter on *closure of the rectum* and anus. This condition varies; the closure may be simply cutaneous, and simple perforation then suffices to afford a passage for the excrement. The rectum may not be sufficiently long to reach towards the integument; in this case a new canal must be formed. It may be too long projecting beyond its normal situation; in this case its opening must be obtained at the proper site, and the superfluous portion removed. Lastly, it may open in another cavity, vagina, urethra, or bladder; here the unnatural opening must be closed and another formed.

The formation of a new canal, when the rectum is too short, is effected, after catheterism of the bladder, by an incision in the situation of the anus half an inch deep, and then carrying on a trocar in the direction of the rectum to the extent of two inches, if necessary. If the gut is opened, it is cleared by injections of warm water through an elastic tube, and the opening kept constantly patent. If the rectum can be brought down to the external skin, it is to be stitched there.

When the rectum opens into the vagina, two methods of operation are laid down—the former easy, the latter more difficult, but its result more speedy. The former consists in passing a bent sound through the vaginal opening for a little distance up the rectum, inserting the knife close behind the opening outside the vagina, and carrying it through the skin close to the coccyx, without enlarging the opening into the rectum; then the rectum is laid bare, and its edges united with those of the skin. The vaginal opening is afterwards treated by caustic. After complete healing, the inferior anterior open perineal end is separated by transverse incision from the vaginal wall; the rectum thus drawn back into the cicatrized opening, and the new wound of the perineum united.

In the second method an oval portion of skin is excised in the natural situation of the anus, and the rectum freed both on its anterior and lateral

posterior wall of the vagina; the part where the opening of the rectum unites with the vagina is divided by fine scissors, and the separation of the perineal end completed by the scissors. The border of the freed rectum is then united by fine sutures with the external skin.

In all the cases of rectal opening into the bladder which Dieffenbach has seen, death followed from inflammation set up by the entrance of meconium.

Stricture of the rectum is treated in the following chapter. It may be superficial from ulcers around anus, and the contraction or adhesion consequent upon their cicatrization. Dilatation, incision, and sometimes excision of cicatrices, are necessary.

In some cases from inflammation, or cicatrization of ulcers of the mucous membrane, the rectum is drawn together as though it were surrounded by a ligature, and an opening, only sufficient for a quill to pass, remains in the centre. This is treated by passing a small knife along the finger and notching the cicatrix, and afterwards passing an oiled wax bougie. If the contraction return, and the stricture is not more than two inches from the anus, it must be extirpated. A curved blunt-pointed bistoury is introduced along the left index finger, and the anus enlarged to the extent of an inch and a half, above, and also towards the coccyx; the edges are then held apart by blunt hooks, and a sharp hook passed through the stricture, the ring of which is drawn down, and its base surrounded by incision with a small scalpel; the part included in this incision is then fixed by hooks and cut away with scissors. Between the superior and inferior broad portions of the rectum a chasm now remains, the superior and inferior edges of which must be united by seven or eight sutures, so that a fissure remains. The threads are cut away close to the knots, charpie introduced in the rectum, and the external wounds united by sutures; cold applications are employed, and the patient constipated by opium. The sutures of the rectum are left to separate of themselves. "In this manner I have completely cured many patients who had long been unsuccessfully treated by bougies, and in whom I had often incised the strictures in vain, and a relapse has never taken place." (p. 687.)

Spasmodic stricture has its seat in the external or internal sphincter. Narcotics and enemata are only palliatives; bougies make matters worse; and division of the sphincter is the only cure. This may be done in the common way, carrying the knife along the finger introduced into the rectum, or by subcutaneous incision, but the latter method possesses no advantage, as the wound in the other case always heals readily.

Colotomia, or the formation of an artificial anus, is the subject of the next chapter. Our author follows Amussat, and therefore we need not condense his descriptions at present, but take the opportuity of referring our readers to an excellent article on Intestinal Fistula, by Mr. Teale of Leeds, in one of the last published numbers of the 'Cyclopædia of Practical Surgery,' where this subject is most fully and satisfactorily treated, and pass on to the modes of curing preternatural anus by operation, which are described in the 62d chapter. This subject, however, is one of such

importance that we think it better to devote a separate article to its consideration hereafter, than to pass over it here in a cursory manner.

FINGERS AND TOES. The 64th chapter includes the operations for the separation of fingers or toes, which have been united together either congenitally or in consequence of disease. In young children, when the uniting membrane does not quite reach the tips of the fingers, round smooth cords passed between the fingers like violin strings, and fastened to a bandage round the arm, by long-continued pressure sometimes effect a cure. Simple division with the scalpel is sufficient when the union is membranous and superficial, fine linen being wound round the finger after bleeding has ceased. The hand is supported by a pasteboard splint, and after a few days small compresses, wet with lead lotion placed between the fingers hasten the formation of skin over the wounded surfaces.

Subcutaneous division of the fascia palmaris is sometimes necessary to remove deformity produced by these adhesions.

It having been found that after simple division reunion was liable to take place by granulation from the palmar end of the incision, it was proposed by Rudttorfer to make an opening in the commissure, and when this had become cicatrized, to divide the remaining portion. The idea is clever, and the practice often successful. The commissure is perforated, and a piece of leaden wire is passed through and kept in the wound by being bent, until the edges of the opening have healed, then the remaining part of the commissure is divided, and the fingers bound with linen as before. Dieffenbach has not found this method so successful as others have done, the irritation produced by the lead leading to the necessity for its removal, and no formation of an open canal following.

In some cases, after division of the commissure, Dieffenbach has successfully united the edges of the wound made by this division by sutures, assisting the approximation of the edges by lateral incisions.

When the union is of the whole breadth of the finger, the only sure operation is the transplanting a flap of skin. The flap may be formed from the skin of the back of the hand, but it is apt to contract, the sutures suppurate, and the flap dies; and Dieffenbach, in all cases to which it is applicable, performs an operation which he describes as most successful, forming the flap from the commissure. We give the description in his own words:

"A long incision is made with a small pointed scalpel upon the back of the hand, at the border of the union, and of a finger which reaches to the middle of the posterior phalanx. A similar incision, parallel with the first, is made along the posterior part of the finger; then both incisions are united by a cross cut; then the anterior border of this strip of skin is fastened by hooked forceps, and loosened, with the greatest possible quantity of the subjacent cellular tissue, as far as the point where the fingers are normally separated. Its breadth, in an adult, should be a quarter of an inch; in children it is of course smaller. Then the points of the fingers are pressed apart from each other, and a long incision is made between the fingers through the skin, first on the back, and then a second longer one on the under surface reaching to the hand, and the remaining adhesion separated. On the volar aspect of the finger a cut, rather more than a quarter of an inch long, is made across before the end point of the incision; then the flap is turned in between the separated fingers, the epidermis surface thus remaining naturally turned outwards, and the anterior narrow end of the long four-cornered flap is fastened by three sutures to the wounded edge of the cross incision on the

volar aspect. The remaining wounded edges of the fingers are brought towards each other by strips of plaster, and the space between the fingers is filled by soft charpie, and their points kept so far apart from each other by a thick compress, that the flap may not undergo the least pressure, and die from the approximation of the fingers; then the fingers are surrounded by a finger bandage, in order to retain the charpie, and the hand is fastened upon a splint.

"The neighbourhood of the flap, which must be free from the bandage, when inflammation comes on is covered by lead lotions, and the sutures are removed after its firm union; but still for a long time pressure is made upon the flap by small strips of plaster passing from the back of the hand between the fingers, reaching over the middle of the palm. The healing of the other wounds of the fingers follows without difficulty by granulation and cicatrization. If several fingers are united, the operation is repeated in the same manner at a subsequent period." (pp. 744-5.)

If the union be bony it may be divided after incision of the soft parts with a finger saw, but only in slight cases, taking care not to open the joints. In cases where bony union of all the fingers with each other exists operation is quite useless.

Operations upon adhering toes can be scarcely ever necessary, as the toes are as useful as when separated. If there is also contraction by which the anterior phalanx and nail are turned to the ground, subcutaneous division of the flexor tendon will be necessary.

In the following chapter a form of prominent ulcer is described frequently following loss of the toes, or their removal either at their articulation or in the middle of a phalanx. It is very obstinate, and causes great suffering. The means proposed for its cure are, after surrounding incisions and paring away the surface of the ulcer, cutting a proper sized flap from the back of the foot, covering the site of the ulcer with it by turning, and then uniting the edges by suture. We can say nothing on this point from personal observation.

MUSCLES AND TENDONS. A section of 100 pages on the division of tendons and muscles concludes this volume. In our Thirteenth Volume (pp. 1-28) we made a full analysis of the work of Dieffenbach on this express subject, and also of that of his follower and commentator, Phillips; and in our Eighth Volume, (pp. 385-405), of those of Stromeyer, Bouvier, and Little. Anything like a connected account, therefore, of the section before us is clearly unnecessary, and we shall conclude the present article by a few remarks upon such interesting points as strike us in perusal.

After the division of tendons, Dieffenbach much prefers flannel to linen bandages, being more elastic, and consequently impeding the circulation less should swelling come on.

With regard to the operations for stammering, he states that he operated in about eighty cases; a quantity (Anzahl) were completely cured; others which appeared to be cured commenced stammering again, some sooner, some later. Some upon whom the operation had apparently slight effect, became afterwards improved, but in by far the greater number, in whatever manner, or however often the operations were performed, they were ineffective. It is therefore only to be recommended in the most obstinate cases of stuttering, when exercises of the voice and other means have proved useless. It is a great pity that the exact number of those cured and benefited is not given. "A quantity," or "a certain number," is very indefinite.

of the back, in cases of spinal curvature, is scarcely ever of any use.

When speaking of the division of muscles to favour the reduction of old dislocations he refers to a case which had resisted all attempts at replacement, and which readily yielded after subcutaneous division of the tendons of the pectoralis major and teres minor.

After division of the tendon achilles on account of contraction, the result of paralysis of the extensor muscles, extension must be used with great caution. Directly after division the foot can be brought to a right angle, and the end of the tendon separated some inches, and if this is done the space becomes filled by exudation of blood, and union does not take place. Fourteen days should be allowed to pass in such cases before the extension apparatus is applied, and the cure is complete a few weeks later.

We find nothing more which is not fully noticed in our former articles, with the exception of a few remarks on habitual spasm of the flexor pollicis longior. Many persons much accustomed to the use of the pen are subject to spasms in the muscles of the thumb and index finger, and the influence of the will over the direction of the pen is lost for the time, and they write quite a different hand to their ordinary character. Their writing appears as if they had been in a shaky carriage on a bad road. With time the spasms become more severe and constant, and shaking of the fingers, which may be compared to stuttering, comes on; at a more advanced stage the pen can only be held by passing it through a cork, and after a time even this becomes too small to be held. In one case the pen had to be passed through a cork from three to four inches square, and this was inclosed by the whole hand. Usual remedies are of no avail, except long continued disuse of the pen. In one case a patient who did not take a pen in his hand for six months, at the first trial afterwards wrote with great ease. With one sole exception the author has observed this condition in males only. He has practised tenotomy in such cases, but in only one was the cure perfect; in six others the state of things was the same after as before the operation. He divided the flexor pollicis brevis and longus, adductor pollicis, and the muscles of the index, according to the circumstances of the case, but without result. After the healing the spasms returned in the same degree, without any ill effect following, or the utility of the finger being much lessened.

Nerves. When we stated that the operative orthopedy concluded the volume, we had not observed a short chapter on the division of nerves. It is a very good one, giving a clear account of the experience of European surgeons, and taking a very unfavorable view of the operation under any circumstances. One case of the author, however, is interesting: very severe neuralgia had followed venesection; the injured nerve was divided, with instant effect, the pains, which had continued a month, immediately ceasing. A similar case is quoted from Hirsch, in which convulsions and coma accompanied local neuralgia, which were removed by two deep incisions over the wound. But whether for the relief of neuralgia, or tetanus, or for any other cause, success is rare; and "surgery is here under the greatest obligation to physiology, for pointing out that the sanguinary path is not the right one." The remedies the author has found most successful are the decoct. Gittmanni, iodine, and the cod-liver oil ("thrancur.")

many particulars defective, cannot be denied; still, in the departments of plastic surgery and tenotomy, it is by far the best work extant. The style is generally clear and forcible, and the descriptive portions, particularly when treating of the sufferings of patients or the results of operations, often border on the poetical. Still, in the description of many of the more delicate operations, the want of woodcuts in illustration is painfully felt. In more than one instance we had vainly endeavoured to satisfy ourselves that we had exactly comprehended the directions laid down, and in such cases have given a literal translation rather than a paraphrase, that we might not run the risk of misinterpretation. We would strongly advise the publication of a series of diagrams with the second volume, illustrating not only the operations to be there described, but also those included in the volume before us; the value of the work would then be greatly increased, and the student's task much facilitated.

Two courses were open to us in the composition of the preceding article: the one, to confine our attention to a very few subjects, and treat them fully by copious extracts and critical remarks; the other, to endeavour to give a general outline of the contents of the work before us, and a condensed account of its most important or new contents. We have adopted the latter, as likely to be the more useful; and as the work must become one of standard surgical reference, we shall hereafter have occasion to refer to it, when commenting on other works treating on any of the subjects we here have glanced over. To those who think we might have followed both courses, we would remark that it is not easy to condense eight or nine hundred pages of Dieffenbach's into forty or fifty of our own. This difficulty, however, is a test of the value of the work. We could point out effusions of similar extent in our own tongue, on which a single page would be fruitlessly expended. If writers would follow the example of our author, and keep silent until they had something valuable to communicate, much of the critic's task would be spared. It now only remains for us, in the name of our operating brethren, to return most cordial thanks to the veteran professor of Berlin for this most valuable contribution to the literature of our profession.

Art. II.

1. *Second Report of the Commissioners for inquiring into the State of Large Towns and Populous Districts.* 2 vols. 8vo.
2. *A Bill for the Improvement of the Sewerage and Drainage of Towns and Populous Districts, and for making Provision for an ample Supply of Water, and for otherwise promoting the Health and Convenience of the Inhabitants.*

I. Report of the Commissioners. In this, their second report, the commissioners observe, that until the publication of the reports made to the Poor Law commissioners in 1839 upon the condition of the poorer classes of her Majesty's subjects in certain parts of the metropolis, fol-

lowed by the report of a Select Committee of the House of Commons, in the year 1840, "on the Health of Large Towns and Populous Districts," the extensive injury to the public health, now proved to arise from causes capable of removal, appears to have escaped general observation, while the means of remedying the evils by improvements in drainage, or by other structural arrangements, as have been carried into operation, have been executed more with a view to the appearance of the town or the comfort of a portion of the inhabitants, than directed to maintain the health of the whole community. They also state that subsequent investigations and reports have excited increased attention to the importance of providing for the physical condition of the poorer inhabitants of large towns. The wealthy and intelligent classes resident in them are now for the most part becoming alive to this great question, and to the necessity of providing for the removal of those causes which tend to vitiate the air in the quarters occupied by the poor, and especially in those most densely crowded.

There can be no doubt that the course of inquiry the commissioners adopted for obtaining information was eminently judicious, and has mainly led to these results. By calling to their aid the assistance of the most influential and intelligent of the inhabitants, through whose means and local knowledge the peculiar conditions of many localities were closely investigated, scenes of misery and neglect were exposed to their view, of which many were previously ignorant, and their attention directed to causes of disease, arising from the defective state or absence of proper structural arrangements.

The necessity and utility of sanitary arrangements for towns is by no means a new topic of discussion within the profession. Even the idea of making the refuse of towns serviceable to agriculture, and a source of wealth to the country (developed by Mr. Smith, of Deanstown, in his report on York and other towns), was broached so early as 1798. In the second number of the 'American Medical Repository' of that year, Dr. Mitchell proposed, "to convert the mass of nuisance, which we now are with such happy success engaged in removing from the city of New York, by the powers of vegetation, from poison to wholesome articles of food." His plan was, that lime, potass, and soda, should be thrown about the streets to neutralise the emanations, and make the refuse more efficacious as manure. In 1802, the question of town drainage was again mooted in the United States and England. In that year, Dr. Patterson, of Londonderry, thus writes to the editor of the 'London Medical and Physical Journal:' "Perhaps some of the readers of the Medical Journal may be able to give an instructive answer to the following question, asked by Dr. Miller, of New York: 'Can you direct me to any source of full and satisfactory information concerning the best method of discharging from cities the filth of animal and vegetable kinds constantly accumulating in them, and especially the filth of privies? We are endeavouring to make some improvements of this description in New York, and wish to be instructed and assisted by the wisdom and experience of older cities.' Give me leave, on this occasion," adds Dr. Patterson, in the language of the day, "to second this wish of Dr. Miller, and to interest on this occasion your ingenious and philanthropic correspondents." Either the editors had no correspondents of the required description, or more probably their ingenuity and philan-

We believe that, although there was a general impression on the minds of professional men as to the noxiousness of emanations from putrid or decaying animal or vegetable matter, less specific information was possessed by them, previously to the appearance of Mr. Chadwick's famous report, than the majority would willingly allow. Stinks on a small scale were unnoticed; they crept through the net of professional criticism. Stinks on a large scale were adjudged offenders, but were too great to grapple with, and were passed over with a shake of the head and a solemn protest. Formerly the judgment of the public was not enlightened; consequently the appeals of the profession to the dull and ignorant authorities might as well have been made to a statue of Memnon. It is by the active collection and diligent diffusion of information on matters relating to the public health, that the Health of Towns' Commission have done such good and lasting service to the public.

As the subjects specified in her Majesty's commission were essentially of a practical character, the commissioners have endeavoured to avoid as far as possible the discussion of the theoretical causes of disease. All the medical witnesses examined before them are unanimous as to the injurious effects produced by emanations from animal or vegetable matter in a state of decay, whether they act as direct or contingent causes of disease; and they are quite concurrent in their opinion that the existence of such causes and their prevalence have been sufficiently ascertained to require the interference of the legislature. The presence of such emanations, whether they be derived from stagnant ditches, from open cesspools, or from accumulations of decaying refuse, is a great cause of disease and death, not confined to the immediate district in which they occur, but extending their influence to neighbouring and even distant places.

It is too commonly supposed, the commissioners observe, that the evils above adverted to are the inseparable concomitants of poverty; and doubtless, so long as the inhabitants of the most neglected and filthy abodes in crowded cities are unable to provide for themselves better and healthier dwellings, sufficient light and air, more open situations, effective cleansing and drainage, and adequate supplies of water, their vigour and health are undermined, and their lives shortened by the deleterious external influence consequent upon the want of efficient arrangements for securing the above objects.

The commissioners farther advance, as a result of their inquiries, that other diseases besides fevers, namely, scrofula and consumption, are developed by injurious emanations and impure air; that the ascertained excess of deaths amongst artisans is in a great degree due to the defective ventilation of their places of labour; that the crowded and impure courts are distinguished by a high infantile mortality, and an increased proportion of births; and that many of the pecuniary embarrassments and much of the wretchedness and moral degradation of the poor may be traced to the physical degradation consequent on the neglect of hygienic measures. One of the facts which came to their knowledge, although not within the terms of their commission, they express themselves as bound to report on; it is the "terrible practice," as they term it, of giving opiates to children;

debility and imbecility in those who survive the administration of the narcotic.

The following is a short outline of the measures they propose to be adopted: their main reasons for these will follow.

"We are of opinion that, for the effectual correction of the evils above adverted to, additional legislative measures are requisite. It is necessary that the Crown should have power to inspect and supervise the execution of all general measures for the sanatory regulation of large towns and populous districts.

"That the local authorities entrusted with the execution of such measures should be armed with additional powers, and that the districts placed under their jurisdiction should in many cases be enlarged, and made coextensive with the natural areas for drainage.

"We recommend that the necessary arrangement for drainage, paving, cleansing, and an ample supply of water (the most important matters conducive to health) should be placed under one administrative body.

"We also urge the necessity of some general sanatory regulations relative to buildings and the width of streets, and that low lodging houses should be placed under public inspection and control." (p. 11.)

It is a remarkable circumstance that most serious attention was given to works of drainage from the earliest periods of our constitutional history. The earliest fundamental provisions have been based upon the footing that such works, as well as measures for the maintenance of the free flow of running waters, were of general public and national, rather than of exclusively local, consideration. It is held, by the first legal authorities, to be one of the prerogatives of the Crown to issue commissions for the protection of the population, by the enforcement of proper works of drainage, and this prerogative appears to have been exercised by the issue of special commissions, as well after as before the passing of statutory provisions on the subject. The intervention of the Crown was often urgently sought for the public protection against the injurious encroachments of private interests upon the great public watercourses for mill power or for fishing-weirs. The 16th chapter of Magna Charta is a defence of the public rights against the growth of such encroachments. The fourth statute of the 25 Edw. III, c. 4, provides for the putting down of mills, weirs, dams, and other obstructions, and commissions appear to have been issued from time to time to see to the execution of the laws provided thereon. The commissioners give numerous examples of royal commissions for these purposes, and note the various modifications they have undergone from time to time until absorbed in modern legislation.

The inefficiency of modern, and especially local acts, seems to be partly due to the uncertain tenure by which the authorities hold office, partly to local influence, and partly to inefficiency of the powers given by law. It appears that ignorance of the authorities, (usually tradesmen, merchants, or lawyers,) has also been no trifling impediment to improvement.

The first and most important step, the commissioners observe, in providing for the efficient and economical execution of any plan of drainage, is the preparation of an accurate general survey, upon a large scale, of the area which it is proposed to drain. This view is supported by a large mass of valuable and important testimony, proving it to be the necessary preliminary to any such work. The extent of country to be comprised

within the jurisdiction of any local authority should be the entire natural area for drainage.

At present no such plans or surveys are accessible to builders or others engaged in works requiring a knowledge of the level of the adjacent lands. Hence serious losses have been entailed on the public by the construction of sewers and drains at improper levels, and of a capacity insufficient for the probable wants of a future population; and houses have been placed in situations, regardless of the means of drainage. Great loss and inconvenience from this cause have very generally occurred, and even very lately it has become necessary to enlarge and deepen some of the sewers recently put in.

The prevailing want of information among the surveyors and other officers having the charge of the drainage of towns, regarding the levels of the sewers, and frequently even the entire ignorance of their existence, may be traced to the absence of any proper survey. At Bristol, the first attempt to form a complete map of the sewers was commenced during the inquiry of the visiting commissioners, and in the town of Preston it was a work of several weeks to open the streets in order to ascertain the lines and depths of the sewers. In some large towns, as Wigan, Rochdale, and Bolton, there is not the slightest knowledge of the plans of the sewers. The charge by the Ordnance Office for surveying such a parish as St. Clement Danes, and laying down contour lines, with the sewer, gas, and water pipes, would not amount to more than 8*s*. per acre; but if such additions were made while a survey for the Ordnance map was in progress, the extra cost would be reduced to 1*s*. 4*d*. per acre, or to 6*d*. only, if the levels merely are taken and bench marks inserted; the scale in that instance would be 60 inches to the mile, but it appears that the extent of the scale would scarcely make any appreciable difference in the cost. This cost would vary in a trifling degree, according to the density of the dwellings and population in a given area, but it has been calculated that for laying down the levels, an outlay of from 30*l*. to 40*l*. would be enough for the average size of towns containing 20,000 persons; from 100*l*. to 120*l*. for towns of 60,000; and from 200*l*. to 250*l*. for those of 120,000 inhabitants.

The commissioners in the course of their investigations noted numerous evils to arise from limited jurisdiction for drainage. The suburban houses of many large towns are under no jurisdiction whatever, and sewers and drains are constructed or not, according to the whim of builders or occupiers. The outfalls of the natural drainage in several instances is occupied by dams, &c., for obtaining mill-power, as at Bradford, Halifax, and other places. The upper and lower parts of a town again are under a different set of authorities, and these clash and obstruct each other. The commissioners think the Crown should be empowered to define and to enlarge, from time to time, the area for drainage included within the jurisdiction of the local administrative body.

The necessary works for the drainage and sewerage of towns demand a surveyor of competent information. It appears, however, that local improvement acts do not require any qualifications, nor do they advert to the necessity of skill and experience in the officer; indeed, many of them do not so much as allude to the office. Everybody knows that merit does not always secure the favour of public bodies; so the blind leads the

blind, and sad botches they make. The eminent truth of the following remarks is obvious.

"All local works of improvement should be planned, and their execution superintended by a person having a competent knowledge of engineering. New subjects, connected most closely with the general health of the community, are now constantly attracting the attention of persons engaged in works of construction. The importance of the questions of ventilation and warming having been fully established by recent investigations, particularly demands the attention of the architect and engineer. The important duties which may in future devolve upon the officers charged with the construction of works, and the large discretionary power that must be vested in them, will undoubtedly render it necessary to establish some mode of testing the competency of persons offering themselves as candidates to fill such situations. Some assurance should be given to the public that the persons intrusted with these responsible duties are properly qualified." (p. 36.)

The deduction made from inquiries on this point is embodied in the following:

"We, therefore, recommend that the local administrative body appoint the executive and other officers under it; that the appointment and dismissal of the chief surveyor be subject to approval; that such officer produce proof of his qualification for the office to which he shall be appointed, and, if required, be subject to an examination." (p. 37.)

Hitherto public bodies have been found slow to act for the general good, and easily moved by private feelings in the removal of nuisances, and the regulation of matters involving the public health. The commissioners observe, that the contentions of parties and the influences of local interests frequently impose a serious obstacle to any scheme of improvement, from the unfounded fear that their interests will be affected. Such persons frequently obtain great influence in the decision of questions in relation to any alterations calculated to effect improvement in the condition of the working classes. It seems a fair proposition, that where the public welfare requires it, the inhabitants generally should be made responsible for the execution of the duties imposed upon them by law, for sanatory objects, on the same principle as they are now liable, for the public benefit, to repair the highways. On these and other grounds, the commissioners

"Recommend that, upon representation being made by the municipal or other authority, or by a certain number of the inhabitants of any town or district, or part thereof, setting forth defects in the condition of such place, as to drainage, sewerage, paving, cleansing, or other sanatory matters, the Crown direct a competent person to inspect and report upon the state of the defects, and, if satisfied of the necessity, have power to enforce upon the local administrative body the due execution of the law." (p. 39.)

The enormous defects in the construction of main and branch sewers in many towns, and their total want in many courts, alleys, and close thoroughfares, have been strongly pressed upon the attention of the commissioners. The result of their deliberation on this point is, that the construction of sewers, branch sewers, and house drains, ought to be intrusted to the local administrative body.

After one or two recommendations as to the modes in which the necessary funds should be raised, the commissioners proceed to the important matter of paving. They do not seem, however, to have devoted much, if any, attention to the comparative merits and hygienic relations of the

various kind of pavement lately brought into notice, but simply content themselves with expressing the opinion, that the whole of the paving, and the construction of the surface of all streets, courts and alleys, be placed under the management of the same authority as the drainage, and that the limits of jurisdiction for both purposes, wherever practicable, be co-extensive: also that the principle before submitted in respect to the cost of making drains and sewers, and the equitable distribution of the expense, be adhered to in the case of laying out, levelling, and paving of streets, courts, and alleys; but for the purpose of insuring the greatest efficiency and economy in the execution of the work, they recommend that it be performed by the local public officers.

Few of our readers are unacquainted with the distressingly filthy condition of the streets and courts occupied by the labouring classes of towns; of the numerous and formidable diseases arising therefrom, and of the difficulties such a condition raises against the successful treatment of disease. All this is *proved* by the most extensive and elaborate statistical data in the reports of the commission. It is a more interesting question *now* to determine the mode in which the refuse of a town can be best removed.

The earliest regulation appears to have been this; that all householders should cleanse that portion of the street before their respective houses, and to heap up the dirt and soil in preparation for the scavengers. We find such duties prescribed in a very early act for Liverpool (21 Geo. II, c. 24). It is thereby enacted, that the occupiers shall sweep their portion of the streets at least twice a-week, on every Monday and Thursday, or oftener if required, and that the scavengers shall attend every Tuesday and Friday, "or oftener if occasion be." It appears, however, that although the provisions of this act, positively directing the scavengers to cleanse all the streets twice a-week, were in force up to the year 1842, it was the practice to cleanse the minor streets only once a-week, and the others, where there was less traffic, when required.

The manifest inconveniences of such a system naturally led to the appointment of scavengers paid by the householders. In Aberdeen, the local act requires the appointed scavengers to cleanse the foot-pavements, and the whole of the streets, closes, courts, &c. every day, under a penalty. This work is done at a profit of £600 a-year to the city, and in other towns in Scotland similar examples are found. The commissioners observe that the law which the legislature has made so stringent at Aberdeen, and which has been carried into execution there and at Edinburgh, may with equal advantage be applied generally to all parts of Great Britain.

The removal of dirt, ashes, and rubbish from all houses and premises is another important point in town-cleansing. This has also been made a profitable affair. In the metropolis the ashes are an article of considerable trade. The contractors are in the habit of paying large sums of money to the parishes for the right to collect them, and the large parishes frequently make a considerable profit from the exercise of this right, which aids in the diminution of the rates, after paying the expense of cleansing (in the mode considered sufficient by them) all the streets and roads in the parish. The commissioners, for these and other reasons, recommend that

the provisions in local acts, vesting the right to all the dust, ashes, and street refuse in the local administrative body, be made general; and that the cleansing of all privies and cesspools at proper times, and on due notice, be exclusively intrusted to it.

The removal of nuisances is the next question considered by the commissioners. The first noticed is that of "midden-steads," or dung heaps. Perhaps there are no more general and frequent sources of fever than these. In Ireland they probably cause the greater proportion of fever cases. That country, in fact, may be medico-topographically described as the land of dung-hills and fevers. But in England, with all its pretensions to cleanliness, towns are bad enough. Let us take Sunderland as an example. In this borough, on the testimony of its inhabitants, there are no fewer than 182 public midden-steads, receptacles for filth of all kinds, which are stated to constitute one of the greatest nuisances within the borough. They are generally situated in the close, narrow streets and lanes inhabited by the poorer classes, and are frequently resorted to by them. In some cases these midden-steads are actually in the basement floor of a dwelling-house, the upper stories of which are occupied as bed-rooms, &c. The contents of these midden-steads are afterwards conveyed to large depôts, of which there are two in the parish, one very lately advertised as containing 1000 tons for sale. This belonged to the borough.

The state of the slaughter-houses are also an almost constant source of complaint. They are very rarely placed under any regulations with regard to the constant removal of the animal refuse, their proper ventillation, or a sufficient supply of water to ensure due cleanliness. The improper situations in which these places are found, sometimes even under dwelling-houses, and the effect produced upon the health of the inhabitants, is described in the report on the towns of Lancashire. In these towns slaughter-houses are found below dwelling-houses, the smell in which was most insufferable. In many of these cases the inhabitants looked pale and sickly, and diarrhœa frequently prevailed, although absent from the court, contiguous. Yet the state of the law prevents any interference with the manner in which these slaughter-houses are conducted. True it is that aggrieved parties may indict the occupiers of the premises, but they being labouring men, can neither afford the time nor money to pursue such indictments, nor do they belong to a class aware of the pernicious effects arising from the presence of decomposing refuse. The uncertainty of the result, moreover, is alone sufficient to deter even those who have both the means and the inclination to suppress them. In scarcely one instance, however, in which shambles or slaughter-houses have come under the observation of the commissioners, either in the metropolis or in the provincial cities or towns, have there been found in force any regulations or authoritative supervision to compel the speedy and regular removal of offal from, or the efficient cleansing of such places. They have, on the contrary, been found to be, almost without exception, centres for the diffusion of noisome influences, affecting, with more or less intensity, the immediate vicinity, deteriorating the sanatory condition of the surrounding population, commonly poor and dense, as recorded in the local reports

of the commissioners, and in a more remote degree vitiating the general atmosphere of the town, and thus becoming a nuisance to the inhabitants at large.

A second evil and nuisance, necessarily contingent upon the locality of slaughter-houses, however stringently supervised and regulated, in the midst of large and populous towns, is the quantity of animal ordure deposited upon the public streets and thoroughfares leading to such slaughter-houses, which, besides forming a most offensive addition to the ordinary surface-filth, excites and accelerates its decomposition. This evil is augmented in the ratio of the size of the town, and where, as in London, most of the surface-filth of the streets is washed down into the sewers, the continual passage of cattle, sheep, and pigs, in the neighbourhood of the intra-mural slaughter-houses, must materially increase the amount of that decomposing matter, the emanations of which are constantly escaping from the untrapped gulley-holes, to infect the atmosphere of the metropolis. Nor ought the occasionally fatal injuries, and the constant peril of life and limb, incurred by the inhabitants of large towns, the streets of which are so frequently traversed by goaded and over-driven cattle to be overlooked.

The practice of keeping pigs is a most odious nuisance. They are eminently stinking animals when in a state of confinement. In Birmingham there are 1600 pigsties: at Sunderland pigsties were said to be the *foci* of cholera. It is remarkable that, although there are by-laws in several corporate towns forbidding the accumulation of offensive matter occasioned by these sties, and by slaughter-houses, they are totally disregarded, as, for example, Newcastle-on-Tyne, Sunderland, Norwich, &c. The powers of courts-leet have been equally inefficient. This inefficiency manifestly arises from the reluctance of a party to bear witness against his neighbour. On this point of health-police, the commissioners observe:—

"By the introduction of better regulations on the subjects which we have now been noticing, it is to be hoped that the occasions for any interference of this kind will become less frequent; but as the most constant attention is required for the punctual enforcement of any laws or regulations, we are of opinion that an officer should be appointed in each town, who, in addition to other duties that may be placed under his charge, should be required to report upon any neglect on the part of the scavengers, or any infringement of rules for the prevention of nuisances, or of any other matter affecting the health of the inhabitants, and, if necessary, to commence proceedings in his own name, and as an informer on the part of the public, for the punishment of offenders before the magistrates Such an officer would receive much valuable assistance in the execution of his duties, and the public would be checked in their infringement of the law, if the police were directed to report upon any breach or neglect of it. These public servants, now generally a numerous and efficient body in each large town, although the constant witnesses of such offences, are not charged with the duty of reporting them to their superiors, or any officer empowered to correct them. It has been represented to us that this duty could be most efficiently and conveniently executed by the police, without any serious addition to their labours, or increase of expense to the inhabitants." (p. 79.)

The commissioners propose that the keeping of pigs, the collection of dung, and the establishment of slaughter-houses, within the precincts of towns, should be absolutely forbidden, and summarily abated. Perhaps,

as regards the keeping of pigs, it would be as well to make private interest subservient to public duty, and adopt such arrangements as will encourage the sale of pig-wash and offal-food to wholesale pig-feeders.

We think it would have been as well had the commissioners directed their attention to the condition of stables and mistals, or cow-houses, in large towns. The confinement of large numbers of cows in a limited space, and surrounded by the impure atmosphere of a large city, cannot but be injurious to the health of the animals, and render their milk unwholesome. There are reasons for suspecting that the use of such milk may induce phthisis. The extension of railway communication will now render the prohibition of cow-keeping in large towns feasible, as the milk may be brought from the country in a few minutes.

The abatement of the nuisances arising from the smoke and noxious emanations from mills and chemical manufactories, is proposed to be effected by requiring the combustion of smoke, arising both from mills and steamers, and such other appliances as may be requisite. The same power, the commissioners observe, that advances the chemistry of the arts and manufactures has multiplied the means of controlling and destroying offensive and injurious products from chemical operations. Taking, therefore, into consideration the facilities that now exist for preventing such noxious emanations, we feel convinced that much advantage will accrue both to the manufactory and to those who dwell in its vicinity by the right application and more careful investigation of the means which have already been put into successful operation for abating similar evils. The recommendation on this point is, that in cases where complaints shall be substantiated that the inhabitants of any house, street, or district, in towns, are injuriously affected by the noxious exhalations of any factory, power be given to the local administrative body to ascertain the cause of such exhalations, and to take legal proceedings for the abatement of the evils, in the event of such evils not being removed on due representation.

The necessity of an ample supply of water to towns, as the basis of efficient sanatory measures, is dwelt upon at considerable length by the commissioners. They observe, that the filthy condition which the abodes of the poorer classes so constantly exhibit, has produced a very general impression that they are not capable of appreciating the advantages and comfort either of personal or domestic cleanliness. The information derived from the investigations of the commissioners, and the evidence obtained through other channels, has convinced them that this is a most erroneous view of the feelings and wants of those persons; and they are most desirous to correct this impression, which, if it were well founded, would form a barrier to any prospect of improvement, and would render nugatory the recommendations that they may subsequently make for facilitating increased supplies of water. The general habits of the poor, with regard to cleanliness, must not be compared with a high standard; their daily occupations, and the nature of their employments, are such as frequently render personal cleanliness comparatively unattainable, and unless every possible facility is afforded for this end, they soon become insensible to its importance. The present difficulty, and the labour, after a hard day's work, of obtaining water, has a very great effect on their

economy, their habits, and their health. The obstacles to the maintenance of domestic or personal cleanliness soon produce habits of personal uncleanliness, which rapidly lower both the moral and physical condition of a whole population. Wherever, on the contrary, a better supply has been afforded, the poor have appreciated the boon by readily paying for it, and using it, to the manifest improvement of their health and comfort. In addition to the necessity of an ample supply for domestic use, the watering and cleansing of the streets and drains, and the ready extinction of fires are dependent upon civic hydraulics.

Many towns are most imperfectly supplied with water; but there is great difference in this respect. At Longton, a town in the Potteries, of 2000 houses, nearly all have a separate and constant supply. Bath is also eminent for its ample provision. The system of common stand-pipes for the supply of the poorer classes is strongly objected to by the commissioners. It is obvious that they must watch their opportunity of collecting water during the period that it is turned on, and those who are engaged in occupations from home necessarily lose their chance of getting a supply. This inconvenience is particularly felt in districts where women and children have much employment. When pipes are not laid on to each house, much labour is expended in fetching the water, and time is lost in waiting for their turns to fill their vessels. Where many persons are collected, as frequently happens, quarrelling naturally ensues for precedence, while serious injury is often inflicted upon the morals of the better portions of the population. The advantages of a system of constant supply are so clear, and have become so generally known since the inquiries of the commissioners have been made, that we believe in a few years it will be generally adopted.

In estimating the quantity of water to be supplied, the commissioners think that in all cases where an ample supply can be procured, it ought not to be calculated at a less rate than twelve gallons per diem for each individual of the population. The quantity required for public purposes will vary according to the situations and other peculiarities of towns. The water necessary for flushing the sewers will diminish as the natural advantages for drainage are greater; and the quantity used for watering the streets will vary according to the materials of which they are constructed. A more abundant supply may lead to the adoption of a system of washing the dirt from the foot-pavements, and other roads which are constructed of such materials as will admit of this mode of cleansing. The recommendation of the commissioners is, that it be rendered imperative on the local administrative body to procure a sufficient supply of water for these purposes. They strongly object to the competition of rival companies, because it eventually fails to attain the object for which it is begun. They rather recommend that where any independent body has the management of the supply of water, it be liable to comply with the demand of the local administrative body on equitable terms; that the local administrative body be empowered to purchase the interest in water-works, subject to the control of the Crown, whenever the proprietors are willing to dispose of them: that on the establishment of new companies it may be made a condition that the local administrative body be enabled

upon certain terms, and upon a rate of interest to be fixed; and that, with a view to economy, competition between water companies be discouraged as far as practicable. That the supply may be universal, and therefore afforded at an easy charge to each individual, the commissioners recommend that as soon as pipes are laid down, and a supply of water can be afforded to the inhabitants, all dwelling-houses capable of benefiting by such supply be rated in the same way as for sewerage and other local purposes; and the owners of small tenements be made liable to pay the rates for water, as they have recommended in respect to drainage.

The system of public baths and laundries, begun at Liverpool, has extended to London and other towns; another recommendation of the commissioners is, that every facility be afforded to furnish ample supplies of water to public baths and wash-houses that may be established for the use of the poorer classes.

The extinction of fires they propose to secure by a system of fire-plugs opening into mains filled with water at high pressure. As we have alluded to this point in our notice of the first report, we need not say more.

The regulations for buildings constitute an important part of health-police. It appears that the legislature has hitherto sanctioned but few local acts containing provisions for regulating the disposition of land as regards the width of streets, and the space to be allotted for houses; and restrictions are rarely placed upon the mode of constructing houses, either with a view to prevent the extension of fire, or to provide the occupants with those comforts and conveniences which are now considered necessary parts of every dwelling. The results of this neglect may be seen in the narrow courts, cellar-dwellings, and alleys, abounding in all our large towns, but pre-eminently so in Liverpool, Manchester, Preston, and Birmingham. To abate the existing evil, the commissioners recommend that, subject to proper control, the local administrative body be empowered to raise money for the purchase of property for the purpose of opening thoroughfares, and widening streets, courts, and alleys, so as to improve the ventilation of the densely-crowded districts of towns, as well as to increase the general convenience of traffic. With a view of insuring better external ventilation in future erections, they recommend that courts and alleys be not built of a less width than twenty feet, and that they have an opening of not less than ten feet from the ground upwards at each end; the width of the court being in proportion to the height of the houses. With respect to cellars, they recommend that after a limited period the use of them as dwellings be prohibited, unless the rooms are of certain dimensions, are provided with a fireplace, a window of sufficient size and made to open, have an open space in front, and that the foundations be properly drained.

The sad neglect of builders of cottage tenements in not supplying them with suitable privies, is strongly commented on by the commissioners, as inflicting extensive injury on the health, decency, and morals of the poor. The large number resorting to these places deprive them of all privacy. To save the space occupied by a privy in each house, a number of them, for the use of an entire population of a court, are commonly crowded together in one corner, and not unfrequently placed under other dwelling-

houses. The owners of the property are themselves also sufferers. Many instances occur where the walls of the adjoining houses are constantly wet with fœtid fluid, which frequently affects the atmosphere of the rooms so as to render it impossible to keep food for one single night without its becoming tainted. The walls of the houses receive considerable damage, and the foundations are completely saturated with the foul water that percolates through from the cesspools. The deterioration of property from this cause is very considerable. Added to this, a constant loss is incurred by the inability of tenants to pay their rents, from sickness, and not unfrequently from the impossibility of finding persons reduced so low in the scale of society as to occupy such abodes. The commissioners recommend that the local authorities should have a discretionary power of compelling the erection of privies where circumstances permit, and that all new houses be provided with them.

Fires are rare in the dwellings of the poor. Indeed, the tendency of the evidence they have collected on this point has led the commissioners to the conclusion that any general interference with the minute details of buildings for the poorer-classes in the great majority of towns in England and Wales is unnecessary. With regard to ventilation, their views are very different. It appears that architects and builders rarely make any provision in buildings constructed by them for a regular supply of fresh, or the removal of vitiated air, beyond what is afforded by the windows, door, and open chimneys. All practitioners know how perseveringly the poor (and the rich too) close every crevice that will admit cold air. Notwithstanding the apparent difficulties with which the ventilation of private dwellings is surrounded, a minute examination of the circumstances of the case has convinced the commissioners that no field of improvement holds out a more promising result than that which may be anticipated in future from the more successful ventilation, even of the humblest dwellings. But although the propriety of enforcing the introduction of a system of ventilation has been urged upon the commissioners, the general balance of opinion is adverse to that view. In this conclusion they concur; and although attaching the utmost importance to the introduction of some means of purifying the air in the abodes of the poor, they cannot recommend the adoption of compulsory provisions for this purpose, which, even if capable of enforcement, must lead to a most objectionable interference with the privacy of domestic life. The application of proper principles must be the result of a more general acquaintance with the subject on the part of individuals. Places of public assemblage and resort, especially schools, ought however to be under supervision. They recommend also that when, on the complaint of the parish medical or other authorized officer, any house or premises are in such a filthy and unwholesome state as to endanger the health of the public, and an infectious disease exists therein, the local administrative body have power to require the landlord to cleanse it properly, without delay; and in case of his neglect or inability, to do so by its own officers, and recover the expense from the landlord.

Medical practitioners conversant with the domestic condition of the poor must be well acquainted with the miserable state of the common lodging-houses. Their crowded condition is really awful. Instances

occur frequently where the beds are placed in tiers one above the other. Dr. Howard, who has had great experience from his connexion with the fever-wards in the hospital at Manchester, states that he considers the lodging-houses as the most frequent source of infectious fevers in Manchester, and he ascribes the permanence of the infection to the want of cleanliness in the beds, which are rarely purified, even after having been occupied by patients suffering from fever. In Scotland, examples have come under the notice of the commissioners in which the local authorities have power to license all the lodging-houses, and to issue regulations for their proper management. No such regulations have been adopted, it appears, in England and Wales, but they have been found to operate very successfully in Calton, which forms part of the borough of Glasgow. There the commissioners of police limit the number of persons to be accommodated in each house licensed; they require that the house shall be whitewashed periodically, and that in other respects due attention shall be paid to cleanliness; and above all, that immediate notice shall be given of the occurrence of any case of sickness. Under these regulations, many ill-conducted lodging-houses, the common resort of the infamous of both sexes, have been suppressed; while those now remaining, being licensed, are under the more direct control of the commissioners. Similar measures are recommended for adoption in England.

The appointment of a medical officer of inquiry is next adverted to by the commissioners. We subjoin this part of their report at length:

"The most eminent medical witnesses concur in declaring, that it is by the careful observation of the causes of disease and mortality operating upon large classes of the community, that the mode and extent of their operation may be ascertained, and the power of diminishing and preventing them be acquired. For this purpose the appointment of an officer, whose duty it would be to direct his undivided attention to such causes, would in our opinion be a public benefit, more especially to the poorer classes, and might be advantageously employed in making investigations into matters affecting the sanatory condition of the district under his charge. We therefore recommend that the local administrative body have power to appoint, subject to the approval of the Crown, a medical officer properly qualified to inspect and report periodically upon the sanatory condition of the town or district, to ascertain the true causes of disease and death, more especially of epidemics, increasing the rates of mortality, and the circumstances which originate and maintain such diseases, and injuriously affect the public health of such town or populous district." (p. 122.)

This part of the report concludes by recommending the establishment of public walks, and calling attention to the evils of intra-mural sepulture.

The drainage of the metropolis evidently occupied the particular attention of the commissioners, and a lengthened exposition of the early and existing laws regarding it is given, and will interest the metropolitan practitioner, to whom we particularly recommend its perusal. It is illustrated by two large and valuable coloured maps, the one showing the drainage and the other the water supply of London.

We subjoin the general conclusions of the commissioners, as containing most valuable truths:

"In submitting to your Majesty the measures we recommend for ameliorating the physical condition of the population inhabiting large towns and populous districts by improvements in drainage, cleansing, ventilation, and the supply of

water, we must again express our deep conviction of the extent, importance, and difficulty of the subject—a conviction strengthened by the continuance of our investigations. The most important evils affecting the public health throughout England and Wales are characterized by little variety, and it is only in the degree of their intensity that the towns exhibit the worst examples of such evils. Villages and clusters of houses inhabited by the poor are often under the influence of the same causes of disease, though their effect in such situations may be frequently rendered comparatively slight from the more free circulation of the external air. The vitiation of the atmosphere from over-crowding, and the absence of proper ventilation in individual apartments, produces in the rural districts the same disease that arises from the same causes in a town population.

"Though we venture to consider that the recommendations we now lay before your Majesty will, if sanctioned by the legislature, tend to diminish the evils into which it has been our duty to inquire, we cannot conceal from ourselves that in many cases a considerable time must elapse before permanent structural arrangements can be placed on that footing which their importance requires. Though those, who may be specially intrusted with the execution of the legislative powers recommended, will be enabled, by an earnest discharge of their duties, to accomplish great good, we still look to the co-operation of the public for important aid in the removal of those causes of disease to which the poorer classes of your Majesty's subjects are more peculiarly exposed; we do this the more confidently from the interest that has been recently manifested so generally on this important subject, and from the extent to which causes affecting public health have been made known through so many different channels, leading to the introduction of simple, economical, and highly beneficial improvements even in the humblest dwellings. With such co-operation, we have the greatest confidence that vast physical benefits will ensue, and that they will be accompanied by a corresponding improvement in the moral and social condition of the poorer inhabitants of large towns and populous districts." (p. 137.)

II. The Health of Towns' Bill. Before noticing the appendix to this report, we will draw the attention of our readers to the Health of Towns' Bill, brought in at the close of last session by Sir James Graham and the Earl of Lincoln. We are induced to adopt this as the more natural arrangement of our subject, because the enactments of the bill are mainly founded upon the recommendations of the commission. As it is a bulky document, extending to 118 folio pages, an abstract will, we are sure, be acceptable to our readers.

Clause 1. Limits the operation of the act to England and Wales, exclusive of the metropolis. Clause 2. Enacts that it shall be lawful for one of her Majesty's principal secretaries of state to nominate and appoint a sufficient number of fit and proper persons, of competent skill and science, not exceeding ———, to be inspectors, for the purpose of superintending and otherwise assisting in carrying the act into execution, and for performing the other duties required thereby. Inspectors are to inquire into and report on the state and condition of any town or district, particularly in respect to the state of the drainage thereof the quality, and quantity of water supplied to the inhabitants, the average amount of mortality among the population, and generally on the sanatory condition of such town or district, and on any other matters or things which may be deemed necessary and proper, for the purpose of enabling her Majesty to judge of the necessity and expediency of ordering the provisions of this act to be in force within any such town or district. They are also to

define the boundaries of towns and districts, for the purposes of the act; to divide them into wards, apportion the number of commissioners, &c. The Crown may make an order for enforcing the act. Every order in council to be published in the London Gazette.

Clauses 14 to 30 regulate the election of commissioners by the ratepayers; 31 to 41 regulate the selection of commissioners from town corporations, justices of the peace, and local boards of trustees. Clauses 42 to 72 regulate the qualifications and proceedings of the commissioners. Clause 73 provides for the appointment, subject to approval by one of her Majesty's principal secretaries of state, and with such salary as shall be approved of by him, to be paid by the commissioners, a person duly qualified as a civil engineer, to act as a local surveyor of the drainage and other works authorized under the provisions of the act. The surveyor cannot be dismissed without the sanction of the secretary of state. Clauses 74 to 84 provide for the appointment of a clerk, treasurer, and collector, and regulate their proceedings. Clauses 84 to 89 refer to the accounts and to the duties of the auditors, who are appointed by the Crown. 90 to 96 empower inspectors to visit towns and make reports; to attend the meetings of the commissioners; when called upon by the latter, through the secretary of state, "to prepare plans of any new works, additions, alterations, or amendments, that may be required for the drainage of the houses, streets, courts, open spaces, and roads, within any such town or district, including provisions for properly trapped drains or channels of conveyance for the removal of all waste water and refuse from the houses, and from the surface of such streets and places as aforesaid; and also to draw on such plan the most advantageous lines for main-sewers, and the best outfalls for clearing the whole area or district of surplus moisture, and for effecting the drainage of the subsoil; and also to point out the best arrangements that can be made for obtaining supplies of water, and report thereon in manner hereinafter provided; and also, if required, to examine and point out the most appropriate means and sites for the collection and sale of such filth and refuse, and its application as manure for agricultural purposes." The inspector must also prepare estimates for such proposed works, and advise the commissioners generally. Clause 96 provides that the report of every inspector shall be forwarded to one of her Majesty's principal secretaries of state, who shall take the same into his consideration; and if it shall appear to such secretary of state that the provisions and regulations of this Act have not been complied with, or that any of the commissioners have exceeded or contravened the powers and provisions of this act, then such secretary of state shall certify the same to the attorney-general, who shall proceed to enforce the law against such commissioners. This is an important part of the bill. Clauses 97 to 99 provide for the construction of maps of the several districts. 100 to 109 repeal or amend existing acts, and provide for compensations. Clauses 111 to 124 regulate the paving. 125 to 140 prescribe the management of drains, public and private sewers, and of cleansing. No houses are to be built without drains; vaults and cellars are not to be made under the street, without consent of the commissioners; gully holes to be trapped; drains, privies, and cess-pools to be kept in good order, and subject to the inspection of commissioners. The latter to

refuse to be vested in them. If the dung or soil of any stable, cowhouse, pigstie, &c. be allowed to accumulate for more than fourteen days, notice to be given for its removal within twenty-four hours; and if not removed the dung to be vested in the commissioners, and removed and sold by them. If any person shall allow any dung or filth to accumulate in his house for three days, after the scavenger has applied to remove it, he shall be subject to a fine of not more than forty shillings. If a certificate, signed by a medical officer of health, or by any two legally qualified practitioners, set forth any accumulation as injurious, notice for its removal within twenty-four hours is to be given by the commissioners, on failure of which it becomes their property, and is to be removed and sold by them. Similar provisions are directed against the removal of offensive matter during the day, and against stagnant pools, and other annoyances. Clause 174 provides for the erection of public conveniences. Clause 175 provides for the appointment of a medical officer of health, *which*, we observe, *is not compulsory*. We subjoin this clause and the two following, entire.

"And whereas, the health of the population, especially of the poorer classes, is frequently injured by the prevalence of epidemical and other disorders, and the virulence and extent of such disorders is frequently due and owing to the existence of local causes which are capable of removal, but which have hitherto frequently escaped detection from them, it is expedient that power should be given to appoint a duly qualified medical practitioner for that purpose; Be it therefore enacted, that it shall be lawful for the said commissioners to appoint, subject to the approval of one of her Majesty's principal secretaries of state, a legally-qualified medical practitioner, of skill and experience, to inspect and report periodically on the sanatory condition of any town or district, to ascertain the existence of diseases, more especially epidemics increasing the rates of mortality, and to point out the existence of any nuisances or other local causes which are likely to originate and maintain such diseases and injuriously affect the health of the inhabitants of such town or district, and to take cognizance of the fact of the existence of any contagious disease, and to point out the most efficacious modes for checking or preventing the spread of such diseases, and also to point out the most efficient means for the ventilation of churches, chapels, schools, registered lodging-houses, and other public edifices within the said town or district, and to perform any other duties of a like nature which may be required of him; and such person shall be called the medical officer of health for the town or district for which he shall be appointed; and it shall be lawful for the said commissioners to pay to such officer such salary as shall be approved of by one of her Majesty's principal secretaries of state.

"And be it enacted, that whenever it shall be lawful for any coroner to summon medical witnesses, and to direct the performance of a post-mortem examination, under the provisions of an act passed in the session of Parliament held in the sixth and seventh year of the reign of his late Majesty King William the Fourth, intituled 'An Act to provide for the Attendance and Remuneration of Medical Witnesses at Coroners' Inquests,' it shall be lawful for such coroner to issue his order for the attendance of the medical officer of health for the town or district within which any such inquest shall be held, and to direct the performance by such medical officer of a post-mortem examination, with or without analysis of the contents of the stomach or intestines, without fee or reward; and any provisions contained in the said Act for imposing any penalty on any medical practitioner for any disobedience of any order of such coroner shall be taken to extend and apply to such officer of health.

"And be it enacted, that when it shall appear to the said commissioners, either from the report of the officer of health or otherwise, that any house, or the premises adjoining to any house, is or are in such a filthy or unwholesome condition, that the health of the inmates or of the public is thereby affected or endangered, or that the whitewashing, cleansing, or purifying of any house or part thereof, would tend to prevent or check infectious or contagious disease which may have occurred therein, it shall be lawful for the said commissioners from time to time, if they shall think it expedient, to order the owner or occupier of any house or dwelling, or part thereof, within any town or district, to whitewash, cleanse, and purify the same in such manner and within such time as the said commissioners may deem reasonable; and if such owner or occupier shall not comply with such order, he shall forfeit and pay any sum not exceeding *ten shillings* for every day's neglect thereof; and it shall be lawful for the said commissioners to cause such house or dwelling, or any part thereof, to be whitewashed, cleansed, and purified, and to recover the expense thereof from such owner or occupier; provided that when, on account of the poverty of such owner or occupier, or other special circumstances, it shall appear expedient to the commissioners to pay the whole or any part of such expense, it shall be lawful for them so to do."

Clause 184 is also so important that we give it at length too:

"And be it enacted, that it shall be lawful for the said commissioners, and they are hereby required from time to time to make by-laws as they shall think fit, for all or any of the purposes following; (that is to say,)

"For preventing nuisances and annoyances in any streets, or near thereto, and for effecting cleanliness therein.

"For making regulations for registering and inspection of slaughter-houses and knackers' yards, and for keeping the same in a cleanly and proper state, and for removing filth therefrom at least once in every twenty-four hours, and for requiring that they shall be provided with a sufficient supply of water.

"For regulating the manner of keeping swine, and for preventing the keeping thereof in any dwelling-house, and for describing the limits in such town or district within which it shall be lawful to keep the same.

"For the punishment of persons selling unwholesome meat, and for seizing and condemning the same.

"For regulating the duties of scavengers, and for regulating the management of public privies.

"For making regulations for the registering of lodging-houses, and for maintaining cleanliness therein, and keeping them in a wholesome condition.

"For laying down rules for cleansing filthy and unwholesome dwellings, and to ascertain and fix what pecuniary penalties shall be incurred by persons breaking such laws.

"Provided always, that no such last-mentioned penalty shall exceed for any such offence the sum of five pounds, and in the case of a continuing nuisance the sum of ten shillings for every day during which such nuisance shall be continued or unremedied."

These by-laws are to be approved by the secretary of state, and published in the newspapers.

The supply of water and the extinction of fire are provided for in clauses 190 to 251. The power granted to the commissioners for these purposes are of the most extensive description; they constitute them, in fact, a little civic parliament, possessed of more authority than corporations or other existing public bodies.

Clause 250 merits notice, as it provides for the establishment, by the commissioners, of a "humane apparatus, with all such matters and things as may appear to them necessary, to assist in searching for drowned per-

sons, and restoring animation to persons apparently drowned, and to employ and reward assistants therein, in such manner as the said commissioners shall deem advisable."

The remaining clauses of the bill are of a financial character, and need no special notice, except that, by clause 254, commissioners are authorized to lay a water rate.

We can scarcely hope or wish that this bill will or may give satisfaction to the profession. The science of medicine holds apparently a very subordinate position in the opinion of its concoctors. The secondary appointment of the officers of health is not even imperative on the local authorities, as it undoubtedly ought to be; while there is no provision made for those higher applications of medical science to political economy, the practicability and propriety of which we have from time to time demonstrated.

III. Appendix to the Report. This, like the appendix to the first report, contains an immense amount of information on matters relating to public hygiene. This information is principally comprised in the local reports of those members of the commission who were itinerant. It is extremely difficult to give a condensed analysis of these reports. In the main features of sewerage, cleansing, and structural arrangements, the towns of England closely resemble each other, and consequently the facts related by each commissioner do not widely differ. In some particulars, however, the towns do differ widely. The manufacturing have few public places of recreation; Birmingham, for example, has no public walk, and there is no place in or near it where the working classes can bathe. The same remark applies to Stourbridge. Mr. Slaney made a discovery here:

"In the immediate vicinity of this neat and flourishing town is the large, straggling, and populous hamlet of King's Swinford, called by the appropriate name of 'Lye a waste.' These waste people are almost all nailers; their houses, or rather huts, are of all forms, grouped in two, three, or more together, over a wide space. Filthy open dithecs, heaps of rubbish and dirt, surround their neglected habitations; disorder and poverty appear on all sides; there are no regulations or attempts at improvement." (p. 209.)

The following observation, by the same gentleman, has a general application, and ought to be seriously considered. Referring to Droitwich, in Worcestershire, Wellington, in Shropshire, and other small towns, he remarks:

"It is matter of remark also, that in many of these places a colony of poor Irish have planted themselves, who are *fast increasing* in numbers: their habitations are habitually dirty and neglected; the frequent source of disease; and the inhabitants are generally, from their frequent quarrels, and the bad example they afford to others, a great trouble to the vicinity." (p. 219.)

The following is Mr. Slaney's summary of the evils arising from the want of proper sanatory regulation:

"1st. Shortening the duration of the lives of the community.

"2d. Disease, suffering, and inability to work on the part of many who survive; the causes of great cost to the country.

"3d. Crime, theft, and the loss of property, which the police constantly point out as arising from these neglected classes.

"4th. Riots, disturbances, and drunkenness, which may generally be traced to the same class of persons, often to the same places.

"5th. Great injury to the education of the poor, which is constantly neutralized in its good effects by the neglect and evils they see around them. The same observation applies to the inestimable advantages of religion, and attendance on religious worship.

"6th. Great discontent in some, and sluggish apathy in others, producing recklessness of conduct, indifference, and want of attachment to the institutions of the country.

"7th. The loss to the humbler classes of the cheapest, best, and most enduring pleasures, viz. those arising from the kindly influence of the domestic relations between husbands and wives, parents and children, brothers and sisters,—that pure source of happiness derived from mutual kindness, attachment, and good offices, is, amid the hardening and disgusting scenes described, almost destroyed." (p. 223, seq.)

Mr. Slaney estimates the pecuniary loss consequent upon these evils: the following is his bill for a town with a population of 20,000:

"Loss of the labour of those prematurely cut off, as before stated, per annum	£ 6500
Loss during illness of those who recover	1000
Their support in illness	1000
Cost of crime and vice arising from the same causes stated	6250
	£14,750"

The report of Mr. Slaney is followed by that of Sir H. T. De La Beche on Bristol, Bath, and other towns in the West of England. It is illustrated by a geological section and map of Bristol, showing the common seats of fever and cholera, and by a geological map of Bath.

Dr. Lyon Playfair assisted in the report for Bristol. A peculiarity of Bristol is in the existence of parochial conduits—one or two of ancient foundation; there are also many public wells. It appears, however, that there are few towns so inadequately supplied with water. In other respects the sanatory condition of Bristol is low; the buildings and drains badly constructed, slaughter-houses and dead-yards scattered through the town. Indeed, Bristol, with a climate known to be mild and salubrious, enjoys the unenviable celebrity of being the third most unhealthy town in England. The sanatory condition and arrangements of Bath present a favorable contrast to those of Bristol.

Mr. Payne, surgeon, of Frome, supplied Sir H. De La Beche with a report on the health of that town, from which we extract the following statement, as interesting to statisticians.

"There is one great evil which I see adverted to in the interesting report furnished by Mr. Chadwick, and which I know prevails to a great extent as well in this as in other manufacturing towns, and which I consider to be of serious importance, viz. the calling children still-born who have been born alive, but from neglect or intentional ill-usage have soon ceased to exist. This is one great source of the incorrectness of tables relating to statistical investigations. I do not see how these can be correct, when registration takes no cognizance of still-born children, who are, undoubtedly, as much a part of the population, although from neglect or some difficulty in delivery, probably mechanical compression, are to all appearances dead, as others, who, not being exposed to such casualties, are born alive. There can be no question that gross neglect is practised by the loose women who act as

midwives to the poor, which adds greatly to the amount of mortality amongst infants, and the consequent evil (generally overlooked) of a quick succession of unhealthy and rickety children. I have frequently seen newly-born infants put aside as still-born that have been lustily formed, and who, in the space of half-an-hour, have astonished the bystanders by loud cries; and others, who, with the ordinary attention and application of the usual remedies generally resorted to on such occasions, have been speedily resuscitated. Indeed, so ignorant and unfeeling are the majority of the poor women of this place, that they would rather see their infants perish than have recourse to any measure, and even censure the adoption of means calculated to recover such as are still born. I have very frequently met with such myself, and I can assert that it is the common practice; and, from what I have witnessed repeatedly, I can fully corroborate the printed evidence given by other registrars, as reported in Mr. Chadwick's investigations." (p. 293, seq.)

This is really an appalling statement.

Mr. Payne being both union officer and registrar has had opportunities for a careful collection of facts. These he has arranged, and states as the result that "in Frome, 1 in 5·5 of the total deaths was from phthisis, and 1 in 21·8 from pneumonia; 1 in 10·7 was from continued fever and typhus; 1 in 11·8 from debility, chiefly new-born infants; 1 in 10 from diseases of the brain and nervous system; 1 in 23·4 of dropsy; 1 in 49·3 of accidents; and 1 in 5·5 of age."

The town of Swansea is remarkable for copper works, which impregnate the air with sulphureous, sulphuric, and arsenious acids. In consequence of these gaseous products, vegetation is destroyed around the works, particularly westward, towards which the prevalent winds more frequently drive the "copper-smoke," so that on the exposed side of Cilfay Hill no plant can grow, and the very soil is washed from the subjacent gravel and rock, from the absence of protecting vegetation, and where, moreover, the glass in the windows of the town is corroded from the same causes. It becomes an especial object of interest to see how far the condition of the atmosphere may affect the health of the organizations within its influence. Many plants cannot be grown within the range of these vapours even where they become, as it were, diluted with pure air: the colour of the *convolvulus major* has been known to be changed to red, after a few hours' driving of the "copper-smoke," at the distance of two miles from the works; and the horses and cattle that feed upon the grass, where it can grow within the range of much of this smoke, are affected with a great thickening of the knee-joints, and their teeth suffer, so that they must be frequently removed from such localities to preserve them.

Although it might be considered that vapours which clearly caused such destruction to vegetation could scarcely fail to injure the health of persons coming within their range, the general impression seems to be, that these vapours by no means produce the serious consequences to health that might be supposed. It is stated, that in proportion as the copper-works have been erected, ague, which once prevailed in the low grounds near the course of the river, to the northward of the town, has disappeared, so as now to be little known; and it is thought that the copper-smoke greatly counteracts the injurious effects to health which would otherwise arise from the neglected sewerage, drainage, and scavenging of the town.

The report on the sanatory condition of the large towns in Lancashire is, we think, the most important document of the kind we have ever

perused. It is divided into two parts: the first discussing the civic structural arrangements of the towns situate in the manufacturing emporium of the world; the second determining the results of those arrangements on the health and happiness of the people. In the first part, we have a repetition of the odious and abhorrent details noted before, indicating the same want of hygienic regulations as is observed in other towns. Stagnant pools, unsewered streets, dead dogs and cats, filth of all kinds, cottages stinking like privies,—all usque ad nauseum. But here is something of a novelty in the reeking monotonous gurgle. It is the statement given by a shopkeeper of Manchester, whose rooms, in which he had resided some years, overlook a slaughter-house for pigs:

"What occupation does your neighbour pursue?—He kills pigs, which he gets over from Ireland. Often the pigs, in coming over in the packet, die, and I have seen as many as thirty dead pigs at a time brought into the yard. They are thrown under that shed there, until there is time to cut them up, and by that time I have seen the maggots fairly dropping out of them. Then they are cut up, and I believe are made into salt bacon or sold for sausages. The entrails of such pigs are generally too far gone to be of use, and they are thrown into the dunghill. When the dunghill is stirred up to be taken away, oh! sir, the smell is awful: we are forced to shut our windows and doors, and stuff pieces of cloth into the keyholes; but all this does not keep it out. The entrails of the live pigs killed in the yard are boiled and sold, and give out a very bad smell, but nothing like the others.

"Have you not complained of this nuisance?—Yes, we have; but we were told it was no use complaining, for doctors agreed that these smells were very healthy. (!) Besides, the owner of the yard is a very good neighbour, and tries to keep things as clean as he can, but his occupation beats him in that.

"Is that your only child?—Yes; but it is a poor sickly thing for 15 months old. I thought at one time these smells might have something to do with its being so poorly, but that can't be if they are healthy." (p. 374.)

Who will blame an abstemious Hebrew after reading this?

The details regarding the lodging-houses in Manchester and Liverpool, already referred to, although presenting another aspect, have the same distressing character. Dr. Playfair says,

"I will not dwell upon scenes which I myself have witnessed on entering these dens during the night; but their nature may be easily conceived, and their immoral tendency rendered obvious, when it is considered that the lowest mendicants, thieves, and prostitutes, make these houses their usual abode; and with these abandoned persons the travelling artisan and his family are thus brought into close contact."

The results of these and the other hurtful agents alluded to, are dwelt on at length in the second part. In particular, an elaborate table is given of the pecuniary damages inflicted by them. Mr. Chadwick was the first to show that an excessive mortality from disease and hunger does not retard, but rather accelerates the increase of the population. The proportion of remarriages, Dr. Playfair observes, affords a capital test of the truth of this view, for it is obvious that the death of one of a married couple must take place at an early age, if the relict again marries. The two extremes in Lancashire form powerful proofs of this position; the proportion of remarriages to 100 marriages being only 5·52 in Ulverstone, the most healthy district, and as much as 14·27 in Liverpool, the most

unhealthy district. In the ten towns of Lancashire in which the average age at death of adults is the highest, the proportion of remarriages to 100 marriages is 10·41; and in the eleven towns in which the average age at death of adults is the lowest, the proportion is as great as 12·99.

As the premature loss of one member of a family enables the relic to remarry, in this point of view, the centesimal proportion of remarriages becomes a natural index of premature adult mortality. The early death of a male parent in the class of operatives, occasions, in almost every instance, a pecuniary burden upon the surviving relatives, or upon the public at large, part only of which is exhibited by the large amount of widowhood and orphanage dependent on the poor-rates. The table we have referred to gives the general result, without taking into consideration the diminution of the physical and mental energies of the survivors from sickness, and other depressing causes; without estimating the loss from the substitution of young and inexperienced labour for that which is skilful and productive; without including the heavy burdens incident to the large amount of preventible widowhood and orphanage; without calculating the loss from the excess of births, resulting from the excess of deaths, or the cost of the maintenance of an infantile population, nearly one half of which is swept off before it attains two years of age, and about fifty-nine per cent. of which never become adult productive labourers; and with data in every case much below the truth,—Dr. Playfair estimates the actual pecuniary burdens borne by the community in the support of removable disease and death in Lancashire alone, at the annual sum of five millions pounds sterling!

Opiates are extensively used by the lower classes for the purpose of dosing their children, not only in the form of Dalby's carminative and other quack medicines, but in that of pure laudanum. There is an estimate that one druggist supplies 700 families weekly with the "medicine." Only allowing one ounce to each family, Dr. Playfair observes, after some remarks of this kind,

"Thus we have *three* druggists, all of acknowledged respectability (!) in one district of Manchester, selling respectively five and a half, three and a half, and one—in all, nine gallons weekly, two of them testifying that 'almost all the families' of the poor in that district habitually drug their children with opiates; and the third, after a lengthened examination of all the customers who attended a pawnbroker's shop, kept by a relation of his own, giving as a statistical result, that five out of six families in his district were in the habitual use of narcotics for children." (p. 455.)

In Rochdale and other towns, similar evidence is given. "A. B. is in disgust at the assertion that the practice prevails to a *great extent* in Rochdale, stating as the result of his inquiries, that out of ten families of the operatives, *not more than six* are in the habitual use of opiates;" while another druggist, who also had abundant opportunities of knowing the custom, considers "one third of the working people used these sleeping-stuffs." At Clitheroe, 4,000 poppy heads are annually sold for making "sleeping tea" for children. In Rochdale, the poor, finding Godfrey and Dalby (society will heavily curse these men some day) too expensive, have got into the practice of buying at a time a pennyworth of solid opium, and a pennyworth each of anise and carraway seeds; these they boil with

sugar and treacle, and dose the children with the mixture. And so Dr. Playfair goes on with fact after fact—all alike, yet all different; dwelling upon them with a praiseworthy but sickening minuteness. The little victims, with a wonderful precocity, are described as stretching out their tiny hands for the bottle, "for they know it, and when they get it, drink it as eagerly as a drunkard empties his glass!" Equally deplorable is the description given by a druggist, who says, "I have seen the little children in the shop put the neck of the bottle in their mouths and bite the cork, so fond are they of the preparation; for coming to the shop so often, they know the bottle!"

Dr. Playfair found it difficult to write as calmly as became a commissioner upon such facts as these.

Our fast diminishing limits warn us, however, to hold our hand; and leaving some important statistical details, as to the hygiene of morals given by Dr. Playfair, pass on to the second volume. This comprises reports on the condition of the towns in the north and midland counties of England, by Dr. Reid, Professor Owen, Mr. J. R. Martin, and Mr. Smith of Deanston. There is also an elaborate report on the condition of the city of Exeter, by Dr. Shapter. The notabilia in this volume are, firstly, that Dr. Reid in his report on the northern coal-mine district, sets forth the principles of ventilation as applicable to dwellings, mines, factories, churches, schools, &c.; illustrating his views by twenty-one coloured lithographs, containing nearly one hundred figures! Dr. Reid shows also the intimate connexion between cholera, typhus, and the ordinary sources of malarious emanations.

Mr. Martin, in his report on the state of Nottingham, Coventry, &c., sets forth certain questions which he addressed to Sir James Macgregor, as to the condition of the recruits obtained from the manufacturing districts. The answers are striking, and corroborate similar observations made in France. (Vide our Fourteenth Volume, p. 457.) Sir James observes,

"It is a matter of every day experience that the physical and moral qualities of the country recruits are of a much higher order than are met with in men who have been reared in large towns. In the former, the bodily and constitutional powers are in general much greater than in the latter. There is much less proneness to disease in countrymen, while the power of withstanding the inroads of sickness is in them considerably greater, and they are far more capable of enduring the fatigues and privations to which soldiers may be subjected than recruits of the latter class, in whom the stamina at best have been but imperfectly developed, and whose general health has been depreciated by the nature of their occupations and habits,—impure air, want of proper exercise, and the many debilitating and morbific influences inseparably connected with the condition of artisans in large towns.

"2d. For these and similar reasons, there exists a marked superiority, both physically and morally, in the capabilities for foreign service of the country recruits over those possessed by townsmen." (vol. ii, p. 131.)

About one fifth only of the country recruits are medically rejected, while nearly one half of the civic are found unfit.

Mr. Martin notices the one-eyedness of honest folk, when giving a sanatory character of their own locality.

"The observations of lay persons on questions affecting public health are of great importance; indeed, the experience of observant laymen has not been suf-

ficiently regarded by professional inquirers. But, admitting its importance, there are yet some circumstances connected with lay evidence requiring that we should sometimes receive it with reserve. One of these drawbacks consists in the impression so common amongst citizens, that *their* city or town forms an exception to the rule of unhealthiness; indeed, they receive any intimation to the contrary as an impeachment (*stigma* is the term they use,) of the character, moral as well as physical, of their favorite locality; and so strong is this feeling, that men of the highest integrity and intelligence will be found continually to deceive themselves, and, unintentionally, they will endeavour to deceive others on such points. . . . . . But laxity and want of accurate observation is not exclusively confined to the lay community; for sometimes we have an exordium of the following character from quarters whence a more exact information might be expected: 'This town, I consider, upon the whole, one of the healthiest towns in the kingdom.' Again, of another town (like the last), most unhealthy, we have it reported, that 'the general condition of the town is comparatively healthy: at the present time it may be considered in a very healthy state.' It is but justice to state, however, that, in general, throughout the entire extent of my personal examination, I found the medical witnesses alike intelligent and humane." (vol. ii, p. 129, seq.)

Mr. James Smith notices another "element" of hinderance; it is singular that he alone makes the observation.

"There is another element which presented itself as a serious barrier to the carrying out of local works by the authorities as at present constituted—namely, party divisions. At Hull, when I visited the town, and paid my respects to the mayor as a public officer, without knowing or thinking it my duty to notice of what political party he was, and made inquiries of the municipal officers, and perambulated the poorer districts with, I found that this very innocent act of mine was regarded, by very respectable persons, but an anti-corporation party, as 'taking a side.'" (Ibid. p 163.)

The most remarkable portion of Mr. Smith's report is his essay on the application of sewer water to agricultural purposes. He proposes to irrigate the arable land, around our large towns with it, as a substitute for guano, using pipes and steam power for distributing it. In making an estimate of the cost, Mr. Smith observes:

"I have confined the district to be supplied to an area of four square miles, containing 2560 statute acres I have supposed the whole to be laid off in 10-acre fields, and have put down the position for the service-pipes in such order, as to effect the distribution of the water over each area of 40 acres by a hose-pipe, 312 yards long The main piping I have assumed at the length of the side of the square, with one mile added to clear the suburbs of the town. The main is taken at 12 inches in diameter, which will be sufficient to pass the quantity of water required for a great extent of land; and the service pipes are taken at 4 inches diameter, which is very ample, as never more than two or three jets will be playing from one service pipe at the same time. The main pipes I have estimated as of cast-iron, the service pipes as of fire-clay, as I have ascertained that such can be had at one third of the price of cast iron pipes, and I have seen such proved to a pressure of 600 feet. They will certainly stand a pressure of 300 feet. These pipes I suppose to be sunk two feet under the surface, with a plug opening for attaching the hose for each four fields or 40 acres. The hose pipe and jet must in all cases be worked by persons employed by the sewer-water establishment, who will apply the liquid at such times, in such manner, and in such quantity as the farmer shall desire, under proper regulations. Part may be delivered by jet, part for purposes of irrigation; and it is evident that any farmer would be greatly benefited by appropriating a portion of his farm as meadow, to be irrigated by the sewer-water for the production of early and abundant crops of grass." (Ibid. p 176.)

From various data he calculates, that the cost of manuring one acre with sewer-water, in strength equal to 2½ cwt. with guano, costing 1*l*., or to fifteen tons of farm-yard manure costing 3*l*., will amount only to 12*s*. 9*d*. The net income from the sewer-water of a town is estimated at nearly 1*l*. per head.

"Taking a general view of the subject, we may safely assume a clear revenue from the sewer-water of all towns of 1*l*. for each inhabitant, either in a direct money return, or partly to the inhabitants in a reduced price from the increased abundance of produce; and it is obvious, that such income, annually accruing, will provide a sufficient fund for the improvement of all towns, in a manner corresponding to the most enlightened views with respect to sanatory regulation and improvement of the present time, and will remain as a source for accomplishing such further improvements as science and practical experience shall from time to time suggest." (p. 478.)

An insuperable bar to the accomplishment of Mr. Smith's views will be found, we suspect, in bucolic stupidity.

We here terminate our notice of the labours of the health of towns commission. The four volumes present an invaluable repository of facts bearing on public hygiene, collected at a great expense, by most competent observers, from some of the largest and most important towns and cities in the world. As books of reference they are essential to the practitioner's library; none should be without them; and no practitioner should neglect their perusal, because, sooner or later he will certainly be called upon to act in his professional capacity, in respect of one or other of the numerous matters and things bearing on the public hygiene of his country.

ART. III.

Lectures on the more important Eruptive Fevers, Hemorrhages, and Dropsies, and on Gout and Rheumatism, delivered in the University of Pennsylvania. By N. CHAPMAN, M.D., Professor of the Theory and Practice of Medicine, &c. &c.—*Philadelphia,* 1844. 8vo, pp. 448.

THE present volume is precisely of the character of the former one, by the same author, reviewed in our Thirty-seventh Number. It contains much good matter of various kinds, the results of great experience and reading, and many sound practical observations; but the whole is enounced amid such confusion of style and arrangement, that the task of perusal, and even of comprehension, becomes irksome and difficult. But we shall not dwell on these defects, but proceed, as on the former occasion, to glean from the heterogeneous mass such things as may be most interesting and useful to our readers.

SMALLPOX. Dr. Chapman writes with some authoritativeness of the comparatively modern origin of this disease. "From the historical evidence," he observes, "it results that the disease, probably, was developed at the siege of Mecca, for the first time, though under what peculiar circumstances generated we have no real information." But if we allow that the disease appeared from time to time during the three centuries which elapsed between the time of Mahomet and Rhazes, although no clear

description of the phenomena which it presented has come down to us, it surely requires no stretch of credulity to suppose that undescribed visitations of the disease may have existed during many centuries previous to the birth of Mahomet. Ozanam, no mean authority, considers the description of Aëtius sufficiently characteristic of the disease; and whatever diversity of opinion may exist regarding the time when Aëtius flourished, no one will place it later than the fifth century. The description by Eusebius of a febrile distemper, as occurring A. D. 311, rendering thousands of women and children blind,—the reference of Hippocrates to some fevers which are pustular and dreadful to behold, *οἱ δὲ περιφλυκτώδεες ἰδεῖν δεινοὶ*,—and the tradition of various eastern nations regarding the ambiguity of the disease, independent of allusions in various classical authors to some at least similar affection,—give plausibility to the opinion, that a wish to connect the ideas of moral and physical evil, rather than a studious regard to accuracy, led authors to fix the origin of the disease on the era of Mahomet, and authorise us in discountenancing all dogmatical attempts to settle a question involved in obscurity, which no illumination of modern learning can be expected to dissipate.

Of the circumstances attending the rise and spread of smallpox, Dr. Chapman, from his own experience, writes as follows:

"In 1823, when smallpox appeared in Philadelphia, after a long interval, it could not be traced to any imported or derivative source of contagion. Cases sprung up, as it were, spontaneously, at a distance from each other, independent of any probable intercourse, wearing universally a most formidable character, and the failures of variolation, and especially of vaccination, were numerous, with some few examples of the disease previously had in the natural way, affording no security. As further proof of the dominant epidemic influence at the time, it may be said, that in the whole compass of our experience, never was exhibited such a tendency to cutaneous affections—every disease, whatever might be its nature, displaying in its course some eruptive appearance, and often of the most anomalous character and aspect." (pp. 25-6.)

Notwithstanding the plausibility of the conjecture which such facts countenance, that the contagion of smallpox is sometimes generated anew, our author is not disposed to adopt it.

"To me," he observes, "it seems probable that the semina of contagion, like those of plants, or ova of animals, and especially of insects, may remain dormant for an indefinite period. As the latter are hatched into existence by a proper degree of temperature and other propitious circumstances, so it is required, to bring the former into activity, a peculiar constitution of atmosphere. We are not wanting in proof that the seminal principle, in each of the instances cited, will endure for a long term of years in a latent state, waiting, as it were, for the vivifying impulse to be supplied: and may it not be equally true in regard to contagion? The musquito, the locust, not to enumerate more examples, disappear for a protracted season, having deposited their eggs, to be awakened into life at some favorable conjuncture. Every agriculturist is aware of the reversions of certain plants, at remote and irregular periods, the seeds of which must have remained in the soil. As clearly does it seem, that the material of this, and all other contagious diseases, is governed by a similar law. Much reliance, I am aware, has been placed on the doctrine of equivocal generation, in the explanation of some of the preceding phenomena; but can it be reasonably credited, that any fortuitous combination of elements, which this doctrine supposes, is productive of such definite results?" (p. 27.)

We cannot avoid remarking that some of the analogies noticed in the above paragraph possibly involve truths calculated to illustrate the phenomena of epidemic diseases. Among all the hypotheses yet constructed to explain the origin of some of the most remarkable epidemics, we believe it may be said that the animalcular is the only one capable of being reconciled with the singular phenomena which attend the visitation of these diseases. Dr. Chapman concurs with Heberden and Haygarth in the opinion that the contagious property of variola does not occur till after the appearance, and perhaps maturation, of the eruption; but thinks it probable that, as in the instance of the vaccine affection, the contagion may exist in the vesicle as well as the pustule. He differs from those who believe the poisonous effluvia may escape from the lungs as well as the skin, thinking it evident, that, as the lungs are destitute of the eruption, they cannot concur in the generation or emission of the contagious halitus.

The insusceptibility of some individuals, and even families, to the contagion, is worthy of notice:

> "Foderé mentions the very remarkable instances of those of both of his grandfathers having escaped, and that he himself, though in advanced age, and often subjected to its contagion, had never had the disease. It has, indeed, been calculated, that one in fifty has such a constitutional immunity. But this estimate seems too large, and in no instance is an exemption from it to be confidently relied on. Examples are numerous of persons who, after escaping for a term of years, where affectability being awakened by some mysterious change of condition, the disease attacked, and for the most part, fatally." (pp. 24-5.)

His suggestions on the treatment of smallpox do not claim any particular comment. He justly observes, that it must be adapted to the condition, whether inflammatory, congestive, or mixed. He attaches considerable importance to the use of emetics where not distinctly contraindicated in variola, as in all eruptive fevers, and relies mainly on laxatives and a cool temperature, employing bleeding or cupping when the fever is of an inflammatory character. We differ from the professor as respects his recommendation of the use of diaphoretics, such as antimony and acetate of ammonia, remedies which we consider calculated to increase the quantity of eruption. It is an interesting fact, that the cooling practice in smallpox was distinctly recommended by Rhazes, but was superseded by the dogmas of a false pathology. John of Gaddesden recommended the patient to be wrapped up in red cloth. Sennertus, in the fourteenth century, urged the importance of a warm chamber, and Diemerbroeck advised his patients to use the thickest blankets, and avoid the risk of change of linen. It was reserved for the sagacity of Sydenham to rectify the error, and dictate a return to the ancient practice. Dr. Chapman, considering that, especially in the confluent state, the condition of the skin resembles that of a burn, occasioning nervous inquietude and death, in a similar manner, suggests the application of flaxseed mucilage, and in the asthenic condition, camphorated or Kentish ointment.

Our veteran author dwells on the importance of preventing the marks which are so detrimental to female beauty. After enumerating the numerous suggestions and practices noticed in books, as mercurial ointment, camphor ointment, calomel ointment, a corrosive sublimate wash, covering the face with gold leaf, puncture of the pustules, &c., he adds:

"My own practice has been to subdue inflammation as far as possible by cooling lotions, to open the pustules as soon as they fill, and wash them with milk and water, taking care also as far as posssible to keep the face covered. It appears, however, that the most effectual means is the exclusion of light. Experiments made some years ago, at New Orleans, if they can be relied on, and I know of no reason why they should be distrusted, are very satisfactory on this point.

To try the effect of this expedient, a certain number of patients, during the eruptive and maturative stages of the disease, were confined in a dark ward of an hospital, and not a pit or scar, or other deformity of the skin, was left, though some of them had the disease most violently, even in the confluent form. These experiments were originally performed by Dr. Picton, a graduate of the University of Pennsylvania, and were contained in his inaugural thesis, which I had published on account of its merits. Notices of their confirmation I have lately seen in the medical journals of several of the European countries." (pp. 43-4.)

Medicus, Meige, Professor Schroider of Gottingen, Odier, of Geneva, Schevanke, and some other respectable authors, are of opinion that inoculated smallpox is not contagious.

"'To this question,' says Walkinson, 'I have paid particular attention since the establishment of a dispensary for general inoculation, and can with truth affirm that not a single instance has occurred in that charity in which the contagion was spread by an inoculated patient. Where the chance of spreading it has been apparently great, I have been very strict in my inquiries.' He adds, 'that some inhabited narrow streets, or little courts, and ground floors, the doors of which were kept open, and though surrounded by persons obnoxious to the disease, and especially by a set of children, who continually played before the houses, a few yards only from the sick, all escaped infection.' Exceedingly strong is the testimony of Mr. Holwell, at least in regard to India. Living in that region for thirty years, and during the period inoculating multitudes, he affirms positively that it never spread the infection, as is commonly imagined in Europe." (pp. 45-6.)

Dr. Chapman observes that this opinion is supported by various analogies, especially by that of the vaccine affection, which,

"While contagious, is totally void of infection, and as it is probably modified smallpox, it may be rationally conceived, that by inoculation such a change is wrought in the latter disease as to bring it into the same category." (p. 46.)

The professor claims for his own country much credit in the establishment of the practice of inoculation.

"In 1721, when it had only been adopted in England in a few instances, and these chiefly malefactors, whose punishment by death was commuted, and a load of prejudice still existed against it, Boylston of Boston, a name deservedly high in the annals of American medicine, against the unanimous opinion of the other physicians of the city, and in direct contravention of an edict of the municipal authorities, carrying with it heavy penal consequences, had the intrepidity to inoculate two hundred and eighty-six persons, of whom six only died. The disease was prevailing extensively at the time, and out of 5759 persons, who took it in the natural way, 884 perished.

"To escape from the conviction of the inestimable advantage of the process was impossible after so successful an experiment, and when the result transpired in England, doubt and hesitatation were rapidly removed, and the practice in no long time came to be generally adopted." (p. 49.)

The reader will admire the very natural complacency of our author, who concludes his review of the obstacles opposed to the spread of inoculation in many other countries with this remark:

"Enough perhaps has been said to show that this in common with every other improvement, wheresoever it may emanate, was eagerly received, and skilfully pursued by our own, in the proper sense of the term, truly enlightened country, whose mind has never been prevented by vulgar prejudice, or weakened by fearful superstition, or its determinations thwarted by aristocratic influence, or the power of antiquated corporations, or that of government itself, so perniciously felt in the old world." (p. 51.)

COWPOX. It is unnecessary to follow the professor in his references to the facts tending to establish the identity of variola and vaccinia, as the readers of this journal are familiar with the admirable and perfectly conclusive experiments of Mr. Ceeley on this subject.

In America it is thought convenient to employ the scab instead of the pellucid fluid, and to this cause Dr. Chapman thinks it not improbable that the alleged greater frequency of failure in the prophylactic powers of vaccination may be attributed.

The visitation of smallpox which commenced in Scotland in the winter of 1818, and prevailed nearly contemporaneously in England, especially in Norwich, and on the Continent, after progressively spreading through Europe, crossed the Atlantic, diffused itself over Mexico, South America, and the Antilles, and passing to the East Indies, pervaded nearly the whole world, proving one of the most extensive epidemics on record. In July 1823, four cases of strongly marked variola occurred at the same time, in widely separated parts of Philadelphia, in individuals who had not had any intercourse with one another. Such cases multiplied. The disease as it extended presented every variety from the mildest varicella to the most malignant smallpox. The former variety, however, was the most prevalent.

We quote the author's account of some peculiarities of this visitation.

"The protective powers of vaccination proved with us infinitely less than elsewhere. From data tolerably authenticated it is computed that between 4000 and 5000 failures of this process took place, and I have not been able to collect more than 30 instances of alleged secondary smallpox, and very few were the previous attacks in the natural way, or so violent by inoculation as to have left any marks behind.

"Curious is the fact that neither Dr. Physick nor myself, on this or any other occasion, ever met with an unequivocal instance of secondary smallpox. Many of the cases reported to be such I visited, and detected a source of deception which ought to be guarded against in the investigation of the subject. The disease chiefly prevails among the most stupid and ignorant classes of society, by whom the term inoculation is only employed, and hence they are apt to report themselves as having been variolated, when really the act was that of vaccination.

"The following table, taken from the report of Drs. Mitchell and Bell, who had charge of the smallpox hospital, is interesting in several views. It furnishes a statement of the results of 148 cases of the disease. There were 47 cases in persons who had been previously affected by vaccination, none of which died. Eight cases occurred in persons previously affected with smallpox, of whom 4 died and 4 recovered. Ninety-three cases where in persons who had not had either disease before, of which 52 died, and 41 recovered."

"Of the whole number, 69 were whites, and 79 persons of colour. Two out of the 8 persons who had suffered from smallpox a second time, took it the first time naturally or without inoculation. Eight of those vaccinated were so during

the prevalence of the epidemic, and some of the mildest cases were in the persons of those who had been vaccinated upwards of 20 years before.

"The disease, in conformity with most others dependent on a specific contagion, gradually declined on the accession of warm weather, and by the 1st of June entirely subsided. Next winter, however, it reverted, though sparingly, and thence ceased, with perhaps here and there a separate case, till the succeeding winter, when it again returned very much in the same manner as before. From 1825 to 1827, so little was seen of it that hopes were entertained of its disappearance, when it once more revisited our city to a considerable extent. During the next two years, it became nearly extinct, and so continued till the winter of 1830, on which occasion numerous cases occurred. Not much was heard of it after that period, though occasionally solitary instances were met with. But in 1833 it again revived, and spread widely; since which, with the exception of the year 1840, we have had scarcely any of it, and probably the epidemic has become exhausted. Each of its renewals has been marked by nearly the same phenomena, varied chiefly by gradations of violence, and in every instance preceded by varicella, scarlatina, rubeola, as well as by an infinity of other cutaneous affections. It is not to be supposed that we were exclusively the victims of this disease; nearly all our cities, and many portions of the country, have been exposed to its ravages during the same period, though probably not in the same degree." (pp. 90-2.)

Dr. Chapman advocates the opinion that a disposition to smallpox, although interrupted by vaccination, is apt to be reacquired at a distant interval, and thinks it not improbable that a recurrence to variolation may in some instances be found an expedient measure.

We are concerned that a physician of so much experience should lend the force of his reputation to disturb the confidence of the public in the efficacy of vaccination, notwithstanding the evidence which may be collected, even from his own incidental statements, that a vaccinated individual incurs less risk of death from smallpox than one who has already passed through an attack of that disease. We will mention a few facts, which cannot be gainsayed, in proof of our assertion.

In the Royal Military Asylum at Chelsea, between 1803 and 1833, 26 cases of secondary smallpox appeared, of which 3 died; whereas only 24 cases of variola occurred after vaccination, without a single death. In the Smallpox Hospital of London, in 789 cases of smallpox after vaccination 46 died; but of 9 cases of secondary smallpox, 2 died. But the observations of M. Bousquet, during an epidemic at Marseilles, establish the same truth from the magnitude of the scale more forcibly. In a population of 40,000, of whom 2000 had previously suffered from smallpox, 200 had a second attack; 4 died. Among 30,000 individuals who had been vaccinated, 2000 contracted smallpox, and 20 died; thus, 1 in 500 was the proportion of deaths in those who had passed once through that disease; but among the vaccinated, only 1 in 1500.

Indisputable evidence of the efficacy of vaccination is supplied by the records of the army, the precautions employed to secure the certainty of vaccination having almost excluded smallpox from the military reports. This single fact affords conclusive evidence that the supposed inefficacy of vaccination must be attributable to its omission, or imperfect performance. If time renewed the susceptibility, the majority of cases of smallpox after vaccination should occur in adults, but the reverse is remarkably can credit the statement of Dr. Percival of Dublin, that the disease was

the case. From the registrar-general's reports it appears that in the year 1838, in England, of 8706 who died of smallpox, 7575 were under the age of five. In Ireland, during the year 1841, of 58,006 cases, 49,038 were under the age of five. The simple explanation of the prevalence of smallpox is doubtless the neglect of vaccination.

In France, the attention of the legislature has increased the proportion of the vaccinated from one half to five eighths, and the annual proportion of deaths from smallpox has diminished in a remarkable degree.

MEASLES. A visitation of measles has been thought to occur every seven years. Professor Caldwell affirms that "beginning in 1772, and passing down to a period including fifty years, it prevailed epidemically in this city and vicinity every sixth year." With the exception of influenza, it pervades a country more rapidly than any other epidemic. In 1821, it overran in a few months nearly the whole of the United States.

The experiments of Home, confirmed by Wachsel, appeared to show that measles could be communicated by inoculation. The accuracy of these experiments was questioned by Cazenave. Dr. Chapman states that similar experiments with the blood, mucus, and tears were performed at the Philadelphia Dispensary without success; and he is disposed to attribute the occurrence of measles in the numerous cases in which Von Katona of Hungary inoculated with the fluid of the vesicles mixed with tears, to incidental exposure to infection. We cannot be satisfied with this explanation; the number of the cases treated by Katona exceeding 1000, the eruption occurring uniformly about ten days after inoculation, and the disease proving mild.

We agree with Dr. Chapman in assigning from ten to fourteen days as the incubative period of measles.

SCARLATINA. The chapter on scarlatina contains several statements worthy of quotation. Sir Gilbert Blane never saw an individual except one affected with scarlatina above the age of forty. Dr. Chapman attended a patient suffering from the disease at the age of eighty, but he never knew a very young infant take it, however much exposed. Reil witnessed an epidemic in which the disease was almost confined to patients between the ages of fifteen and twenty-five. Different visitations of scarlet fever afford a remarkable contrast in the degree of severity. During the epidemic at Paris in 1743, scarcely an individual who was attacked recovered; on the contrary, various visitations related by Morton and Sydenham were peculiarly mild.

"It is," writes Dr. Chapman, "one of those diseases that very conspicuously appears to have its cycles, in which the most opposite character is presented. The whole history of it warrants this conclusion. It has so prevailed from the earliest settlement of our country. Nearly half a century ago I recollect it as the terror of our community, and so it continued for some time; but, from about 1801 to 1830, it recurred at certain intervals, uniformly with such extraordinary benignity, that it rarely gave any trouble or anxiety in the management. I have heard the late Professor Physic declare that, during that lengthened period, he did not lose a patient in it. For the last ten years, however, it has proved very much the reverse, some seasons highly malignant and frightfully fatal as well immediately among us as elsewhere." (p. 138.)

The contagion of scarlatina adheres to fomites with much tenacity. We

introduced into that city by a box of toys from London, which had been exposed to the contagion.

"Dr. Elliotson mentions that for nearly two years all the children admitted into a particular ward under his care were seized with scarlatina, in consequence of a patient with the disease having been in the ward at that remote period, and this in despite of white-washings and other cleansings." (p. 134.)

At the same time, we attach greater importance to the power of free ventilation in dissipating the contagion, and place no reliance on the famous story of Hildebrand, who assures us that "as soon as he arrived in Padolia, scarlatina broke out and spread most widely, which he ascribes to the retention of contagion in a coat he had worn in the disease a year previously in Vienna." (p. 134.)

In this, as in many other parts of the volume, we have to regret that the writer should associate statements of every variety of probability, without attempting to define the principles which are to guide our credulity, and regulate the surrender or refusal of our assent.

Our author has evidently paid much attention to the duration of the incubative periods of contagion:

"That of scarlatina," he observes, "is said to be from five to six days. My own conviction is that it is usually greater, though I speak diffidently on the point, having been unable to satisfy myself in regard to it. Equally subjected apparently to infection, I have seen individuals break out with the disease from the third to the eleventh day; and we have had some cases lately reported, where, in a family of eight, the interval varied from the seventeenth to the twenty-sixth day, the average being seventeen days." (p. 135.)

Dr. Chapman's views on the treatment of scarlatina are for the most part judicious. After noticing the contrariety of opinion among books and practitioners regarding bleeding, a measure supported by Morton and Armstrong, but opposed by Sims, Withering, Clarke, and Willan, he observes:

"This contrariety of sentiment in relation to the remedy, I presume must be referred to its having been applied under opposite circumstances of the disease, and of course attended by very different results. Directed with discrimination, it cannot fail, according to my experience, to be beneficial, and often even indispensable. Convinced of this, my own practice, indeed, and which is that of our physicians generally, is to bleed in nearly every case of high and active excitement Yet it is true that the loss of blood has no direct curative tendency in the disease, it only abating action, without changing or subverting it, and as usually not well borne to any great extent; it is not safe to detract it with the same freedom as in the more purely phlegmasial affections, or perhaps to the amount that the existing indications in the case itself would seem to demand. Collapse, frightful and sometimes even fatal, I have repeatedly seen to result from an abuse of the practice, and it is always hazardous at an advanced stage of the disease. Local affections merely may be removed by leeches or cups." (pp. 142-3.)

Having observed some striking instances of "the deepest form of collapse" in Asiatic cholera, after resisting all stimulating and arousing means, readily "reacted" by frictions with cakes of ice to the whole cutaneous surface, and the freest consumption of the article internally, Dr. Chapman is inclined to think that similar applications might be useful in the passively congestive variety of scarlatina.

Whenever the disease is excited by an exposure to cold, it is prone to

present the malignant form, and is very difficult of cure. The author prefers burnt alum to nitrate of silver as a local application, both to sloughs and membranous exudation on the fauces. He questions, and most justly, the advantage of mercurials in the typhoid variety of the disease, and makes no mention of what many believe to be one of the most valuable remedies in this condition—we mean the chlorate of potash. We concur with him in a favorable opinion of digitalis in many cases of scarlatinal dropsy.

The professor is inclined to allow some weight to various powerful evidence in favour of belladonna as a prophylactic; from ten to fifteen drops being given night and morning of a solution of two grains of the extract in an ounce of water. *Post hoc ergo propter hoc.*

"Yet (he pithily adds) by the assurance of Hahnemann, the author of the homœopathic doctrines, which, originating in fraud and imposture, and continued by the most atrocious wickedness, that he was equally successful in the infinitesimal dose of a few drops, daily, of a solution which contained no more than the twenty millionth part of a grain of the extract, distrust and even ridicule are cast over the whole affair, and our faith must be withdrawn and suspended till fresh and better evidence is afforded." (p. 151.)

HEMORRHAGE. The work contains a somewhat tedious chapter on hemorrhage, in which the author takes great pains to establish an important truth, distinctly propounded by the best writers, but not yet sufficiently recognized by the public, namely, that "vital" hemorrhage is an effusion from the exhalents, not a result of rupture of vessels. He judiciously remarks:

"Too much importance is by many attached, in the management of vital hemorrhage, to its suppression. Great alarm is created by it in the individual himself, as well as in his friends, and from which the medical attendant is not always entirely exempt. Every exertion is therefore made to check it, and this being accomplished, the anxiety which previously existed heedlessly subsides. Lulled into false security, the patient is too often permitted to revert to his former habits, without any permanent plan of treatment, till again awakened to a sense of danger by a repetition of an attack, and in this way he proceeds till the complaint is often irremediably fixed. Now, the hemorrhage in itself is comparatively of little moment—for the most part, indeed, beneficial—and the real object of attention should be the correction of the condition giving rise to it, and which, by neglect, in numerous instances leads to the most disastrous consequences." (p. 171.)

In proof of the extent to which exhalation may proceed, we extract a case which Dr. Chapman attended with Dr. Dewees:

"A young man, otherwise in good health, who, for three succeeding days, lost about three pints of blood daily from his gums. Both these and his teeth were remarkably sound to all appearance. By wiping the gums clean, the blood was seen in a moment oozing out from numerous pores." (p. 164.)

Hemoptysis. As respects the liability of particular classes to hemoptysis, we are assured by the late Professor Rush, "that those religious denominations who do not sing, and mostly worship silently, are very subject to it, from weakness of lungs, owing to the want in this mode of adequate exercise of these organs;"

"But," says Dr. Chapman, "my own experience, however, does not confirm this observation. Living in the 'city of friends,' I have seen no peculiar liability in this description of people to be thus affected. Clergymen, on the contrary, are

exceedingly subject to hemoptysis, which has been ascribed, though I think erroneously, to the performance of the services of the pulpit. That it cannot be so, to the extent averred, is shown by the comparative immunity of lawyers, and legislators, and lecturers, each of whom harangue more constantly and loudly than preachers. I have, in treating of phthisis, advanced a conjectural explanation of this point. Be it as it may, the fact appears to me unquestionable, that this hemorrhage is far more frequently to be witnessed in connexion with an undue exertion of the lungs than the reverse, or the comparatively quiescent state, to which an allusion has been made." (p. 175.)

Dr. Chapman has observed hemoptysis to occur most frequently during sleep, a circumstance which he attributes to the tendency of a horizontal posture, with bending of the lower extremities, to determine blood to the lungs. Heberden, in a practice of sixty years, never lost a patient from hemoptysis. "This symptom should for the most part carry little further terror than that excited by a suspicion, too often well founded, that it is an outward sign or expression of disease of the lungs, and especially a tubercular state of those organs." In this opinion we concur, believing, with Louis, that, with few exceptions, such as cases arising from plethora, either general or local, *considerable* expectoration of blood is an evidence of the presence of tubercles.

The suggestion to which Dr. Chapman attaches most importance in the treatment is the use of ipecacuanha emetics. "Thirty years' experience of the success of the treatment has inspired great confidence in it." We regret that these thirty years have not conducted him to a more lucid explanation of the efficacy of the practice. "They operate by breaking up the habits and associations which continue the predisposition, and are also well calculated to emulge loaded vessels, and to distribute the blood equally throughout the circulation." (p. 196.)

Epistaxis. In the article on epistaxis the learned professor exhibits the aptitude of his memory for rare and wonderful events. Bleeding from the nose has been produced by loud sounds, "as a clap of thunder or the explosion of cannon."

"Blanchard says, he had seen it occur by the ringing of bells. I had a friend at Edinburgh who assured me that he was never exposed to the screechings of the Scotch bagpipes without fulness of the head, often leading to an effusion of blood; and I have somewhere read of an individual in whom the discords of music operated as a sternutatory, beginning with sneezing, and ending in the escape of blood. . . . . . It is sometimes, too, excited by acrid fumes, and may be by pungent or the blandest odours. I once knew an individual who could bring it on by smelling cheese for only a few minutes, and have heard of another in whom rotten apples had a similar effect. Bruyerin, indeed, gives an instance where the soundest apple induced it, and Rhodius tells us that it has followed the smelling of a rose. Moreover, it is occasioned by mental emotions, rage or terror, or a very excited imagination, or intense study, or anxiety with insomnolency." (p. 198.)

Opiates, blisters to the chest, and the internal use of iced water, the author considers valuable auxiliaries in the treatment. Acetate of lead is more effectual in doses of a grain or two at short intervals, than when given in larger quantities.

Epistaxis occasionally assumes an epidemic character. Such an event is related by Morgagni as leading to great mortality in Tuscany and other

parts of Italy, and a similar occurrence was observed in Pennsylvania during the year 1823. The quantity of blood lost by epistaxis is sometimes enormous:

"Bartholin mentions a case of forty-eight pounds within a period not given. Rhodius another of eighteen pounds within thirty-six hours; and a respectable writer in the Leipsic Acta Erudita, a third example of not less than seventy-five pounds within ten days. The Ephemera of Natural Curiosities contains a case where the quantity is not stated, from the difficulty of taking an account of it, which continued without cessation for six weeks." (p. 201.)

Dr. Chapman seems to experience peculiar pleasure in recording instances of great losses of blood; he mentions having attended patients affected with intestinal hemorrhage, one of whom lost eight gallons in a fortnight (but recovered); another eight quarts in three days, who died; a third, a pint daily for a fortnight, who ultimately did well; a fourth, three gallons in twenty-four hours. He mentions some other fatal cases under his own care, in which the quantity could not be ascertained, and he refers to the remarkable instance related by Michelotti in the 'Transactions of the Royal Society,' of a young man with enlargement of the spleen, who threw up in two hours more than twelve pounds of blood, and finally recovered. In the chapter on Hemorrhoids similar marvels are related; for example, on the authority of Lieutaud, six quarts in two days; on that of Panaoli, a pint daily for two years; and of Hoffman, twenty pounds in less than twenty-four hours.

Hematemesis. Emetics, a favorite remedy with our author, appear, from the narratives which he has given, to have proved very salutary in cases of hematemesis. With this exception he prefers turpentine to every other remedy, administering it after suitable aperients, and in doses of from 20 drops to a drachm, in such complaints.

We do not dispute the occasional usefulness of emetics, which indeed are strongly recommended by Dr. Osborne and other judicious practitioners, but the prepossession of Dr. Chapman in their favour is obviously so strong as to impair the authority of his testimony. One of the cases adduced by him in proof of their value being that of a lady, of whom he never heard after recommending her to "commence a course of emetics." He adds, "It were easy to adduce further cases of a similar kind, though perhaps not so striking."

Hemorrhoids. Hemorrhoidal affections are said to be more common in hot, damp, and miasmatic, than in cold and dry countries, and to prevail more in the South than in the North of the United States. They consist, according to our author, most commonly of protruded mucous membrane, as asserted by Colles and Kirby, not often, as alleged by Brodie, of varicose veins. He doubts the accuracy of the popular prejudice against aloes. So do we. As local applications, Dr. Chapman recommends ointments containing lead, laudanum, or mercury, narrow-leaved dock, stramonium, or mullen, the last of which remedies he considers lenitive. When the removal of hemorrhoidal tumours is determined on, he strongly deprecates the use of the ligature, excepting in varicose cases. Referring to Tilanus and others for bad results of this measure, he says, "It is wonderful indeed that such catastrophes have not oftener happened, for no demon of wickedness ever devised and practised greater torture than is here some-

times inflicted." Dr. Physick was accustomed to excise external hemorrhoids, and introduced wire ligatures for those which were internal. Dr. Harris, an American surgeon, "of equal judgment and very wide experience, invariably practices excision." We suspect the practise of Dr. Physick to be the best, and attribute the occasional bad effects of the ligature to its not being applied with sufficient firmness to destroy the vitality of the constricted part.

In a note appended to the chapter on Purpura we find references to some interesting cases of exudation of blood in the manner of perspiration through the pores of the skin; but none of these are original.

DROPSY. We are inclined to acknowledge the influence of climate in producing a disposition to dropsy, a disease which prevails in chilly and damp regions, but rarely appears in countries such as Egypt, Syria, and Nubia, which are steadily warm and dry. We are not, however, fully satisfied with the opinion apparently countenanced by Dr. Chapman, that a diet of pork or fish conduces to the production of this disease. The following passage contains a curious and characteristic collection of observations materially differing in the degree of probability.

"Many years ago I was called in consultation with the late Professor Physick, to a gentleman from Virginia, with general dropsy, who traced its commencement to his having imprudently plunged into a cold bath, while heated and sweating from severe exercise. We were told by him that the effusion took place a very few hours after coming out of the bath, and that previously his health had been perfect. Nearly about the same time Dr. Physick and myself attended a gentleman from South Carolina, with the same disease, by whom we were informed that, in robust health, returning from a tiresome and dusty ride, he went into a bath, the water of which was so hot that he could scarcely bear it. Continuing in it, however, for only a short time, on leaving it he found his skin very florid, even scalded, and soon experienced the distension of ascites, to which succeeded anasarca, and next hydrothorax, so that when he came under our care he had general dropsy.

"Cases I have seen to follow almost immediately flatulent colic. During the winter of 1812, I attended with the late Professor Wistar, a lad, who having become heated and fatigued by skating, laid [lay] on the ice, and after a short period was seized with colic, attended by a distension of the abdomen amounting to tympanitis. By carminatives, opiates, and external warmth, he was very quickly relieved from pain. But on our next visit a few hours afterwards, we were astonished to find that he laboured under ascites and œdema of the lower extremities.

"In the summer of 1825, a lady from the country consulted me, who after eating water melon and some other fruits, was attacked, as she said, with colic, quickly converted into tympanitis, and ultimately into dropsy of the abdomen, and of the legs and feet." (p. 265.)

"Draughts of cold water have immediately excited the disease, of which probably the most remarkable instance is afforded by Dr. Haen, who informs us that a large portion of the army of the Emperor Charles V., when proceeding against Tunis, fell into dropsy almost instantly after drinking freely of cold water while heated and exhausted, on a march in hot sultry weather."

"Bateman reports a case still more extraordinary, where the disease was at once induced by fright; and Ludolff de Meza insists on its having occasionally followed violent paroxysms of rage." (p. 266.)

Dr. Chapman has rarely seen a child recover when attacked with peritoneal dropsy, and in this observation he is supported by the testimony of

Dr. Physick. In the treatment of dropsy he speaks enthusiastically of the efficacy of compound jalap powder, "in the dose of a scruple or half a drachm of crem. tart. to 5, 10, or 15 grs. of jalap, with a few drops of ol. carui to prevent griping; so repeated as to keep up almost unremitting discharges from the bowels."

"Exhibited in this manner the results in some cases are promptly effectual and almost astonishing. I have seen in a few days the utmost intumescence and distension entirely removed by this remedy alone. It is therefore with the strongest emphasis, and in the highest tone of confidence that I press it on attention. Never, I can truly declare, have I had more reason to be delighted with any course of practice, in any disease, than occasionally with purging by the combination to which I have alluded." (p. 294.)

His favorite diuretics are an ounce or two of nitrate of potash daily in three pints of sugared water. Two-drachm doses of sweet spirits of nitre, or the following combination: ℞ Tinct. thebaicæ, gtt. x; sp. æth. nitr. ʒij; vini antim. gtt. 40; aq. font. ℥ij.

He suggests the annexed formula as a preeminently valuable hydragogue cathartic: ℞ Cambog. gr. iv; elater. gr. ss; sp. nitr. dulc. ℥j; aq. font. ℥iv. M. A tablespoonful to be taken every two hours.

On the failure of medicines to remove the disease, before resorting to the operation, Dr. Chapman says:

"It will be right to try the effect of a large blister to the abdomen, which I have known in two instances to prove effectual. The first of these occurred in a maiden lady somewhat advanced in life, attended by the late Professor Wistar and myself. Every measure used having failed, and the distension being excessively oppressive, we determined on tapping, the next day. But of her own accord an immense blister was applied in the evening, and at our next visit we were astonished to find the fluid evacuated, it having leaked through the skin of the abdomen to such an extent as completely to have soaked her bed. Exactly such a case is reported by Professor Caldwell, of the Louisville College, and with which I became acquainted from another and equally authentic source." (p. 306.)

Ovarian dropsy. In the dissertation on hydrops ovarii, our author expresses his dissent from the doctrine that the disease is owing to hydatids irritating the adjacent surface into effusions, but he considers blows to be a frequent cause. "Lee, no common authority, especially declares that it scarcely admits of doubt, from the progressive enlargement observed of the graafian vesicles, that the cysts supposed to be hydatids often originate in the morbid distension of the former bodies." (p. 311.)

The professor has a great admiration of heroic operations, and falls into the youthful mistake of confounding boldness with skill. He is favorable to ovariotomy, but his own facts are against his doctrine.

Hydrothorax. We are happy to find, in a work written by a physician of the transatlantic school, a caution against injudicious bleeding, and we therefore quote with satisfaction the following paragraph:

"Dropsy of the chest not unfrequently follows pertussis, asthma, and protracted catarrh, or bronchitis, especially in weak lymphatic persons. It is also and more frequently consequent on disorganization of the heart and the great vessels, or disorders of the chylopoietic viscera, including the stomach, sometimes misplaced or irregular gout or rheumatism, or repelled or imperfectly cured eruptions; above all scarlatina, as well as a state of anemia, however induced, particularly by excessive losses of blood. The fact is too important not to be mentioned, that

it very often happens, where the pleurisy of aged or otherwise feeble people is thus treated, though that disease may be cured, an effusion into one pleura becomes entailed. During my long attendance in the Almshouse Infirmary, such an event so commonly occurred among the inveterate drunkards of that establishment, that I became exceedingly discouraged from the use of venesection to any extent in that affection under these circumstances." (p. 319.)

The Professor mentions, with approbation, a prescription for Hydrothorax, which Dr. Ferriar, after a series of clinical experiments, adopted as the best:—℞ E. elater. gr. j; tinct. scillæ, oxym. scillæ, āā ℥ss; sp. æth. nitr. ℥ij; syr. rhamn. ℥j. The dose, one drachm every three or four hours.

Our readers must not be allowed to lose the narrative of the *walking-off a dropsy*, which will not fail to remind them of the famous story of the Hypochondriac cured by an enforced ride from London to Inverness, to see a certain hypothetical doctor Macdonald.

"The late Professor Rush was in the habit of relating the fact of a poor man, who, despairing of being relieved at home of an inveterate dropsy, determined to seek his advice, and for this purpose travelled on foot several hundred miles. Encumbered by the disease, and exceedingly weakened, he at first could walk only a very short distance. But, as he proceeded, the effusion diminished, and his strength returned, so that he was enabled to complete the arduous undertaking, and presented himself, on his arrival, perfectly recovered. From this anecdote may be deduced the efficacy of exercise in one instance of the disease at least—the value of diffusive professional reputation, and the extent to which it was enjoyed by the great physician from whom it was derived." (p. 375.)

We regret that Dr. G. O. Rees had not an opportunity of examining the blood-corpuscles of this patient before and after his expedition.

Hydrocephalus. We find nothing of especial novelty in the article Hydrocephalus. It contains some good practical remarks on the question of depletion in this disease, a subject of great importance, but fully discussed by Dr. Risdon Bennett, in his excellent treatise on that formidable disease.

Gout. The following observations on the prevalence of gout in Philadelphia, and its dependence on modes of living, are most valuable. We believe analogous testimony, though perhaps not to the same extent, can be furnished by many of our older British physicians. We are happy to record the doctor's report of the improved habits of his countrymen.

"Of the dependence of gout on the habits of living, no stronger proof can probably be supplied than from the annals of this city. When I commenced my professional career, the disease abounded in the higher circles; and then it was the practice to drink punch in the forenoon, to continue it at dinner, or to resort to ardent or malt liquors, followed by a liberal use of diverse wines, closing the evening with substantial suppers, and stimulating potations. But, in this respect, within the last thirty years, a signal change has taken place. No punch or distilled spirits, and comparatively little malt liquor has been consumed; and the custom of supping is nearly extinct. Temperance has superseded debauchery or excess, and gout, thus deprived of its aliment, is fast perishing away. My opportunities have enabled me to ascertain the fact, that so late as the commencement of the present century, a hundred cases of the disease existed in this community where one is now to be met with, and with few exceptions, these are the remnants of other days serving as memorials of a state of society of which there are scarcely any other traces to be recognized." (p. 385.)

It is remarkable what trivial circumstances will produce gout in pre-

disposed subjects. A cider-manufacturer, with whom Dr. Chapman was acquainted, was obliged to desist from his occupation, finding the mere odour of the liquid sufficient to excite a paroxysm.

The theoretical objections of Sydenham to the use of purgatives in the treatment of gout, are strongly opposed by our author, who places his chief reliance on this class of remedies; but he has sometimes found emetics useful, especially in the case of a gentleman who was constantly attacked with the complaint whenever he visited a badly-drained estate on the banks of the Susquehana.

Rheumatism. Dr. Chapman, in reference to the practice introduced by Brocklesby, of administering large doses of nitrate of potash in the treatment of rheumatism, remarks that an ounce of that remedy, daily, is more than most stomachs will bear. This observation is not easily reconciled with his own recommendation of still larger doses in the treatment of dropsy. After noticing atrophy of the muscles as an occasional result of rheumatism, he adds:

"Marasmus of the muscles takes place occasionally without any appreciable rheumatism, or if such attacks as I allude to are of this nature, they must be of the kind vulgarly called dumb rheumatism, devoid of expression by symptoms. The disease mostly comes on with no premonition, sometimes when the individual seems to be in good health, and the first indication of it is the obvious wasting of one or more of the large muscles, usually those of the neck or back, or hips, with corresponding imperfection in the motions dependent on those muscles. Gradually, other muscles become involved to a greater or less extent, and emaciation proceeds in them till it is extreme, and all sorts of distortions and deformities are exhibited. For a long period, the general system seems to sustain little or no detriment from this morbid process, and the digestive functions are, to all appearance, actively performed, but, ultimately, febrile irritation arising, the result is rapidly hastened. Five cases of this extraordinary affection I have seen, all brought to me from the country, the whole of which ended disastrously. Three of these were brothers, and the fourth, a nephew of them, is now under the care of Drs. Jackson, Mitchell, and myself; and I have heard that another member of the same connexion has been similarly affected." (p. 438.)

We are anxious not to overlook any remedies to which a practical physician of so much experience attaches importance. He considers savin, when continued perseveringly till warmth, itching, or eruption, arises, especially useful in lessening rigidity of joints from extravasation, and in arresting absorption in marasmus of the muscles.

In rheumatic affections of the chest, as well as in spasmodic asthma, he has observed much benefit to accrue from administering the juice of the fresh berry of the phytolacca decandria, or an ounce of the tincture three times a day.

Dr. Chapman is not disposed to trouble himself with experiments regarding the virtues of iodide of potassium, for reasons which we will quote:

"Well-tried remedies, like well-tried friends, I always adhere to, and am very slow and reluctant to abandon either without sufficient grounds. To do it in the former instance shows a weak and credulous head, and, in the latter, a capricious and depraved heart." (p. 445.)

But we must abandon our "old friend" Dr. Chapman; for, notwithstanding the merits which we acknowledge him to possess, we are heartily

wearied with the confusion of his facts, the intricacy of his thoughts, and the obscurity of his language. The work, as our readers must have perceived, is not deficient in materials which a skilful hand might have erected into a stately monument to the honour of science; but the actual author has suffered these materials to remain a disorderly heap—unshapen and uncemented. We have rarely encountered a more striking evidence of the necessity, to the medical practitioner, of that early mental discipline, without which, the greatest industry, the longest experience, and the most accurate memory, are likely to prove almost useless in the service of science; and we finish our laborious task with increased anxiety, that such improvements in preliminary medical education may speedily be introduced, as shall render infrequent the faults which it becomes our duty so often, and, on the present occasion, so particularly to deplore.

Art. IV.

Transactions of the Medical and Physical Society of Bombay for the Years 1842 and 1843. pp. 196; pp. 250.—*Bombay,* 1842-3.

In the present days of bookmaking, when the press is teeming with works so numerous that it would require a person's undivided time to peruse them, periodical literature assumes an importance which under other circumstances could not be claimed for it. It becomes the channel through which many, whose avocations necessarily occupy a large portion of their time, become acquainted with what is going on in the literary and scientific world, and who, by the notice taken of the emanations from the press, are led either to read them as being valuable contributions to the information on the subjects of which they treat, or to pass them over as undeserving of the time required for a careful perusal. On this account 'Reviews' in the present day deservedly occupy a very prominent position. But there is another branch of periodical literature which also is of much value, comprising those 'Journals' set apart for original communications, and the 'Transactions' of learned societies. By means of these, observations, cases, and papers, which although valuable in themselves, and often leading to important results, would not be of sufficient consequence to warrant the publication of a monograph, are brought under the notice of the profession. This class of periodicals is particularly available to officers in the public service, who enjoy many excellent opportunities of collecting facts and making important observations, but who from the numerous demands upon their time, especially in unhealthy colonies, are unable to work up their materials into a volume, and even did circumstances permit this, are unwilling to risk the expense of publication, especially when at a distance, and unable to superintend it themselves.

The volumes now before us we consider to be of high value, as affording a channel through which medical officers in India can make known to the profession the results of their observations in that wide and rich field for the cultivation of physical and medical science, which has hitherto been much neglected or at least but partially improved. Considering the disadvantages under which these officers labour, from the numerous demands

upon their time, the amount of mental exertion required in the discharge of their duties, and the enervating effects of the climate, we cannot but look upon these Transactions as highly creditable to all concerned in their production.

The Medical and Physical Society of Bombay was established in November 1835, its principal object being to collect and disseminate information on the medical and physical sciences. In furtherance of these views, it was proposed to publish, half-yearly if practicable, a volume containing, "1st, original communications, whether received from members, or from government, or from the medical board, which shall be recommended for publication by the committee of papers; 2d, a report for the past half-year, drawn up by the secretary of the society from such documents as may have been placed at the disposal of the society by government or the medical board,—this report to be read at a meeting of the society before publication; and 3d, an appendix consisting of selections of interest from medical works and periodicals." The society seems to have received every assistance from the authorities; the local government having assented to the transmission of all letters on the business of the society, under the signature or to the address of its secretary, free of postage; and having given permission to have its circulars printed at the government press. The medical board and deputy-inspector-general of hospitals also became patrons of the society, and placed at its disposal the returns and reports in their offices, while the same facilities were afforded them by the medical authorities at Madras. Under such auspices the society could scarcely fail of success. Some modifications have been found necessary in its arrangements, but on the whole it seems to be in a flourishing condition. An annual volume has been published instead of a half-yearly, and the proposed report by the secretary does not appear to have been drawn up. We do not doubt that the society has been productive of much good by stimulating the industry of the medical officers in the Presidency. It is a matter of regret that a similar volume of transactions has never been got up in this country by the army medical department, which might easily be done were similar encouragements held out by the government and medical board as in India.

We do not propose to enter into a detailed consideration of the papers contained in the volumes before us, but shall briefly notice one or two of the most important. Before doing so we may remark, that in the fifth volume is given a recapitulation of the titles of the papers in the four preceding, a reference to which will show those interested in the subject the nature of the contributions which this society has been the means of eliciting.

The first paper we shall notice is a "Medico-historical abstract of the first year's service in the East Indies of H. M. 14th regiment of Light Dragoons at Kirkee, under the Bombay Presidency, by J. W. Moffat, esq. surgeon, H. M. 14th Light Dragoons." Kirkee is the only station in this Presidency provided with permanent accommodation for a regiment of European cavalry. It is about 60 miles up the country E.S.E. from Bombay, at an elevation of 1820 feet above the level of the sea, and is celebrated as the spot where, in 1817, Mr. Elphinstone's subsidiary force scarcely numbering 3000 Sepoys, under Colonel Burr, repulsed a sudden attack

made upon them by upwards of 20,000 Mahrattas. The barracks were erected in 1827, are well constructed, and calculated to afford excellent accommodation for a regiment 700 strong. There is a good hospital, built in 1830, capable of holding 120 patients, and allowing 1100 cubic feet of space to each.* Besides a good description of the station, this paper contains many judicious remarks on the causes of diseases to which soldiers are more particularly exposed on their first arrival in India; the best means of avoiding or diminishing these, and the plan of treatment which our author found most successful. He has drawn up some excellent tables, showing the strength and deaths of the different ranks, and the influence of age and length of service in India upon the mortality. We do not intend to enter upon these points as the data are insufficient, and the period of observation too short, to warrant positive deductions being drawn, but if similar abstracts are furnished by the medical officers at Kirkee for a series of years, they will afford the means of elucidating several still controverted points. We trust that by thus calling attention to them, officers at other stations may be induced to follow Mr. Moffat's example.

In treating of the interior economy of the regiment our author says:

"Tables and forms are provided also for the convenience of messing, and a scale of diet is laid down, and furnished by government at a reduced rate to the soldier. The daily ration drawn for each man, not in hospital nor in imprisonment, is—bread, 1 lb., second sort; beef, 1 lb.; rice, 4 oz.; salt, 2 oz.; coffee or tea, $\frac{4}{7}$ oz.; sugar, 1½ oz.; firewood, 3 lbs.; with one dram of arrack at the option of the soldier, to be consumed at three regular meals, breakfast, dinner, and supper. The men are messed in squads of four, and native cooks are entertained at a small weekly cost, who are further engaged to provide milk for the tea or coffee, butter, eggs, steaks, chops, additional vegetables, cheese, &c., as they may be occasionally asked for by varying tastes and appetites. The station being in such proximity to a populous city, (Pooua, 2½ miles distant,) all sorts of supplies are procurable in abundance, and little could be altered in the ration for the advantage of the men beyond the occasional variety of mutton. The supply of vegetables is probably wisely left to the choice of the messes; and for the rice, which is not much relished, over and above what is used for the soup, unless when curried meat is the order of the day, the cooks are not averse to substitute carrots, onions, country greens, and occasionally pumpkins, yams, &c. A dram of spirits still continues to be offered to the men at dinner time, and an inducement to take it is held out in its issue at a lower rate by one fourth than its ordinary retail price. The issue of a ration dram is a relic of former times, when salt meat and stale provisions were the ordinary and only rations provided; but surely it may be well dispensed with when mixed provisions of an excellent and wholesome quality are supplied, and its continuance leads to a wrong impression of its utility and necessity."

The spirit ration has been long abolished in our other possessions, and we can see no good reason for its being retained in India. It is true the soldier is not now as formerly compelled to swallow it or to throw it away, lest he should sell it to some thirsty comrade, but as he is offered it at a lower rate than he could purchase it at, he looks upon it as part of the reward for his services, and rather than give up any of his rights he

* The author of the paper states 1100 cubic *inches*, but this is evidently a mistake. He has fallen into the same error regarding the space in barracks, which he states at 1400 cubic *inches*. By a calculation made from the dimensions of the rooms, as given in this paper, the cubic space to each man appears to be about 1000 feet.

accepts it. By this practice not only is the intemperate confirmed and encouraged in his vicious habits, but the young soldier is induced to commence a system of dram-drinking, acquires a fondness for it, and is soon converted into a drunkard, unfit to be trusted, a disgrace to the service, a burden to himself, and a nuisance to every one connected with him. We have frequently heard officers inveigh against the debauchery and drunkenness of the soldier in India, but is not much of the blame attributable to those who thus tempt him onward in his career of intemperance? To Sir Henry Hardinge the army is deeply indebted for the abolition of the spirit ration in 1830, and most sincerely do we trust that he will take advantage of his present exalted position to extend this boon to the troops in India, and to remove finally and completely this source of disease, insubordination, and crime.

A ration of spirits was originally issued from a mistaken idea that it conduced to the preservation of health, but we fear it is not issued now from any idea of benefit to the soldier, but for the sake of the large profits accruing from the canteens, and the duty on arrack, to the Hon. Company of Merchants trading to the East Indies. If our conjecture is right we may well say, in the words of Lord Liverpool, "This is too bad."

The above description offers a very enviable picture of the condition of the cavalry soldier in India, at least when not on field service. To illustrate the great improvement that has taken place in this respect, through the force of public opinion, and the gradual progress of civilization, we subjoin a statement, by one who had practical experience of it, of the mode of messing troops in Ceylon in 1803, and we have reason to believe the description was applicable, at that time, to India. Of the truth of this account we entertain no doubt, although, contrasting it with the preceding, it seems scarcely credible, because we know that the same system was in operation fifteen years afterwards: it has only been during the last twenty-five years that these improvements have been effected.

"When the meat was brought to the cooking-place, it was thrown down upon a dirty mat, and chopped up, then the cooks sat down upon their hams, placing a knife between their toes, and cut it up into small pieces; and thus daubed all about, and without even being washed, it was boiled in curry, the rice being boiled at the same time in another earthen vessel, called a chattie, as all their boiling is done in earthen vessels, and then brought into the barracks at twelve o'clock, when the soldiers gathered round them like as many voracious hounds, with their chatties in their hands, bawling blasphemy, and grumbling, not so much at the quality as the quantity of their food.

"At night we got what was called supper, which consisted of a small cake, made of rice flour and water, and a liquid called coffee, although there was not a single grain of the berry in it. What we used for sugar was called jaggery (a coarse kind of sugar, made in Ceylon, from the cocoa-nut,) which was made up in cakes, very insipid and dirty; it bore no resemblance to the sugar used in Europe. At eight o'clock in the morning we got breakfast brought to us in the same manner; it consisted of the same cake, fish, or bullock's liver, and jaggery-water, and this formed the daily diet of the British troops in Ceylon. Had it been of good quality, and properly cooked, it was well enough: our allowance of liquor, which was arrack, was one quart per day, to five men, or about two drams to each.

"The beef, of which we had a pound per day, was given out along with the rice in the morning, for which sixpence per day was kept off our pay. But it

was rather carrion than beef. The cattle, sometimes buffaloes, sometimes bullocks, having been used in their husbandry, were generally lean, old, or diseased; the meat was soft, flabby, and full of membraneous skin; it had a rank, heavy, loathsome odour, was offensive to both sight and smell, and hurt me more than the climate, and was, I am certain, the cause of much of the disease and death which thinned our numbers. The rice, too, was small and of a bad quality, full of dust and dirt, from which our rascally cooks were at no trouble to free it."*

Shades of the 19th regiment, think of butter, eggs, steaks, chops, cheese, carrots, onions, pumpkins, and yams!!!

The principal mortality in the 14th Light Dragoons, during its first year in India, arose from epidemic cholera, by which 22 were cut off. But for this unfortunate circumstance the deaths would have been very little above the proportion incident to a regiment at home, amounting only to 14 from all causes, out of a strength of 650. The diseases most prevalent were fevers, chiefly of the continued type, and the treatment appears to have been very successful, only 1 case in 76 of this class having proved fatal. The most important feature in them was a marked predisposition to the symptoms of *delirium tremens*, which perhaps in no small degree arose from the habitual use of ardent spirits, sanctioned and encouraged by authority, as already noticed.

The diseases next in frequency, were diarrhœa and dysentery, which, however, were not of a very fatal character, owing probably to the sound constitution of the men being as yet unimpaired by the climate; we fear that subsequent years will show a much less favorable result. We would strongly recommend a perusal of this paper to young medical officers going to India, both as containing much information and many useful hints, and also as an example worthy of being followed. In concluding our notice of it, we deeply regret to add that its author has since fallen a victim to the Indian climate.

Dr. A. S. Thomson has entered at considerable length into the very interesting and important question, "Could the natives of a temperate climate colonize and increase in a tropical country, and vice versa." To elucidate this he has collected from various sources as much information as he could upon the results of the successive attempts at colonization in India, by the Portuguese, Dutch, English, French, and Danes, and by the Dutch and Spaniards in the Indian Archipelago. He also reviews the state of the European colonies in tropical Africa and tropical America, and has brought together a large amount of statistical evidence. From these data, which would occupy too much space to lay before our readers, he concludes that "there is little doubt the tropical parts of the world are not suited by nature for the settlement of natives of the temperate zone. European life in these parts is with difficulty prolonged, much sickness is suffered, and their offspring become degenerate and cease to propagate their species in a few generations; and should necessity force Europeans to perform the drudgery of labouring in the field, their lives will be rendered still shorter, and their existence will be little better than a prolonged sickness."

The evidence on the practicability of natives of a tropical country colonizing a temperate climate is more limited, but would appear to

* The Life of Alex. Alexander, p. 109.

justify our arriving at the same conclusion. In addition to the fact of the great mortality in the 4th West India regiment, when removed to Gibraltar in 1819, we find that in Philadelphia the deaths among the blacks amount to nearly 5 per cent., or 1 in every 21 living, and in New York to 1 in every 22 living, a rate of mortality which must eventually terminate in the extinction of the black population, unless it is kept up by immigration. From a careful consideration of the evidence he has collected, Dr. Thomson infers,

"That man, both from his mental and physical structure, is able to resist sudden vicissitudes of climate better and for a greater length of time than any of the inferior animals, yet he is only born to flourish in climates analogous to that under which his race exists, and that any great change is injurious to the increase and to the mental and physical development of man. The nearer the climate of the original resembles the adopted country, so much the less will be the injurious effect ..... There is a theory in medicine denominated the doctrine of *acclimatization* or *seasoning*, the principles of which are, that the human constitution, after a certain length of time, becomes suited to any great change of food, climate, or indeed any unusual mode of life. On this theory is founded the opinion that the first few years of an European's residence in the tropics are more fatal than any other period, an opinion opposed to many statistical facts and at variance with the statements adduced in this paper; for it must be obvious if the doctrine of *seasoning* were correct, the European ought to be completely fitted in the course of sixty years to resist every injurious effect of the tropics, and that their offspring should almost resemble the aboriginal races in being able to bear the heat, a result perfectly opposed to almost every fact which has been quoted.

With the exception of a few barren islands, the whole surface of the globe at present known is peopled by the human race. This diffusion of man from the poles to the equator must have been extremely gradual, an opinion which an examination of the sacred and profane history of the world proves..... No great vicissitude of climate or sudden change of food or mode of living was therefore experienced, and the human race increased and multiplied on the earth."

In the fifth volume Dr. Thomson has given a "history of the epidemic fever which prevailed among the men of H. M. 17th regiment during the monsoon of 1841, when quartered in the Colabah barracks, Bombay," which we notice because it affords a striking example of the benefit derived from sending the sick to sea. 166 sick, all of whom had suffered from fever, were embarked on the 27th of October, and cruised northward to the Persian gulf, and arrived at Bombay on the 24th of November. "The sick, embarked emaciated and without appetite, soon began to eat and get strong; almost every man had a paroxysm or two of fever during the cruise, but mild in its nature compared to what they had at Colabah." No case proved fatal at sea, and very few of them were severe.

We are surprised that an officer who understands the principles of statistics, as Dr. Thomson has proved he does, should have omitted entirely to state the strength of the regiment during the prevalence of this epidemic, as it must be obvious that a mere statement of the number of cases admitted into hospital conveys very indefinite information when the amount of the force among whom they occurred is not given at the same time.

In the fifth number of the Transactions is an article, entitled, "Reports on the medical statistics of Upper Scinde, drawn up by the medical

officers serving with the force under the command of Brigadier England. Presented by the medical board, November 1842." These reports were called forth by the following

"MEMORANDUM FOR MEDICAL OFFICERS. The medical officers of this force are to be called on by the assistant adjutant-general to report, for the information of the supreme government of India, on all points regarding the medical statistics of their respective stations. 1. The climate generally as regards the troops. 2. The average number of soldiers per cent. under medical treatment, during the worst months and during the best. 3. The seasons, showing the best and worst for the troops, and whether the past season has been in any way a peculiar one. 4. The number of deaths during the last four months, out of men. 5. The sort of accommodation which the troops have had, and specifying how far sickness has been influenced thereby. 6. The particular months during which each station is most unhealthy, and the causes. 7. The unwholesomeness, generally of particular places. 8. The medical officers are to be directed to make these documents as complete as possible, and to offer such suggestions, as to clothing, &c., with a view to remedy the evils discovered, as may be most likely to lead to the attainment of that desirable object. By order, (signed) W. WYLLIE, Major A.A.A. General, Scinde Force. Camp, Quetta, 6th September, 1841."

We believe it is not customary to criticise the literary composition of orders issued by the military authorities, especially when on service; but we may remark that the gallant major might, without much trouble, have written (if, indeed, he wrote it at all,) a more grammatical and more intelligible memorandum. But we have still more important faults to find: it was not issued till the beginning of September, and we have no reason to suppose the medical officers had any previous intimation that they would be called upon to afford information on these points. During the period respecting which they were to report, therefore, many opportunities of investigating these subjects may have been allowed to pass unheeded, and when at length the Memorandum was issued, the officers may have been obliged, in the absence of recorded observations, to trust to their memory and the impressions left by events long past. Surely such a document should have been issued at the commencement of the expedition.

We are not informed whether this memorandum was issued by the advice of the medical board, or whether any medical officer was consulted as to the information most likely to be useful, or the form in which it ought to be called for; but it is very evident that whoever drew it up must have been extremely ignorant of the principles of statistical investigations. For instance, the officers are ordered to report "the average number of soldiers per cent. under treatment," but this information is of very little value unless we know the amount of the force; one or two unsound men in a very small body of troops might by frequent readmissions into hospital increase the ratio per cent. to such an extent as to lead to the inference (and it might be a very erroneous one) that the post was unheathy, while such a circumstance would scarcely affect the results if the numbers were considerable. Again, they were to report "the number of deaths during the last four months out of men," (!!!) a statement which would not convey the slightest information of practical utility as the strength was not given. A simple monthly numerical return of the strength, the admissions into hospital, and deaths, with the diseases by which these were occasioned, would have been much more useful than the reports furnished,

as from it the authorities could have obtained all the information called for in Nos. 2, 3, 4, 6, and 7, while a few remarks appended to it on the climate, barrack and hospital accommodation, diet, and clothing, would have rendered it tolerably complete, with the additional advantage of the results being in a form which would admit of their being compared with each other. It may be remarked, that of the nine reports in this volume, one only, that of surgeon Patch, 21st N. I. and superintending surgeon S. P. contains the information in the form we have above suggested.

We may be supposed to have dwelt too much on the imperfections of these reports, but we have done so not from useless regret for a lost opportunity of collecting valuable information on the influence of a climate hitherto unknown, but in the hope that similar opportunities may not be thrown away through mis-directed exertions. Scarcely a year now passes that some expedition is not fitted out in India to be sent into parts of that extensive empire hitherto unexplored or but little known, and it is much to be regretted that due advantage is not taken to procure, in a systematic form, the observations of the medical officers accompanying the troops,—a class of men who from their education and general acquirements are well qualified to carry out scientific investigations. We have been informed that it is intended to adopt in the Indian army, official medical returns similar to those which have been so long in use in the British army; this will go far to remedy existing defects, by introducing a uniform system throughout the service, and will render the information obtained from every different quarter available for the purposes of comparison. But we trust the East India Company will not rest satisfied with merely organizing a set of returns, but will make the necessary arrangements for having them properly condensed and published annually, with a selection from the observations in the accompanying reports. If they are to be consigned to the dusty shelves of a government office for want of duly qualified persons being appointed to reduce them to a practical shape, the medical officers will soon discover this, and make them out in any way that causes least trouble; indeed, under such circumstances they might just as usefully send in a quire or two of blank paper. Among the objects sought for by the establishment of statistical returns in the army may be enumerated two: 1st, that of affording to the military authorities information regarding the efficiency of the troops at the various stations, the circumstances affecting their efficiency, and the best means of promoting and increasing it; and 2d, of furnishing to the medical officers the results of the experience of their professional brethren as to the salubrity of different situations, the causes of disease, and the hygienic and therapeutic measures best adapted for the maintenance of the soldiers in an efficient condition. To effect the last of these objects a volume should be published annually, giving in a condensed form the information contained in the returns and reports, a copy of which should be transmitted to each medical officer in the army. There can be no doubt that this would prove a powerful stimulus to officers to exert themselves to collect information of a useful and interesting nature, as it would be the means of bringing their names favorably under the notice of the profession.

It is much to be regretted that no arrangements have ever been made to render available the vast mass of valuable reports which have been accu-

mulating for the last thirty years at the medical board in this country. A portion of the numerical returns has been condensed and published in the "Blue Books," on the Health of the Army, but the medical histories of diseases, &c. still lie there, nor, so far as we can learn, is there any prospect of their being made any use of. We trust such will not be the case in India, but that coeval with the adoption of a system of returns and reports will be the establishment of a department to render them available to the military authorities and to the medical profession.

In the 6th No. is a series of "Cases illustrative of the pathology of the diseases of Bombay, by Dr. C. Morehead," the secretary of the society, which appear to have been drawn up with much care. The paper was commenced in the 2d No. of the Transactions, in which 45 cases were detailed; in the present number he has added 68 to these; but as he states that he proposes in a future volume to make the practical remarks on pathology and treatment which a consideration of them, in connexion with cases of similar diseases successfully treated, is calculated to suggest, we shall reserve our observations till the series is completed.

Having thus noticed a few of the more important papers in these Transactions, we may in conclusion express our hearty wishes for the society's prosperity. It has in the volumes before us given practical proof of its utility as a medium of eliciting and communicating valuable information, and we trust that as years roll on, it will continue to collect and disseminate still more valuable fruits than it has hitherto done. We had almost forgotten to add, that while the matter is good, the style of getting up these volumes is very creditable to all concerned.

Art. V.

Die Krankheiten des Gehirns und Rückenmarks bei Kindern. Durch Krankheitsfälle aus dem ersten Kinderspitale erläutert, von Dr. L. W. Mauthner.—*Wien,* 1844.

A Treatise on the Diseases of the Brain and Spinal Cord in Children, illustrated by Cases occurring in the Wards of the First Children's Hospital. By Dr. W. L. Mauthner.—*Vienna,* 1844. 8vo, pp. 456. With five lithographed plates.

The profession is much indebted to Rokitansky and other members of the modern Vienna School for their zealous cultivation of pathological anatomy. Practical medicine, however, is a department in which they have of late laboured but little, or to little purpose, if we except the physical diagnosis of diseases of the chest, which Professor Skoda and his followers have done so much to elucidate. This being the case, there is some danger of our over estimating the value of a work in which the author's aim has been entirely practical: just as the rarity of a gift always seems to enhance its worth.

The merits of Dr. Mauthner's work, however, are such as would secure for it attention whenever it might have appeared. It contains the results of seven years' careful observation in the wards and among the out-patients of the children's hospital which he established in the year 1837, and

which has since flourished under the patronage of the empress. The hospital contains 36 beds, and between 2000 and 3000 children are admitted as out-patients, some of whom are visited at their own homes. Of 15,836 children who have thus come under his notice, 1747, or about 11 per cent., were suffering from some disorder of the cerebral system, of whom about 10½ per cent. died. The frequency and the importance of diseases of the brain and spinal cord in childhood have induced Dr. Mauthner to make them the subject of this volume; and he promises at some future time to investigate in a similar way the other diseases of early life.

"Diagrams," says the author, "are in geometry just what the histories of cases are in medicine. The former illustrate the proposition, and serve to reduce it to absolute certainty; the latter illustrate the laws which regulate nature even when suffering, and lead to as complete a measure of clear perception as is to be attained in an experimental science."

The whole book is a kind of practical commentary on this motto; for it contains the minute details of 123 cases, connected by general observations on the diseases of which they are instances. Such an arrangement unquestionably presents many advantages, but inseparable from it are several inconveniences which greatly detract from its usefulness. No clear description is given of any affection, but its peculiarities have to be learnt by the somewhat tedious process of reading several cases. The particulars of one case suggest remarks on one striking character of the disease, and, eight or ten pages farther on, another case suggests the addition of a new feature to the portrait. It thus happens that no adequate conception is formed of the subject as a whole, from its not being represented, except in parts. Another defect, traceable to the same cause, is, that the author's remarks are too often merely suggestive, and his descriptions so brief, that, though their truthfulness would at once be recognized by those who are already familiar with their subjects, they are frequently little more than allusions which would be wholly inadequate to teach the inexperienced. But, though these circumstances somewhat limit the utility of this work, it will yet fully repay an attentive perusal, since it contains very much practical information, gathered, not from the writings of others, but from personal observation at the bed-side of the sick.

The book commences with some remarks on the frequency of diseases of the cerebral system, on their diagnosis and treatment. The author then passes to the consideration of the special diseases of the brain, which he has arranged under the following heads: "1, Congestion, irritation of the brain (*febris cephalica*); 2, Apoplexy; 3, Inflammation of the brain (*encephalitis* and *meningitis*); 4, Acute hydrocephalus; 5, Hypertrophy of the brain; 6, Atrophy of the brain; 7, Chronic hydrocephalus; 8, Tubercle of the brain; 9, Softening of the brain; 10, Cerebral convulsions." (p. 12.)

CEREBRAL CONGESTION. This, though not in strict propriety an independent disease, is yet so frequent an attendant on the different maladies of childhood, as to merit a separate examination of its causes and symptoms. Its frequency is so great, that Dr. Mauthner observed it in 186 out of 229 children who had died of various diseases. In a large proportion of these cases it occurred in all probability in the very article of death,

which tends to empty the arterial and gorge the venous vessels throughout the whole system. In the course of various diseases, however, and under various circumstances, the brain may become overloaded with blood, as the result of an exaggerated or an enfeebled activity in the cerebral vessels and the vascular system in general. On this difference of cause the author founds a distinction of cerebral congestion into the active and the passive, either of which varieties may be acute or chronic. He further attempts a distinction between that form of cerebral congestion in which the surface of the brain is the seat of the affection, and that in which the central parts of the organ are chiefly involved. He confesses, however, that this distinction is no longer practicable when a state of intense congestion of any part of the brain is present; and we have much doubt whether the differences he indicates at p. 14 do not depend on the degree of the affection and the suddenness of the seizure more than on the seat of the congestion.

Febris cephalica. Intermediate, between the acute form of active congestion and the chronic form, Dr. Mauthner places what he calls the *febris cephalica,* which is attended by fever, with pain in the head, disorder, or, more generally, constipation of the bowels, and a train of symptoms, such as are often supposed to be premonitory of an attack of acute hydrocephalus. Dr. Mauthner is of opinion that it sometimes occurs as an independent affection, when it tends to a critical termination by sweat and urine on the third or fifth day. We cannot quite subscribe to the correctness of this last observation, for we do not think that there is any form of cerebral congestion so distinctly separated from others by its symptoms and by its tendency to critical termination at a certain period as to need a distinct place in our nosologies.

It is only in the most active forms of cerebral congestion, and in children above six months old, that the author attempts general depletion, and he adds (at p. 17) a very useful caution against inferring anything as to the propriety of venesection from the excitable pulse, and the changeable hue and temperature of the surface in young children. In milder cases less active treatment may suffice, as the employment of cold applications to the head, the administration of purgatives, of small doses of calomel, and of antiphlogistic remedies, which it is of great importance to administer in a form as nearly as possible tasteless.

The writer defines "passive congestion" as "that form of morbid accumulation of blood in the brain which takes place without violent symptoms of reaction and irritation." (p. 27.) In new-born children who breathe but imperfectly, and whose blood circulates languidly through the brain, this condition is physiological. If, however, there should be much delay in the establishment of respiration, a condition ensues of sopor, heaviness of the head, and want of power over the extremities; the heart beats irregularly, and convulsive twitchings occur. The state of passive congestion kept up by this imperfect respiration gives rise, in the course of time, to serous effusion, and this effusion may become very considerable without inducing reaction in the vascular or nervous systems. By slow degrees the cerebral substance becomes softened, partly by imbibition of the effused serum, partly by the serum being forced out into the parenchyma of the brain, and death ensues from direct paralysis of the nervous

centre. This is the condition described by Billard as non-inflammatory softening of the brain.

In older children passive congestion of the brain is a frequent result of various diseases, both acute and chronic, which give rise to much exhaustion of the nervous power. It is likewise by no means unusual in children who have been exposed to unfavorable hygienic conditions, or who are ill fed. It is also readily induced by the over energetic treatment of active congestion, and the abstraction of too large a quantity of blood. The symptoms which attend it are very different from those of active congestion:

"They commonly consist in a pale and cool, or hot, though moist skin, eyes sunken in the head, and half closed, drowsiness without sleep, weariness, heaviness of the head, stupor, diminished sensibility, impaired digestion; and hence vomiting and purging of food, for the most part indigested, small and frequent pulse, and swelling of the veins of the head. To these must be added sopor and convulsions in the severer degrees of the affection." (p. 31.)

The treatment of this state consists in the use of warm mustard baths, and in the employment of mild diaphoretics and aperients. Alteratives, and even emetics, which latter are quite contraindicated in active congestion, are sometimes of great service, nor is the use of narcotics absolutely excluded. After mentioning a few other points that deserve attention in the management of this condition, and relating several illustrative cases, the author passes (at p. 40) to the next subject of his treatise.

APOPLEXY. He notices the rarity of circumscribed effusions of blood into the brain in infancy, although the symptoms of apoplexy are by no means uncommon. A state of general plethora even is not always essential to the production of apoplectic symptoms, but they may occur from general weakness and consequent diminution of the propulsive power which should impel the blood through the brain. Interrupted circulation, indeed, however produced, may produce apoplexy in early life—a fact, of which the peculiarities in the structure of the infantile brain and skull afford a satisfactory explanation.

"The impulse of the heart is powerfully felt in the cerebral vessels, while neither the external counter-pressure, nor the tension of the surrounding parenchyma is such as to assist its reflux. In proportion to the rapidity with which the atmospheric pressure propels the blood inwards, will be its accumulation in the long, fine, and numerous cerebral vessels, which have no firmer support than the soft tissue of the brain. So soon therefore as by any acceleration of the blood's motion the carotids bring a relatively larger amount of blood to the brain than the jugulars can carry off, danger of apoplexy arises." (p. 41.)

Cephalhæmatoma. Those very peculiarities, however, which tend to favour the accumulation of blood in the brain, either by increased afflux, or by diminished or interrupted efflux, are associated with another which serves the part of a safety-valve, and provides an outlet for its excess. This consists in the very free communications of the cerebral vessels with those of the cranium and the scalp. Hence, in early childhood, effusions of blood beneath the scalp are frequently met with, constituting the *apoplexia tegumentaris* of Billard, and cephalhæmatoma is regarded by the author, in common with Rilliet and Barthez, as the result of causes that tend to produce sudden and violent stasis of the blood in the brain, and as another instance of the utility of this safety-valve arrangement.

In older children, in whom these communications have, with the progress of ossification, become less numerous, death from apoplexy without any extravasation of blood is not very unusual. Such an instance is case 15, in which a healthy boy, aged 5 years, after overloading his stomach with indigestible food, was suddenly seized with convulsions, became livid and comatose, and died in thirty-eight hours. The brain was gorged with blood, but no vessel was lacerated, and no exudation of blood had taken place. The converse of this holds good also, and hemorrhage may occur without giving rise to any symptom of apoplexy, of which case 20 is a striking example.

Passive apoplexy. Some of the cases most difficult of explanation, are those of what Dr. Mauthner terms passive apoplexy, in which apoplectic symptoms occur in children who are neither plethoric nor possessed of vigour of constitution. He adopts much the same explanation of their occurrence as has been suggested by Hachmann with regard to the same condition which he has described under the name of *apoplexia venosa*.

"It is a well known fact," says the author, "established by Magendie's ingenious experiments, that a very slight deviation from the normal properties of the blood suffices to impede its free circulation through the minuter capillaries. No alteration of the blood seems to arrest its passage through the finer vessels to so great a degree as that which depends on a deficiency of oxygen, and a predominance of alkaline constituents. While the circulation is thus retarded imbibition of the blood by the coats of the vessels goes on, the fluid becomes infiltrated into the neighbouring tissues, whence stasis of the circulation, œdema, and even inflammatory action may result. These results of deficient oxygenation of the blood take place most rapidly, when the mucous membrane of the intestinal canal has become diseased, (under the influence of conditions unfavorable to health, especially if with these there be associated a warm, damp, and foggy state of the atmosphere,) and excretes a large quantity of matter abounding in oxygen, the waste of which, owing to the progressive weakening of the respiratory function, becomes every minute more and more imperfectly supplied." (pp. 62-3.)

Inflammation of the Brain. Dr. Mauthner endeavours to distinguish between inflammation of the brain or its membranes, and acute hydrocephalus; since he thinks, and with justice, that the collection of serum in the ventricles is neither the only indication of inflammation of the brain, nor indeed an invariable attendant on inflammatory processes. He states that he met with fluid in the ventricles in 172 out of 229 post-mortem examinations of children who had died of various affections, and that in 123 instances the fluid present was in considerable quantities.

"Encephalitis," says he, "is an independent, inflammatory process, either completely idiopathic, or occurring as a secondary result of some of the dyscrasiæ; hydrocephalus acutus, on the other hand, is the local manifestation in the brain of various morbid processes: as typhus, scrofula, impetigo, softening of the brain, scurvy, etc. A stage of inflammatory reaction may occur as one among the group of symptoms that attend this affection, but this does not, as in the case of encephalitis, constitute the disease itself, since hydrocephalus may occur, as what is called the *waterstroke*, without any previous stage of inflammation. Acute hydrocephalus, therefore, is always a secondary affection, while encephalitis often occurs as a primary disease, although it may likewise be excited by the existence of some other disease in the system. Acute hydrocephalus never occurs as the consequence of external influences alone." (pp. 76-7.)

There is great similarity between the two affections, so great indeed

as to make the attempt to distinguish between them appear at first sight a refinement of little use. There is, it must be allowed, no single symptom that can be mentioned as pathognomonic of either, but the different order in which the symptoms occur constitute, as the writer has well pointed out, the grounds of diagnosis. The stupor and unconsciousness which often exist at the commencement of encephalitis, usually occur towards the end of hydrocephalus. The former runs a rapid course, the latter advances slowly, passing through several stages, often presenting distinct intermissions. Rapid and causeless emaciation mark the commencement of hydrocephalus, and death is often unattended by convulsions; emaciation occurs later in inflammation of the brain, and convulsions are never absent. Another point of difference is found in the age of the subjects who are attacked by the two diseases; hydrocephalus seldom occurring before the end of the first year, and being most frequent from the age of 2 to 7, while inflammation of the brain may come on at any period.

Hydrocephalus. In these remarks Dr. Mauthner has, we think, seized very accurately the distinguishing marks which characterize two forms of disease frequent in early life. The one, acute in its course, tending rapidly to a fatal result if left to itself, but amenable to medical treatment; the other, a secondary affection, slow in its advance, often insidious in its progress, but uninfluenced by remedies. The former of these affections is manifestly inflammatory in its nature, of the essence of the other we know but little, but are wont to characterize the assemblage of symptoms which betoken its existence by the name of hydrocephalus acutus. What then is this formidable disease, and wherein does it consist? Dr. Mauthner's definition we confess does not satisfy us, we almost think it does not satisfy himself.

"The name of acute hydrocephalus," says Dr. Mauthner, "is to be applied to every morbid collection of serum in the ventricles, which supervenes in the course of other general diseases, is occasioned (*bedingt*) by them, and runs an acute course." (p. 107.)

We cannot but regard this definition as an advance in the wrong direction, and as tending to involve in hopeless confusion a subject already sufficiently obscure. We know that the symptoms of what is called acute hydrocephalus by no means invariably depend on the effusion of serum into the lateral ventricles, while most extensive collections of fluid are found after death where during life no symptoms have betrayed their existence. Thus in three fourths of all phthisical patients whom M. Louis examined after death, he found extensive effusion into the ventricles. Dr. Mauthner, himself, allows (at p. 291), that the symptoms of tubercular meningitis are, in the greater number of instances, only a repetition of those which constitute acute hydrocephalus, yet in this form of disease, serous effusion is not invariable in its occurrence, and when present is certainly by no means the most important of the post-mortem appearances. Had the author submitted his numerous observations to a rigid analysis, he would, we are sure, have given us something much clearer and more definite as the results of his labours.

Hypertrophy of the brain. We are glad to pass from the vagueness which characterizes this part of his subject to another form of disease to which the author has devoted much attention, and has treated of rather

fully. It has long been a well-known fact that the brain may, like other organs of the body, exceed its natural size, and that this hypertrophy of its substance is generally associated with some derangement of its function. Few observers, however, have devoted much time or trouble to the study of this morbid condition, or of the symptoms to which it gives rise, a circumstance which imparts a double interest to the results of M. Mauthner's careful investigations.

M. Mauthner has been at the pains of weighing the brain of 216 children at all ages from birth up to the 8th year, during the whole of which period an increase in its weight is pretty constantly going on.

"During this time," says he, "we find a minimum of oz. 10. ʒvj rise to a maximum of oz. 44½. The average weight begius with 13½, and rises to 35½ ounces. During the first year it grows from 13½ to 20½, or 7 ounces; in the second, from 20½ to 25½, or 5 ounces; in the third, from 25½ to 32, or 6½ ounces; and between the fourth and eighth year, from 32 to 35½, or 3½ ounces. Hence it appears that the brain grows most rapidly in the first year of life, that in the second and third years its increase is still considerable, but that its growth is slower after the fourth year. In conclusion it may be observed, as a remarkable fact, that the minimum weight usually occurs in cases of atrophy or phthisis, the maximum in pneumonia, scarlet fever, apoplexy, and cerebral tubercle." (p. 162.)

It further appears, from a minute examination of the condition of the brain in these cases, that its weight is to a great degree dependent on the quantity of blood which it contains.

The general anatomical characteristics of hypertrophy of the brain need no special description, we may notice, however, the author's mention of its frequent coincidence with enlargements of the thymus gland, of the left ventricle of the heart, and of the liver; facts which lend some support to Münchmeyer's theory of the connexion of asthma thymicum with hypertrophy of the brain *. Besides the more usual form of cerebral hypertrophy, in which there is actual increase of the size of the brain, there is another variety in which the increase is only relative. In such cases the brain is not too large but the skull, probably as the result of excessive activity of the process of ossification is too small, and the brain, in consequence of the pressure that it undergoes, acquires a degree of firmness amounting to induration. In such cases the child is always deficient in intellectual power, and is frequently idiotic and unable to walk. The head retains its natural size, but the sutures close unusually early, and the parietal and occipital protuberances are unusually prominent. Such children present none of those indications of rachitis which so often coincide with hypertrophy of the brain, but the lower animal life thrives at the expense of the higher; the skin is firm, and the body fat and ruddy, the muscles and bones strong, the constitution robust, and appetite craving.

A very interesting case is related, at p. 184, in which the symptoms of hypertrophy of the brain depended neither on enlargement of that organ, nor on preternatural smallness of the skull, but on thickening of its diploe as the consequence of rickets.

"The bones of the skull were extremely vascular, the diploe was spongy, an inch thick anteriorly, somewhat thinner posteriorly, but its thickness was everywhere greater than natural. Both the anterior and posterior fontanelles were

* See his essay on the subject in the Zeitschrift für die gesammte Medicin, 1842. Nov. 3.

open; at the posterior inferior angle of the right parietal bone were two spots, at which the walls of the bone were exceedingly thin; the vitreous table presented remarkably deep impressions of the cerebral convolutions, as well as channels for the vessels, particularly on the right side. The dura mater was firmly adherent, the pia mater injected, the convolutions of the brain very distinct, the brain firm, small, all its parts indurated, and weighing 24 ounces." (p. 185.)

It would not be possible during life to form a clear diagnosis of such a case; we mention it rather as a pathological curiosity.

In detailing the symptoms that ordinarily attend hypertrophy of the brain, M. Mauthner distinguishes the passive from the active form of the affection. In passive hypertrophy the cranium early presents a striking deviation from its natural appearance, in the enlargement and globular prominence of the occiput. The parietal protuberances subsequently project, the coronal and sagital sutures continue open in the ninth, or even in the twelfth month, and the fontanelles remain unclosed for a much longer time than natural; the growth of hair is scanty, and the veins of the scalp are much injected. Children in this state sleep much, though they are easily startled, they sweat much about the head, and when in a sitting posture the head drops forward by its own weight. Attacks of crowing inspiration occur when the child cries, and not unfrequently end in, or are accompanied by regular convulsions, and the severity and frequency of these seizures are greatest during the period of dentition. Digestion is at the same time impaired, and vomiting and diarrhea are frequent. By degrees the symptoms of pressure on the brain become more evident, or they are suddenly developed as the result of the supervention of some other disease.

"When hypertrophy of the brain has reached this stage, the skull deviates still more from its natural shape, the forehead sometimes becomes prominent and globose like the occiput, and while the skull goes on acquiring an increased curvature, the region of the temples continues flat and thus contributes to give to the head the appearance of being formed by the union of segments of four spheres. During this stage of the affection the preternatural softening and thinning of the cranial bones, corresponding to the prominences of the convolutions are distinctly perceptible, especially at the occiput. The functions of the brain become now much disturbed, headache, giddiness, impairment of muscular power, and loss of memory occur, the child grows sullen, peevish, sleepless, whimpers continually, and rolls the head constantly from side to side. At the same time it seems choked with phlegm, while the skin becomes every day more flabby, the muscles shrink, the bones grow soft, and the muscular power rapidly diminishes. Hence these children lie usually on their back, breathing with habitual wheezing, and suffering from constant dyspnœa, with occasional asthmatic seizures, such as have been already described. When in this condition slight causes suffice to produce a general excitement of the vascular system, and to excite diseased action in other parts, which render still more obvious the influence of the hypertrophy on the nervous system generally. If the child happen to catch a slight cold, attacks of convulsive cough, or of asthma occur in consequence, or convulsions come on, which terminate life in a few days." (p. 174.)

Such is the course usually run by this affection, but its symptoms differ, when, as is sometimes the case, the hypertrophy is partial, or when the disease assumes the active form, or that in which the walls of the skull, owing to the energy of the process of ossification, do not expand in proportion to the rapid growth of the brain. Its symptoms then are usually

those of active cerebral disease, the result of compression of the brain, and its consequent congestion.

In the chapter on chronic hydrocephalus, the diagnosis between that disease and hypertrophy of the brain is stated at great length. The chief differences insisted on by M. Mauthner will perhaps be best seen by throwing them into the following table.

Hypertrophy of the Brain.	*Chronic Hydrocephalus.*
1. The posterior part of the skull first presents an unnatural prominence.	1. The forehead is the first part to present unnatural prominence; the altered direction of the eyes and the very great width of the sutures and fontanelles are likewise characteristic.
2. Children lie horizontally, or throw the head back.	2. Children lie on the belly, with the head lower than the rest of the body, burying the face in the pillow.
3. Face puffy, eyes inexpressive and staring, mouth half-open.	3. Countenance withered, having expression of premature old age.
4. Functional disturbance comes on very gradually—not before the period of dentition or weaning—and consists at first in affection of the respiratory apparatus, difficulty of breathing, and attacks of apnœa.	4. Functional disturbance occurs early and involves the cerebrum from the very beginning.
5. Patient fat and leucophlegmatic.	5. Patient ill-nourished, subject to rickets and tabes mesenteria.

The treatment of hypertrophy of the brain necessarily differs according to the circumstances under which it occurs. In that form which is connected with rickets, absorbents and rhubarb with preparations of iron, and a properly regulated diet continued for months, are often very useful. Cold sponging of the surface is frequently of service, but in consequence of the tendency to perspiration about the head care should be taken not to leave it quite bare, but it should be constantly covered with a light cap. In the active form of the disease, whatever might tend to excite the brain must be avoided, while the long-continued use of the iodide of potassium has been found beneficial. Warm pediluvia and the occasional application of small blisters to the back have likewise been of service.

Atrophy of the Brain. In the next chapter the author treats of two conditions which are the direct antitheses of hypertrophy of the brain, namely, atrophy of the brain and the hydrocephaloid disease. The size of the brain has been stated by different writers to be uninfluenced by general emaciation of the body, but though this be to a certain extent true, yet the brain itself loses size and weight in cases of softening or long-continued disease of the stomach and bowels. Dr. Sims has noticed that in old age, in the advanced stages of phthisis, in disease of the stomach, and other affections in which great emaciation occurs, the brain likewise undergoes a partial or general loss of substance. The same wasting of the brain takes place very perceptibly during the rapid atrophy of which children are sometimes the subjects, when the natural turgor of the fontanelles and sutures is sometimes so far diminished that the scalp falls into wrinkles, and the edges of the ununited bones may be made to overlap each other. Children in this condition sometimes live in a state of exhaustion a considerable time; they notice nothing, and neglect their playthings, while the automatic movements of their limbs betray an intelligence no higher than that of brutes. Their face grows old and withered, and every

higher expression of the features undergoes a gradual sometimes a total obliteration, while their craving for food and drink becomes insatiable. In older children a somewhat similar retrocession of the intellectual powers occasionally comes on in the course of mesenteric diseases, or of long continued diarrhea. In such cases the weight and size of the brain are found after death much below the average, and there is a considerable interval often occupied by transparent serum between the brain and the parietes of the skull. The pia mater is pale and bloodless, the cerebral substance is soft, anæmic, often infiltrated with serum, and the difference between the gray and white matter is undistinguishable. The convolutions of the organ, and the parts in its interior are but imperfectly developed, and the ventricles are often empty, though sometimes they contain serum, and the characters of atrophy of the brain, then pass gradually into those of chronic hydrocephalus.

The author notices a peculiar condition of the brain which he has sometimes observed in children who have died of marasmus. It consists of a partial induration of the organ, and though often associated with diminution in the size and weight of the brain is sometimes met with independent of any alteration of its volume. He regards it as the result of a state of congestion or inflammation, but the symptoms by which it is attended are very obscure, consisting in convulsions, sopor, and very rapid emaciation. The centrum ovale, and the walls of the lateral ventricles, especially at the anterior or posterior horn, are its most frequent seats, and it is sometimes remarkably evident when it affects the tænia semicircularis. The indurated portion usually has an elongated form, is distinguishable by the gray colour of the cerebral substance, but especially by its cartilaginous hardness. This form of induration of the brain, of which Dr. Mauthner relates three instances has to the best of our knowledge never been noticed by any other writer. It is, therefore, worthy of mention though at present little more than a pathological curiosity.

He next describes that form of atrophy, or rather of deficient development of the brain, which is connected with smallness of the skull, for the most part congenital, constituting microcephalus. Though in point of intellectual endowment such children are on a level with the Cretins, in whom likewise the brain and skull are often inadequately developed, yet there is a wide difference between them in point of physical organization. The body of the former is well developed, and they perform all the functions of animal life perfectly, it is only their brain which seems in fault, while in the latter, all the springs of life seem nearly equally affected by disease, and the ill-formed body, and its ill-executed functions are almost as deplorable as the low intellectual endowment of the child. Children with this congenital smallness of the brain often suffer from early infancy from various convulsive affections, or other indications of diseased innervation; and they either die under the first attack of serious disease, especially if that disease be one of the exanthematous fevers, or they are carried off by the development of tubercular disease.

SOFTENING OF THE BRAIN. In his observations on the hydrocephaloid disease, Dr. Mauthner adds nothing to the well known account of it by Dr. Marshall Hall. We likewise pass over the chapters on chronic hydrocephalus, and on tubercle of the brain, not as by any means valueless, but

as containing less that is new or striking than is to be found in some other parts of the work. These are succeeded by a chapter on softening of the brain, a condition which he, like MM. Rilliet and Barthez, considers to be the secondary result of some other disease, not a distinct and independent malady. One anatomical peculiarity which distinguishes softening of the brain in the child from the same process in the adult is that the gray substance is seldom affected, the central white matter being almost always the exclusive seat of the alteration of texture. Dr. Mauthner recognizes the difficulty, indeed we may say the impossibility of determining in many cases where softening of the central parts of the brain has been found to coincide with the accumulation of fluid in the ventricles, which was the primary affection. He does not, however, seem disposed to regard this softening as in all cases of the nature of mere œdema, but thinks it is one of the consequences of a morbid state of the circulating fluid, which tends to produce disintegration of various organs of the body; since this softening is by no means confined to the brain, but affects other parts, particularly the mucous membrane of the intestines. The only symptom of actual destruction of the cerebral fibres, on which he seems disposed to rely is hemiplegia. He confesses even this to be by no means a certain indication.

Convulsions. In the present state of our knowledge of cerebral disease; after all known disorders of the brain have been classified and described as carefully as possssble, the large class of convulsions still remains, the causes of which are so various that there is no single form of cerebral disturbance to which they can be referred. Dr. Mauthner therefore devotes a whole chapter to the subject of convulsions, one too of the most valuable and practically useful in the book. He commences the subject with remarking that convulsions comparatively seldom occur in children as the primary results of disorder of the nervous system, but that they are usually secondary phenomena induced by sympathy with disease in some other part of the organism. This fact is now generally admitted, but Dr. Mauthner is of opinion that the influence of different states of the vascular system in inducing convulsive affections has hardly been sufficiently appreciated. The healthy performance of the functions of the nervous system depends not merely on the normal relations of the peripheral nerves to the brain and spinal cord, but also on the healthy state of its relations to the vascular system.

"Every disturbance of the harmonious reciprocity of action between vessels and nerves, and between the latter and their common centre, must therefore interrupt the equilibrium between the nutrient parts and the parts nourished, and excite a morbid activity of the nervous system, which displays itself very soon in children, through the medium of their irritable muscles. Disturbances of such a kind are extremely frequent at this period of life, owing to the circumstance that the blood has still to pass through many stages of development; that the brain, the centre and regulator of nervous energy, is in a state of constant and rapid growth, and lastly, that the whole vital powers of the child are silently engaged in the task of building up the unfinished organism. Convulsions are often in early life the indications of febrile action, and are a frequent symptom of some local disturbance in the system." (p. 360.)

We somewhat compress the remarks which he goes on to make, in which he endeavours to show that the development of febrile action, and the

phenomena which attend it, depending on a high degree of vigour in the vascular system; the absence of this power in the vascular system in childhood accounts for the absence of the symptoms which characterizes pyrexia. That very tendency to venous congestion, which is so strikingly marked in childhood, increases the excitability of the nervous system, and produces a liability to convulsive affections. Of the truth of this statement the numerous hypochondriacal and hysterical affections that occur in the adult are ample evidence, as are the cramps and spasmodic pains which attend deranged menstruation and hemorrhoidal affections.

Now it is very probable that there may be in the remarks just quoted rather more of the transcendentalism of the Vienna School than will suit all our readers. Truth, however, forms the basis of Dr. Mauthner's remarks, and perhaps we should not find it easy to express his meaning better. He next glances at the different organs whose disorders are often attended with convulsions, among which the brain and its diseases hold the foremost place. He notices the frequent occurrence of convulsions as the fatal termination of various affections draws nigh, when they indicate that death has begun to assail the centres of life. His next remark is one that we have not seen before in print, but we can confirm it from our own observation, except that we do not think the cessation of thoracic symptoms is quite so complete as he here represents it.

"I have," says he, "often observed in cases of extensive hepatization or tuberculization of the lungs, during the course of which the brain was perfectly unaffected, that the children a few days before death lost all chest symptoms, that the cough and orthopnea seemed to have entirely vanished, their appetite returned, and they seemed cheerful, when convulsions suddenly came on, followed in a few hours by death." (p 362.)

Equally correct is the following statement:

"Another tolerably frequent phenomeuon is this, that in those cases of cerebral disease which were marked at their commencement and during their course by violent convulsions, the act of dying is generally quite tranquil, owing to the paralysis of the brain having suspended its influence over the muscular system." (p. 363.)

Before he passes to the consideration of the treatment, he introduces one very important caution, which we must extract:

"Inasmuch as convulsions are a frequent attendant on disease of the brain, it is certainly very natural to turn one's attention first to the nervous centre. It often happens, however, if much care be not taken to investigate a case thoroughly, that leeches and cold applications to the head are hastily ordered, and calomel given; when the presence of pneumonia is afterwards detected, or some cause of gastric disturbance found to exist, without due attention to which no permanent amendment can result from any treatment. Inflammations of the chest are particularly apt to lead into this kind of error. Their real symptoms are masked by convulsive seizures, the medical attendant fancies on the first day that the case is one of inflammation of the brain, on the next day he thinks it must be pneumonia, and thus the uncertain diagnosis leads to vacillating treatment, and much mischief is the result." (p. 364.)

We do not enter into any minute account of the treatment suggested by Dr. Mauthner in the different forms of convulsive attacks. The judicious employment of known remedies, not the eager hunting after new medicines,

has long been the characteristic of the sound physician. The cases appended to this chapter afford the best possible illustration of the various forms which these affections assume, and the various treatments they require.

The last eighty pages contain an account of diseases of the spinal cord in children. They contain much that is deserving of notice, but we have already exceeded our limits, and must take leave of Dr. Mauthner, heartily recommending his book as the most valuable contribution to practical medicine which has appeared for many years from the Vienna school.

ART. VI.

Die angeborne Verrenkungen. Mit zwei Tafeln. Von LUDWIG JOSEPH MELICHER, Doctor der Medicin und Chirurgie, &c. &c.—*Wien,* 1845.

Congenital Luxations. With two Plates. By LUDWIG J. MELICHER.—*Vienna,* 1845. 8vo, pp. 220.

LUXATIONS of the different articulations of the body appearing at birth is a subject which, till within the last few years, has but little occupied the serious attention of the profession either at home or abroad, and the work before us is, we believe, the first monograph on the topic which has been offered to the public. The theme we need scarcely say is far from exhausted, and yet a great store of facts has been accumulated with which it behoves every well-informed medical man to be acquainted. With the execution of the treatise before us we confess we have been disappointed. It is divided into two sections: the former occupied with what the French call *generalities*; the latter with details; and yet from want of due elaboration,—not to say more—there is a lack of distinctness, and an amount of repetition, which is vexatious. The subject however being handled *ex expresso,* and didactically, proves decidedly suggestive; and though we may not closely follow the plan of the author, we shall endeavour to condense the information he communicates, and add such elucidations from other quarters as suggest themselves.

And let it not be supposed that this is a subject which is more calculated to excite curiosity than to instruct the practitioner. Far from it. Cases are neither of rare occurrence, nor of trifling moment.

"The frequency of these luxations," says M. Melicher, of one variety only, "is greater than is generally supposed. Since the year 1826 when Baron Dupuytren first directed attention to the subject, 180 cases, in various joints, have been recorded. In the course of 18 years this eminent surgeon had seen 26 cases, and M. Guerin had met with 30. Heine had witnessed 11 cases of congenital luxation of the femur, Chelius 9, and I have seen 6. Mr. Smith of Dublin records 5 cases in the shoulder-joint, Adams and Cruveilhier have seen it in the elbow-joint, whilst various authors have noticed luxations of other joints, as will be stated in the sequel."

And as to its importance,

"I have known," said Dupuytren, "many individuals affected with *original* luxation, by sheer mistake of diagnosis, confined to bed for years! I have known others subjected to remedies without number,—to blistering, leeching, cauteries, moxas, &c. &c. I remember one poor girl, who had moxas applied no fewer than twenty-one times, without the useless and barbarous treatment, of course, producing the slightest benefit."

Nurses, again, are in this way often subjected to great injustice, and are most unworthily accused, by heart-broken parents, of having carelessly, it may be barbarously, lamed their children for life; annoyances, these, which competent knowledge and intelligence of the medical attendant would at once have anticipated or removed. Although for ages the subject has been involved in obscurity, it has recently, by the rapid advance of science, been greatly illustrated; and though much yet remains to be done, an account may be supplied not devoid of interest, and which may enable the practitioner to encounter and dispose of the cases he meets, with comfort to himself, and satisfaction to his patient.

Observation and experience have confirmed, what a moment's reflection would suggest to every intelligent practitioner, namely, that luxations appearing at birth, may arise from various and different causes. 1. Considering the nature of the process of parturition, no one can be surprised that the several forces which are then applied to the infant coming into the world, should in some instances lead to dislocation of one or more joints. True, the injury to the limb may not always be of the nature of a luxation, but frequently it is so. These kinds of cases it is evident, though occurring at birth, are not entitled to rank properly as congenital luxations. Hence they should receive a preliminary consideration, because as bearing on diagnosis and practice, they must not be overlooked. They belong to the section of the acquired and violent luxations of our author, *Luxationes acquisitæ violentæ*. As produced during the process of parturition they may appropriately be designated as obstetric luxations, *L. obstetricæ*.

The remaining species as often, at all events, occurring during fœtal life, may well be classed under the category of congenital, *Luxationes connatæ vel congenitæ*. They are very different however in their origin and early history. 2. The joints in the fœtus are, more or less, liable to those complaints which occur in these parts in after life. Of these diseases three have been specified as frequently producing dislocation; namely, first, *morbus coxarius*, or coxalgia; secondly, *hydrathosis*, or an increased secretion and accumulation of the synovial fluid; and thirdly, a hypertrophy of the vascular and fatty cellular structure, which has been named the Haversian gland, which mechanically may dislodge the head of the bone from its socket. Diseases of this character may occur in several joints, though they certainly have been noticed more frequently in the hip joint than in the others. These together go to form that species of disorder which has been denominated spontaneous luxation, *L. spontanea, consecutiva*. 3. It would appear that frequently the immediate dynamic cause of the disorder is an irregular action of the muscular apparatus, excited by a derangement in the nervous system,—the true efficient cause,—a species this which may conveniently be designated functional luxation, *L. functionalis*. 4. And fourthly, one of the most aggravated, if not the most common species of this affection, is a complaint which has its origin, not so much from a morbid, as from a defective action. It appears to arise from what, in general terms, may be denominated arrested development of the joints, wherein the beautiful apparatus of these parts is more or less defective and incomplete; the consequences of which will at once be evident. This species constitutes the *L. originalis*, the *L. originelle*, and congenital malformation of Dupuytren. All these species, moreover, may

and do occur in very different degrees; and hence there are specimens of *complete* and *incomplete* luxation. Under the head then of *obstetric* luxation, *spontaneous, functional,* and *original,* we shall arrange such remarks as our space allows. The two former need not occupy us long.

1. OBSTETRICAL LUXATION. We commence with the obstetric variety of luxation as the simplest, and probably the most familiar of all. And here, in reference to those not unfrequent blunders to which we have adverted as described by Dupuytren, we may allude to a rule, applicable to all the species, noticed by our author as observed in the great Maternity Hospital at Vienna; according to which the child, when undergoing its first ablution, is minutely examined by the nurse who performs the duty, with the special view of ascertaining that nothing abnormal appears in any part of the frame. If anything of the kind is discovered, it is noted at the time, and all becoming attention is subsequently bestowed upon it. The *localities*, according to M. Melicher, in which luxation from violence may occur, are to be found in every free joint of the body; but they are principally observed at the shoulder, elbow, ribs, hip, and ankle.

With the view of throwing light on the liability of the different joints to luxation, and the causes and circumstances in which it is most apt to occur, the author made numerous experiments on the dead subject with the object of inducing artificial dislocation, and this where opportunity offered, where the child was in utero, as well as out of it.

Accordingly he experimented upon the bodies of children, respectively of the ages of seven, eight, and nine months, and found that the disposition to luxation in them depended upon the incomplete condition of the surfaces of the joints. At the earlier of these periods of fœtal life, the free (arthrodial) joints are barely indicated, and in the latter months there is nothing more than a superficial excavation. In the humerus, for example, there is a wide disproportion between the size of the head of the bone, and the cavity in which it lodges. Besides the condition of the bones and cartilages, the natural laxity of the other component parts of the joints, of the ligaments, tendons, and muscles, may all be regarded as predisposing causes of the accident.

The author instituted another series of experiments upon children who had arrived at the full time; in relation both to the humerus and the femur. He found luxation of the humerus could be most easily accomplished towards the anterior edge of the scapula; and to be more difficult in the directions inwards and forwards into the subscapular fossa, and outwards and backwards into the fossa infraspinata.

Again, as regards the hip-joint, he found that luxations upwards and outwards are the most difficult to superinduce. He succeeded only in two cases out of thirty, in which he attempted to produce this luxation,—the epiphyses in many cases giving way. In this joint the luxation downwards and backwards into the ischiatic notch is most readily effected, though even this is no easy task. M. Melicher never succeeded in producing dislocation downwards and inwards into the foramen ovale, or upwards and forwards upon the pubis. From these experiments, the author believes that, in violent luxations, and probably in others, the head of the femur usually leaves the acetabulum at its anterior and inner edge, and is placed between this spot and the ischium, and then slides backwards; so

that luxations during fœtal life, do not take place downwards and backwards into the ischiatic notch, but occur upon its outer edge, and thus the head is either by degrees, or suddenly, shifted from the external edge of the acetabulum by a second dislocation upwards and outwards, into the fossa iliaca externa, and there becomes fixed; an effect which is produced by the position of the extremities of the fœtus in utero; and from their movements, by the contraction of the muscles, especially of the glutei moving the head of the bone upon the external edge of the acetabulum.

But we shall not dwell longer on these details; and may here, in a few words, dismiss the obstetric variety of luxation, which, as we have said, does not accurately belong to the subject of congenital dislocations. The *causes* most likely to operate, enumerated by our author, are irregular and unwonted contraction of the uterus; irregular and unwonted position of the child, more especially of one or other of its extremities, faulty contractions of the maternal pelvis, from mollities ossium, &c. &c. Capuron believes that it may occur in cross births, from the traction used in the inguinal region, by the fingers. Upon six trials of this kind of delivery, effected upon women who had died undelivered, the child being also dead, M. Melicher found that luxation was produced twice, each in one side of the pelvis. This, however, was effected not by the finger but by the crotchet.

The *diagnosis* in this variety of luxation cannot with an average share of attention be very difficult. A degree of external injury must necessarily exhibit itself, if not at the moment, yet within a few hours of the child's birth, the soft parts being implicated not less than the articulation. In fact, however, though there may be present both the external marks of violence, and the symptoms of deeper-seated injury, it will still remain to be ascertained whether the mischief arises from dislocation, or from fracture. This latter accident, it would appear, from M. Melicher's experiments, is the more likely of the two, and will require to be treated upon the common principles for such cases. Respecting cases of luxation, artificially induced, M. Melicher observes, "once dislocated, the head of the bone remained out and was replaced only with the greatest difficulty." In eighteen such cases he only succeeded in effecting reduction three times; once at the shoulder, and twice at the hip. He contends, that the task will prove still more difficult in the living subject, but this seems more than doubtful; and we question indeed, whether such experiments elucidate the subject. Could the parts once be replaced, which, with sufficient care, should certainly be accomplished, the natural action of the muscles and elasticity of the surrounding parts, would greatly contribute to keep all right. The *prognosis* in this variety and the *treatment* must evidently be regulated by the common rules of surgery, and without doubt with a fair prospect of success.

II. SPONTANEOUS LUXATION. Nor need the species of *spontaneous* luxation occupy us long. Every day's experience is teaching that many of those diseases we are familiar with in the adult, are apt to show themselves in the fœtus; and the modifications they thus undergo though curious, are such as the first principles of the science would lead us to anticipate. Diseases of the joints form no exception to this remark, and

they, or rather their consequences, form the spontaneous luxations, *L. spontaneæ, consecutivæ*, of our author.

Much still requires to be done in this part of the subject, the information hitherto collected being far from satisfactory. In the way of illustrating the disorder, we shall confine our remarks to the observations which have been made regarding the hip-joint. Three diseases have been enumerated as occurring here, tending to dislocation, and apparently not without cause; namely, the common hip-disease, *coxalgia*, hydrarthrosis, increased synovial secretion, and hypertrophy of the cellular and fatty apparatus within the joint.

Coxalgia. Hip-disease is generally assigned as a cause of congenital luxation by writers on these complaints; though we suspect from the general obscurity which has heretofore enveloped the subject, more frequently than was meet. Thus two cases of this sort are given by Albers in his 'Prize Essay on Spontaneous Lameness,' where the symptoms of coxalgia were present, but where M. Melicher suspects that the disease did not play so important a part as is assigned to it; and M. Ficker in his 'Prize Essay' has corresponding observations. Our author states that he has not discovered any well-authenticated case, where an infant came into the world with a spontaneous luxation, the result of morbus coxarius having reached its latter stage; though he has himself witnessed instances in which the disease had commenced in new-born infants, had also made some progress, and was apparently approaching to this melancholy consummation. It is, moreover true, that for the first few months of existence, the occasions of judging of the existence of the disease, whether arising from this particular cause, or from any of the others we have yet to advert to, are far from satisfactory, so that there is scarcely any possibility of detecting them. Medical men, moreover, are often not consulted till the time when the child was expected to have walked is passed, and the anticipations of the parents have been disappointed. Attention to the past history of the case, and a minute examination of the affected parts, can then scarcely fail to guide the judgment of the well-informed practitioner. The diagnostic marks, however, for this purpose, will be best enumerated when the other species of the disease have passed under review, and will, till then, be accordingly postponed.

Hydrarthrosis. The principal object of a late memoir on congenital luxation, published by M. Parise, is to demonstrate that this latter affection may arise from an anormal secretion of synovia, and also from a diseased state of what has been called the Haversian gland. An illustrative case of each of these disorders may stand in the place of more lengthened observations. M. Parise states that when he was house-surgeon of the Hôpital des Enfans Trouvés at Paris, he examined the joints of 332 new-born infants. Amongst these, he found only three in which congenital luxation existed from this cause; and the appearances were so similar in all, that the recital of one suffices for the exposition of the others. The subject which afforded this opportunity of examination, was an infant ten weeks old at the time of its death. It had been very much out of health generally. The great trochanters were found further apart than usual, and nearer to the crest of the ilium; the heels were approximated, and

the toes turned outwards. On both sides the head of the femur was found partially luxated upwards and outwards, resting on the crest of the acetabulum, where it had formed a slight depression for itself, which communicated with the normal one of a large size. The Haversian gland was not hypertrophied. The double cavity was filled with synovia, which flowed freely when the capsule was opened. The head of the femur and the trochanters were well formed, and the round ligament was long and on the stretch. The capsule was still entire, and the pelvis otherwise in a healthy state. (Parise, Arch. Gén. de Méd. t. xiv, p. 439.)

Hypertrophy of the Haversian gland. Several cases of luxation from this cause have now been recorded. Paletta gives one, and as illustrating the disease we subjoin a case. A male child, aged six days, died at the Hôpital des Enfans Trouvés, and exhibited a deformity in both hip-joints, which was found to be owing to a cause alike in both. The pelvis, femur, and surrounding muscles were normal. The head of the femur, slightly depressed backwards, did not correspond with the central axis of the acetabulum. This cavity was oval-shaped, having its larger extremity looking upwards and outwards. The deeper parts were filled with a small tumour of a crimson colour, whose texture was uniform, and of the consistence of lard. This tumour was evidently formed of an enlargement of the cellular and adipose structure, called the Haversian gland, which was thickened to the extent of about two lines, and which extended over a considerable portion of the cartilaginous surface of the cavity; and between these parts there was a whitish pseudo-membrane. At the first view the acetabulum appeared single and much inclined upwards; but, upon narrower inspection, a salient line was found to separate the upper and outer third, from the two lower and internal ones. The head of the femur was lodged in the former of these, and was displaced from the latter by the morbid growth. The round ligament was longer than usual, whilst the capsule appeared natural. From the appearances it was judged that very speedily the luxation would have been complete. (Parise, l. c. p. 446.)

The natural progress and termination of some of the varieties of this species, tedious and aggravated, occurring in vitiated constitutions, is to a most distressing consummation. This is well shown in a case which we copy from M. Melicher's pages. The most distressing termination, he remarks, of these cases, is complete anchylosis of the luxated bones, more especially the thigh-bone and the ilium. Thus, I had a patient, a boy, whose parents assured me that he came into the world labouring under the disease, the consequence, as his mother supposed, of an accident she received in the seventh month of pregnancy, when she was thrown from a carriage and fell upon the abdomen. The child was born at the natural time, and was weakly. It was sent to the country, and there somewhat recruited. When seven months old it was remarked, that the lower extremities were not in their natural position; a circumstance which appeared inexplicable, as the child had never been out of his parent's sight. At a later period it was observed that the child could not move its lower extremities at all. A variety of treatment was then adopted, but without benefit, so that latterly it was abandoned. When seen by M. Melicher,

the boy was 14 years of age. All the symptoms of congenital luxated femur were present, with this peculiarity, that the femur was united to the ilium by bony anchylosis. The pelvis was broad, and the rami of the pubis widely separated. The trochanters were prominent and lay close upon the ilium; the nates were elongated and much vaulted outwards; no force could move the femur, its head being firmly fixed upwards and outwards upon the external part of the ilium. The thighs were rotated inwards, the knees were closely approximated and almost immoveable. The legs again, extended outwards, as in the talipes valgus, the toes however turning inwards, and the heels outwards.

Notwithstanding the prominence which on this topic has long been paid to the subject of spontaneous luxations, we will not now longer dwell upon it. That prominence, we believe, has in no inconsiderable degree been owing to the very obscurity in which the subject has been enveloped, and which recent researches have gone far to remove. Much loose and general discussion—we had almost said declamation—has long prevailed, which we anticipate before the views we have now to detail will speedily disappear.

III. FUNCTIONAL LUXATION. *L. functionalis.* We must here say a few words in way of apology for introducing a new name for this species of luxation, which is not employed by our author, nor, so far as we know, by any other writer on the subject. We venture on this step, not because we attach any importance to a mere name, but because accuracy of terms is essential to accuracy of thought and discussion; and because much confusion and obscurity have resulted in medical, as in other branches of natural science, from carelessness and inaccuracy in nomenclature. One of the great sections into which the author has divided his subject, is that of acquired luxations, *L. acquisitæ*, and under this title he has arranged the species we are now considering. But this, so far as the nature of the affection is concerned, is anything but a specific distinction, and is calculated far more to perplex than elucidate. Many of our readers must be familiar with the fact, that the term *acquired* has already been appropriated in the cognate subject of club-foot, which, in fact, is only an example of that complaint on which we are now dwelling; and that it has thus been adopted in medical science as directly opposed, and in contradiction, to the term congenital; the former implying that the disease has made its appearance subsequent to birth,—the other that it has had an existence previous to that event. And many moreover must know, that however important one's birth may be in many particulars, in the matter in hand, whether club-foot, or the disease on which we are now dwelling, it is, in truth, of no kind of moment.

But without dwelling longer on this point, we remark, that we can in no way more effectually, or shortly, elucidate this important species of the so-called congenital luxation, than by exhibiting the true pathology of club-foot, or talipes, as lately expiscated; and by applying it to the whole class, corresponding to our present species, of which it is, in truth, nothing more than a specimen. The new views obtained in talipes must be ranged among the most brilliant improvements of modern surgery; and the light they throw on this hitherto obscure subject, is of the most satisfactory nature.

The different opinions which have hitherto been current regarding club-foot—and the remark equally applies to what has been called congenital luxations—are such as the following: 1st. That the original formation of the bones principally implicated has been unnatural and incomplete. 2d. That the bones, though originally formed perfect, became injured and distorted by causes independent of the formative process; as by the pressure of the fœtus, occasioned by irregular action of the uterus, anormal position of the fœtus, by blows, concussions, &c. &c. And 3d. That whatever may have been the condition of the bones previously, the act of walking displaced and injured them. (See Little on Club-Foot, p. xxii.) Upon these various and discordant views, physiology and pathology have recently shed their light, and brought order out of confusion. Scarpa was one of the first to elucidate the important truth that in talipes none of the tarsal bones were actually dislocated; but, in the anormal state of the ankle, undergo distortion of their axes, more or less extensive. Benjamin Bell, Boyer, and Jörg, again taught that irregular muscular action had an important influence over the disease, whether produced by the debility and paralysis of one set, or the over action and spasm of their antagonists, or from these causes conjoined. Finally, Rudolphi completed the true pathological views by demonstrating that congenital talipes, and the analogous disorders, arise from the disordered influence of the nervous energy of the muscles in the fœtal state, whereby irregular action is superinduced, and distortion and deformity result. Delpech's interesting cases had previously clearly demonstrated, that all these changes could be effected after birth, and even in adult life not less than in utero; and the genius of Thilenius, Stromeyer, Dieffenbach, and we must add our countryman Dr. Little, brought all these facts to bear upon practice. So that this irregular action, and its distressing effects, whether evinced in the most delicate muscle of the body, as in one of the recti of the eyeball, or in the strongest, as in the tendo-achillis—whether in the deformity of squinting or of club-foot—can be removed by an operation not more simple than effectual.

The application of the principles to which we have just adverted, to the subject of "congenital luxations," is too apparent to require any lengthened demonstration.

Instead of this species of luxation being considered as it really is, a mere distortion of parts originally well-formed, (whether occurring in the fœtal state or subsequently,) the popular notion is, that it depends upon some deficiency of the parts, some malformation, monstrosity, or arrest of development. The refutation of this, alike short and complete, is the scalpel, which shows the bones and other apparatus, somewhat distorted indeed, but otherwise normal.

The next cause assigned for this kind of "congenital luxation," and one now the most popular, is that it follows external injuries to the part during fœtal life; as from blows and shocks, abnormal action of the uterus, or malposition of the fœtus. A moment's consideration of the circumstances of the fœtus,—a solid body floating in fluid, and in other circumstances peculiarly calculated to prevent any such effect,—might, on mere mechanical principles, have demonstrated that such an effect could not be produced. But the most direct answer to this hypothesis is, that

these luxations take place after birth; and even sometimes in adult years, as well as in utero. It is ignorance of this fact that has thrown so much obscurity and confusion over the whole subject.

The true origin of the phenomena of these so-called "congenital luxations,"—what we venture to designate functional luxation,—is to be found in some cause resident within the organism of the fœtus. This is a point of so much interest, that we cannot better illustrate it than by a few sentences from Dr. Little's excellent work on Club-foot.

"A child during the progress of dentition, is observed to drag one leg, sometimes both after him; in other words does not possess the full voluntary power of moving it; he has a slight limp in his gait, depending on a slight rigidity or contraction of the muscles of the calf, and consequent stiffness or inability of bending the ankle-joint. The former case arises from partial paralysis of some of the flexors of the joint, the latter from spasmodic contraction of the extensors of the foot. The disease advances unchecked by medical treatment, and the result is one or other form of club-foot. In other cases during the period of life when the nervous system is most sensitive, a child debilitated by some violent disorder, or by a succession of infantile complaints, or by croup, or what is called a fit, is said to have lost the use of one or all his limbs, which disorder may be paralytic or spasmodic. This may affect the trunk, giving rise to deformity of the spine, or the shoulder, elbow, hand, producing the deformities known as club hand, and the scrag-hand, or the fingers; also the lower extremities, the hip, knee, ankle; in fact, every joint of the body. I have treated," says Dr. Little (and we have had such a case under our care), "a child born with spasm of the muscles of the eyes, of the spine, of the adductors of the thighs, and muscles of the calf, producing squinting, partial opisthotonos, rotation of the thighs inwards, and double talipes equinus. And the whole of these affections with the exception of the strabismus, have been almost entirely removed by the use of preparations of iron, calomel, rhubarb, and chalk, with long-continued extension, counter-irritation to the spine, with general friction and champooing."

The efficient cause of these muscular irregularities, which are to be considered as the dynamic cause of the "luxations," resides in the nervous system; either in the nervous centres—the brain or the spinal cord; or the disease exists in some other organ of the body implicating the peripheral parts of the nervous system; for instance, some of the abdominal viscera, the incident nerves of which are morbidly affected; these communicate in the spinal cord, with other filaments—the reflex, or involuntary motor nerves—whereby the muscles of the deranged part are excited to spasmodic action.

Upon this theme we could willingly enlarge, but we forbear; and chiefly because we believe enough has been said to satisfy every intelligent reader of the true nature of this species of the long obscure and perplexed subject of "congenital luxation." The view has all the charm, all the simplicity, and force of truth.

One variety of this species of luxation, as we believe, must not be passed over, as we have not noticed it mentioned by any other writer on the subject, save our author. He calls it *luxatio congenita costarum*, and remarks, that in large hospitals, children are observed to come into the world, emaciated, small, and weak; in many of whom the ribs are more or less compressed, the sternum and abdomen projecting, the spine inclining backwards. In others, several of the ribs, at their junction with the

sternum, especially the fourth, fifth, sixth, and seventh, are found strongly projecting, as if broken. There is however no fracture. The appearance is produced in the cartilages connecting the ribs to the sternum. These are distorted, generally at their costal extremity, sometimes at their sternal. In after years, this constitutes what is familiarly known as chicken-breast. This affection may occur simultaneously on both sides, or be confined to one only. Generally, several ribs are thus "luxated," seldom one or two. The deformity is most frequently found in the four under true ribs.

No difficulty need, of course, remain from the name which has so long been imposed upon cases belonging to this important species. They received the name of *luxation* when their pathology was unknown; just as club-foot was held to be a luxation of the ankle-bones, when it was nothing more than distortion. And they were called *congenital*, because, doubtless, they often do occur before birth; although this is far indeed from being an essential attribute.

And this leads to an observation to which we must revert, after treating of the remaining species of these congenital luxations, namely, that frequently, during the first few months after birth, they are not detected by the relatives or attendants. The cause may have operated in utero, but the child at birth is to all appearance in good health. It continues so; and, if the infirmity be in the lower extremities, these not being brought into play in the first months of existence, it is only after these have elapsed that the disease begins to declare itself. Something also must be said in a subsequent page upon the diagnosis. And the powers of the art on this species of the disorder are not to be contemned. They were of purpose alluded to in the most aggravated case detailed in a former page, and are assuredly of a very high order.

But our contracting space reminds us that we must hasten to the last species of congenital luxation, which has to be brought under notice, perhaps the most singular of all; and which, thanks to the improvement of modern pathology, has at length received all necessary elucidation.

IV. ORIGINAL LUXATION. *L. originalis, Luxation originelle* (Dupuytren), *L. congenita*, Claudicatio, Congenital malformation. This fourth species of congenital luxation, though not altogether unknown in this country, has yet so recently been described, and is so far from being familiarly known, that we shall only be doing good service in presenting, in this its natural connexion, a summary account of it. It is undoubtedly the most striking and melancholy species of the four; and cases are even now coming into our hospitals bearing indelible marks that they had previously been misunderstood. It embraces that considerable class, wherein is included the most aggravated cases of lameness and distortions,—of unfortunate cripples, who cannot be seen without exciting heartfelt sympathy and pity.

The original luxation is apt to occur in most of the free joints, as in the shoulder, elbow, and wrist, in the hip-joint, knee, and ankle; and, for very obvious reasons, it is most afflictive in the larger of them. In them also it is unfortunately the most frequent. As our space forbids us to enlarge, and our object is merely to illustrate, we shall confine our attention chiefly to the hip-joint; the nature and effects of the disorder being,

mutatis mutandis, substantially the same in the others, so that if understood in this case, the disease will readily be comprehended in them all.

The variety of congenital luxation, we are now to consider, was first accurately explained to the profession nearly twenty years ago, by Baron Dupuytren, whose description for vigour and accuracy has not been excelled; and the most important additions made to the facts stated by him, are those collected by Mr. Adams of Dublin. The characters of this luxation, according to the Baron, are a shortening of the affected member, the ascent of the head of the bone into the external iliac fossa, the projection of the great trochanter, the retractation of nearly all the muscles of the upper part of the thigh towards the crest of the ilium, where they form, round the head of the femur, a kind of cone, whose base is at the ilium, and whose summit is the great trochanter; the almost total denudation of the tuberosity of the ischium deprived of its muscles, the rotation of the limb inwards, and consequent abduction of the heel outwards, and the knee and great toe inwards, an obliquity which increases with advancing years, and as the pelvis increases in size, whence results a tendency in the femurs to cross each other inferiorly, making an acute angle where it is attached to the pelvis, and finally, great emaciation of the lower limb, more especially of its superior part.

The isolated movements of the deformed members are in general very confined, especially those of abduction and rotation. Hence innumerable difficulties in standing, walking, and every other movement in which the limbs play a part.

When the person is standing, we are struck with the want of proportion between the upper and lower parts of the body, the imperfection of the lower limbs, and the attitude of the individual. The trunk is well developed, while the lower limbs are short and slender, as if they belonged to a much smaller person. The diminutive size of their limbs appears the more striking on account of the great breadth of the pelvis, and we are surprised with the projection of the trochanters. Regarding the attitude, we observe that the upper part of the trunk is borne far back, and the spine projected forwards, and very hollow behind; the pelvis is placed on the femurs nearly horizontally, and the unfortunate sufferer does not touch the ground except with his toes.

A person thus deformed, when about to walk, balances himself on his toes, then wholly inclines the upper part of his body towards the member upon which he is about to repose his weight, raises the opposite part from the ground, and then painfully transports his weight to it. In fact, every time that this change is effected, the head of the femur, which receives the weight of the body, is elevated in the fossa of the ilium, the pelvis descends, and all the signs of displacement become more marked on this side, while they become less apparent on the other, till it, in its turn, receives the weight of the frame; and it is by this succession of efforts that the weight of the body is alternately transposed from one limb to the other. The cause of all these painful efforts is most evidently the loose condition of the head of the femur, and its continual displacement upwards and downwards, as it receives, and is freed from, the weight of the frame.

At first view, it appears singular that running and leaping to such a person should be easier than walking. This however is the fact. And for

this reason, that in running the energy of the muscular contraction, and the rapid transfer of the weight of the body from one leg to the other, renders the deficiency of the acetabulum, and the looseness of the head of the femur, almost inappreciable. This mode of progression however is so fatiguing to these individuals that they cannot continue it long.

When persons afflicted with this infirmity lie on their back, we are astonished to find that the signs of their infirmity diminish and disappear; which is owing to the circumstance that when thus in repose, the muscles cease to elevate the femurs, and the weight of the upper part of the body, like a cone, presses down the pelvis between the femurs. What authenticates the accuracy of this explanation is, that in this position you can elongate or shorten the limb at will by slight traction, and to an extent of two or three inches, according to the age of the patient, and the extent of the displacement; and all the appearances are at once affected thereby. All these changes are, moreover, effected not only with great ease, but without pain, clearly showing that there is no kind of disease, properly so called, present, and nothing more than a want of the acetabulum and attending alterations.

The *Lusus naturæ* here is so plain as scarcely to require any explanation. According to Dupuytren all the implicated muscles are very much drawn up towards the crest of the ilium, and they are more or less atrophied; some of them being very much converted into mere fibrinous tissue. According to the same authority, the upper part of the femur maintains very much its natural form and dimensions. The acetabulum is either wholly wanting, or is represented by a small osseous projection, in which there is no trace of cartilage, capsule, or rim. Occasionally the round ligament may be seen, but much changed. The head of the femur is found lodged in a cavity analogous to those met with in unreduced luxations, running upwards and backwards. This new cavity, superficial and rimless, is situate on the external iliac fossa, and has limits differing in different cases.

It is worthy of notice, that within Dupuytren's observation this distressing affection occurred but rarely on one side of the pelvis only, and much more frequently upon both. He states that out of twenty-six cases he had witnessed, the luxation was confined to one side only in two or three persons. This experience, however, does not seem to correspond with that of others. Our author states that Professor Chelius had witnessed nine cases, and four of these were on one side only; and Mr. Adams mentions that he had seen the affliction more frequently upon one side than on both. This comes to be an important fact in the matter of diagnosis, and therefore should not be overlooked. It has also considerable bearing on the symptoms and pathology, and should not be disregarded.

When one side only is implicated, remarks Mr. Adams, the weight of the body is thrown nearly wholly on the sound limb, which becomes stronger and larger than usual, while the other becomes more or less atrophied, its circulation languid, and its nervous energy and temperature diminished,—phenomena which are apparent in both limbs when both are affected.

Mr. Adams adds, that when one hip only is abnormal, we find that a

lateral curvature of the spine exists, and the bones of the pelvis are atrophied on the malformed side. He also observes that the os innominatum of the deformed side, together with the femur, and other bones of the lower extremity of the same side, are much smaller than the corresponding bones of the sound extremity; and the former, besides being deformed, are in a state of atrophy in circumference and length, while those of the latter are evidently larger, and better nourished than we could expect in such delicate individuals. In a word, there is a compensation growth of the skeleton on the sound side, to make up, as it were, for the deficient growth of the other side; the head of the femur, too, and the acetabulum are both very large, as is the whole of that side of the pelvis.

Another particular in which Mr. Adams's observations differ from those of Dupuytren, and we believe corrects them, is that wherein the Baron remarks that the upper part of the femur very much preserves its form and natural arrangements. According to Mr. Adams there is, on the contrary, a marked change in these particulars. 1st. The neck of the femur, instead of having its axis directed, as it is naturally, upwards and outwards, loses its natural relation to the shaft of the bone, and the axis is directed upwards and almost straightforwards. And 2d. The head of the bone instead of being directed backwards, as in ordinary luxation, on the dorsum ilii, on the contrary, is directed forwards, and placed beside the anterior inferior spinous process of the ilium, while the trochanter major is directed backwards on the dorsum ilii. The following remarks likewise are too valuable to be omitted: "The anterior spines of the ilium, particularly the inferior, we have usually found to be directed very much inwards towards each other,—the external iliac fossa to be much more convex, and the internal to be much more concave than usual; beneath the anterior inferior spine, we notice a deep groove directed outwards. The sub-pubic angle is remarkably obtuse, the rami of the pubis and ischia are very oblique, and the tuberosities of the ischia everted. (Todd's Cyclopædia, vol. ii.)

It should here, as before hinted, be particularly noted that though these appearances are so marked on dissection, and so striking in the adult, yet are they far from being conspicuous in the infant. If called to examine a case shortly after birth, there are no doubt indices of the vicious malformation, such as the great breadth of the pelvis, the projection of the head of the femur, the obliquity of its shaft, &c. &c. But it usually happens that the abnormal conformation, and the infirmities which result from it, do not attract the attention of the parents and attendants till the time when the child should begin to walk, or actually attempts it, and it is in these circumstances only that the surgeon is appealed to. Then it is ascertained that the child has great difficulty in standing, and still more in walking. But it frequently happens that the friends even then shut their eyes against the calamity which threatens, and making their wish the father of their thought, cherish the conviction that the child is backward only as it respects its walking powers, and will not admit for years that there is any distinct infirmity, not until the defect and imperfection in the form and action of the parts have become so conspicuous, that it appears altogether unreasonable to attribute them to any mere retardation of development and action.

All this shows the importance of an accurate *diagnosis* between affections which it has been remarked, in infancy especially, are so analogous in their symptoms, and so different in their origin, nature, and treatment—the diagnosis between this *original* luxation, and each of the three species which have preceded. Great assistance is here procured from the absence of all pain, swelling, abscess, fistula, or cicatrix, which are more or less found in all the others; and also from the fact that the subject of this defect is generally in all other respects in good health. In obstetrical luxations, and other accidents, there are always present the local signs of injury and contusion; in spontaneous, the symptoms of more confirmed disease, both local and constitutional,—a remark which may be extended to functional luxation. The coincident appearance of this deformity on both sides of the body, though not so common, and therefore not so pathognomonic, as supposed by Dupuytren, is yet, where it occurs, of great value. Although there is no pain in cases of original luxation, either about the hip-joint or the knee, yet there is great inability to move, and great fatigue from the attempt. Though there is no morbid swelling about the parts, there is very marked projection of the trochanters, and the soft parts which surround it, and much unwonted movement in them, wholly unknown in the other exhibitions of the disease. The change of form also in this species is very marked, and differs from that which occurs in the others, as is well illustrated by M. Melicher in his 2d plate, (figs. 3, 4, 5, 6, and 7.)

Such then is an illustration of this congenital luxation as it occurs in the hip-joint, apparently its most common seat. It is by no means, however, confined to this articulation; and to Mr. Smith's pen are we recently indebted for a most lucid exposition of its occurrence in the shoulder-joint. He details five cases observed in Dublin; three in which the luxation was sub-coracoidal, and two in which it was sub-acromial. Of the former, two were in the one shoulder, the left, and one in both. Of the latter, one was in the left, the other in the right side. Mr. Smith also discovered unequivocal evidence of the existence of other cases in several museums, and has brought under this category various anomalous cases which flourish in the archives of the science, as perplexing instances of partial and incurable dislocation. In the examples of this congenital disorder the member was nearly useless, in some entirely so. (Mr. Smith's Paper in Dub. Journ. of Med. Science, 1839.) The same disorder has been observed at the elbow- and knee-joint, also in the radius and ulna, and the tibia.

Upon the *cause* of this original luxation we need not enlarge. That it is congenital will be disputed by none; and that it is original, and takes date from the first organization of the articulation, will be conceded, we believe, by all who take the trouble to investigate the case. It appears to be a defect of the organization in the very germ. This hypothesis accounts for the double luxations, not less readily than the single; and moreover for the perfect health, in all other respects, of the new-born infant, and for its thriving for months and years, with this exception. In perfect keeping with this view some physiologists have combined the speculation, that with the local defect in the articulation there is a corresponding lesion in the brain or other part of the nervous apparatus. This agrees

accurately with what has been previously stated as the cause of functional luxation; and though there are not wanting grounds upon which this speculation has been based, as it respects the present species, we have now no time to adduce them.

That original luxation should occasionally show itself hereditarily will excite the surprise of none. Commencing, though frequently covertly, with the primal days of infancy, it maintains a steady and obstinate course, becoming rather more aggravated with advancing years, though in a few cases, all things considered, there is a surprising degree of agility. Dr. Maissiat, who communicated the details to Dupuytren, informs us that in one family where the disorder showed itself hereditarily, four individuals, who were related to each other, as aunts and nieces, were respectively of the ages of 70, 80, and 84. The niece had a daughter who suffered under an original luxation in her right femur; and this last individual had four children, two of whom had the same deformity, the one on both sides, the other only on the left side.

It might, at first sight, be apprehended that for a complaint, so serious as that on which we have been dwelling, no remedies would be of any avail; but in many cases, at all events, this is not the case. Not that we speak of cure,—but of relief. Scrupulous attention must be paid to the general health, and double diligence bestowed on the infirm member, by the use of friction, champooing, and gentle traction, whereby its nervous and muscular energy may be augmented. And the palliative remedy, so far as the hip-joint, for example, is concerned, is the accurate and firm fitting of a kind of girdle to the pelvis, which is wadded and furnished with a groove for the head of the femur, so that its movements may be restrained, and much more assurance given to the step, and stability to the frame. What has been so successful in the hip-joint, could not fail in similar cases, with equal ingenuity, to be not less so in the others, and the patients' comfort thereby be much increased.

Thus, then, in reviewing the whole subject of this monograph, we find that the topics it embraces involved, even in our own time, in almost impenetrable confusion and obscurity, with the advance of the science, present entirely a new phase. Regarding congenital luxation as a genus, or natural group, we find that it very readily divides itself into the species of *obstetric*, *spontaneous*, *functional*, and *original* luxation, all having many points of resemblance, and yet each very distinct in its nature from its fellows. Viewed severally that nature is simple, and its treatment clear, and the diagnosis, to a careful and well-informed practitioner, can seldom remain long doubtful. We are far from asserting that the subject is even now exhausted. But sure we are that there is abundant ground for gratulation on account of the advances which have recently been made, including some of the brightest triumphs of modern surgery, gratifying alike to the admirer of his art, and the well-wisher of his kind.

Art. VII.

Mémoires de l'Académie Royale de Médecine. Tome XI.—*Paris*, 1845.

Memoirs of the Royal Academy of Medicine of Paris. Vol. XI.—*Paris*, 1845. 4to, pp. 684.

Of the three *éloges* with which the present volume of Memoirs opens, there is one which will be read with deep interest,—we mean that of which Esquirol forms the theme. Strong and enthusiastic as are the epithets which flow from the facile pen of M. Pariset, they do not give an exaggerated picture of the talents and the virtues of the man;—the simple events of his life, the respect with which his memory is honoured in his native land, the impulse he gave to the philosophical study of the terrible maladies with which his name is indissolubly connected, and the vast improvements he introduced in the actual practical comprehension of those maladies, his tried benevolence, charity and lovingness of character, all give warranty to the encomiums of that accomplished writer. It is good, it is gladdening to read all this,—and to know that eminence, distinction, and respect, such as Esquirol gained, may be achieved by a man who started originally in life with scarcely a sou in his pocket.

Of M. Double and M. Bourdois de la Motte, whose lives are likewise briefly narrated, the latter was very little known beyond his own immediate sphere; the former, too, had long ceased to mingle much in medical circles, though a member both of the Institute and Academy of Medicine.

Although we retain unchanged the feeling which sometime since led us to decry the system of indiscriminate laudation adopted in France, in regard of defunct members of learned societies, still, we believe, that it is both wise and proper that some record should be kept of the lives of all the deceased members of our profession. For this reason, we perceive with considerable satisfaction, that the editor of the 'London Medical Directory,' has this year commenced the plan of briefly recording the ascertainable particulars of the lives of those whom the preceding twelve months may have consigned to the grave.

M. Frederic Dubois supplies an interesting essay on the recent progress of medicine and of surgery in France; this will be read with advantage, but does not bear condensation.

I. *Memoir on urethroplasty*, by M. Ségalas. Under this title M. Ségalas describes the modifications which he has introduced in the mode of applying plastic surgery to cure of openings in the urethra. In a letter, which this surgeon addressed to M. Dieffenbach in 1840, he endeavoured to show that the frequent failure of urethro-plastic operations depended upon the great difficulty experienced in preventing the contact of the urine with the parts which the operator proposes to unite; and that hence the first condition of success was the establishment of a temporary channel for the discharge of the urine through the perineum, by means of a gum-elastic bougie retained in the wound. In the present publication, he more particularly seeks to give additional support to his views by the narration of another (his second) successful case.

To detail all the circumstances of the operation would occupy more space than we can afford; but a few particulars will suffice to make the general nature of M. Ségalas' proceeding comprehensible. The patient, a working shoemaker, aged about 30, had lost a considerable portion of the spongy part of the urethra at the age of six years, the missing part having sloughed away in consequence of the owner having playfully tied up the penis with a string. The entire thickness of the urethra, for about an inch in length, was wanting, when the operation was undertaken; the urine and semen of course passed by the posterior opening. The portion of urethra lying in front of the hiatus described, was contracted, but perfectly pervious. The man's general heath was excellent.

The operator commenced (21st July 1841) by placing a small gum-elastic bougie in the urethra. Finding that this instrument caused no particular inconvenience to the patient, though allowed to remain *in situ* for a few days, M. Ségalas, at the expiration of these, incised the prepuce at its upper part, in order to give all possible freedom to the structures to be brought in contact,—a preliminary step, the more important as the patient was affected with phymosis. Six days after this little operation (which produced not the slightest constitutional disturbance), M. Ségalas opened the membranous part of the urethra through the perineum, passed a gum-elastic sound through into the bladder, and then by means of an œsophagus-tube, transforming this sound into a siphon, caused the urine to pass away continuously through the perineum. Sixteen days after (the wound in the latter place having ceased to give the least pain, the patient being in good health, and not annoyed by erections), the closing step of this complicated operation was performed as follows:

"I made two semi-elliptical incisions in a transverse direction on the inferior surface of the penis, in front of and below the hiatus caused by the loss of substance: these incisions met each other on each side of the lateral aspect of the corpus cavernosum. I then carefully denuded the entire surface circumscribed by these two incisions, and bringing together the two lips of the wound thus formed, I united them by means of six points of twisted suture, one on the median line, three to the right, and two to the left. My intention had been to make a third point of suture on the latter side; but an observation made by one of the persons present, that the swelling might cause too great an amount of compression between the parts united, prevented me from doing this, and I left a much wider space between the two needles in question, than between those of the opposite side"

M. Ségalas attaches much importance to this omission, tracing to it the difficulty subsequently encountered in closing a small fistulous opening in the site, where he had proposed, but omitted, to place an additional needle. A minute opening still existed in the month of February following, which was finally closed after repeated failures by cauterization, by a point of twisted suture. The cure continued perfect, when the patient was last seen in July 1844. Three engravings exhibit the progress of events in connexion with the operation. Nothing is said of the effects of the cicatrices on erection of the penis,—a matter of no small importance.

II. *An Essay on the two diseases known under the name of meningeal apoplexy,* by Dr. R. Prus. These two diseases are hemorrhage between the arachnoid and pia mater: and hemorrhage into the cavity of the

serous membrane. Both varieties are illustrated by a certain number of cases (not remarkable for closeness of description in any respect); and the relation of these is followed by a general account of the affections.

A. To commence with the first, whether ***blood effused into the subarachnoid tissue*** has made its way there in consequence of exhalation or rupture of a vessel, it may be found in clots or liquid, may be spread in a layer on the outer surface of the convolutions or penetrate into the anfractuosities, and even pass through the substance of the pia mater. The most extensive effusions occur at the base of the skull, and are most frequently due to rupture of an artery. A peculiarity in these effusions is that, although the progress of the symptoms render it unquestionable, that escape of blood may have, and sometimes must have, taken place on two or more successive occasions, no differences exist in different parts of the extravasated blood significant of a difference of age: the solution of the difficulty suggested by the author, namely, that the want of such appearance depends on the short duration of the disease, is not altogether satisfactory, as indeed he himself admits. The formation of pseudo-membrane round the effused blood is slow to occur in this situation: a case related by Morgagni, shows that no trace of its development may be discoverable even eight days after the hemorrhage has occurred. The author conjectures that the deficiency of false membrane may depend on the flux and reflux of the cephalo-rachidian fluid; but to the movement of this fluid, he conceives, the disappearance of blood effused in this situation (when not in sufficient quantity to cause death) is possibly altogether due. Coagula contained within the ventricles become invested with pseudo-membrane.

Hemiplegia is a very rare effect of the description of hemorrhage under consideration. Of twelve cases of rupture of arteries, two only were attended with this symptom; and in no instance was rupture of the veins or sinuses productive of paralysis. This peculiarity may very probably depend, as M. Prus observes, either, 1st, upon the fact that the sub-arachnoid tissue being naturally destined to receive the cephalo-rachidian fluid, is capable of giving lodgement to an undue quantity of blood without ill effects; or 2dly, that the effused blood being spread over a large surface by that fluid, exercises no special pressure upon any point in particular of the cerebral surface. In a certain class of cases, however, where a very large quantity of blood is thrown out, such marked compression of the brain arises, that sudden and complete annihilation of power generally takes place, and in consequence no local paralysis can be particularly traced.

In the cases observed by M. Prus the only invariable symptom was coma, preceded several times by general discomfort and prostration. Cephalalgia, and redness and heat of the integuments of the face, are rather to be considered in the light of premonitory symptoms, than evidences of the actual occurrence of hemorrhage. Slight delirium was noticed in one case; the intellectual faculties are scarcely ever perverted, but for the most part weakened.

The course of the disease, commonly continuous, is in some instances intermittent,—obviously a consequence of successive hemorrhages taking place. Death occurs more rapidly in certain cases of sub-arachnoid hemorrhage than of any form of effusion of blood into the cerebral structure itself; the prognosis, too, seems absolutely without hope, as death

was the invariable result, either immediately or within the space of eight days, in the cases brought together by the author. No appearance of cysts has yet been discovered in the sub-arachnoid cavity.

B. *Effusion of blood into the cavity of the arachnoid*, always occurs by exhalation, and not in consequence of rupture of vessels. The blood does not stain the cephalo-rachidian fluid,—seeing that it does not come into contact with it; and, when it has been extravasated some five days or so, is invariably invested with a pseudo-membrane. Differences in the characters of different parts of the coagula, significant of differences in the periods of effusion of the blood, have frequently been noticed. Intra-arachnoid coagula, generally adhere closely and firmly to the external lamina of the serous membrane; traces of absorption through the agency of cysts have sometimes been discovered.

It will be observed, that all these anatomical conditions are different from those noticed in sub-arachnoid hemorrhages; and a corresponding difference in symptoms may be traced. Paralysis, at least of movement, is common, having occurred in six of eight cases related by M. Prus; paralysis of sensation is less usual. In three of these eight cases sudden loss of consciousness occurred. Somnolence and coma occur almost invariably towards the close of cases of both these forms of hemorrhage; but in the intra-arachnoid form, cephalalgia, dryness of the tongue, fever, and delirium, are almost always observed,—and these symptoms are doubtless the effects of inflammation of the arachnoid, itself due to congestion either preceding the hemorrhage or caused by the irritative action of the effused blood. Delirium commonly occurs about the fourth day in cases of intra-arachnoid hemorrhage,—the precise period at which the investiture of the clot with pseudo-membrane becomes apparent.

M. Prus contrasts the symptomatic characters of meningeal, with those of cerebral hemorrhage, somewhat in the following wise. Sudden loss of consciousness existed in three (or rather but two only, as in one of the three death was immediate) out of fourteen cases of meningeal apoplexy; it is unnecessary to state how different the proportion of cases would be in cerebral hemorrhage.

The paralysis of movement occurring in intra-arachnoid hemorrhage, is not generally as complete as in effusion into the cerebral substance,—and is but rarely, and then temporarily only, accompanied with paralysis of sensation. Again, paralysis of movement, resulting from meningeal apoplexy, may completely disappear,—a most rare result in old subjects, where the symptom is produced by cerebral hemorrhage. Deviation of the mouth, so common in the latter species of effusion, is extremely rarely met with in cases of meningeal apoplexy; in the single case in which M. Prus observed the symptom, the patient had previously had hemorrhage of the brain itself. In fine, somnolence and coma constitute, from their intensity and continuance, one of the principal characters of meningeal apoplexy.

The duration of the sub-arachnoid apoplexy does not, according to the experience of M. Prus, exceed eight days; it is always fatal. The duration of intra-arachnoid hemorrhage may extend to a month and upwards; and recovery may take place, as shown by the discovery of cysts in the cavity of the serous sac.

The only point regarding treatment to which M. Prus' observations would lead, is that in cases of intra-arachnoid hemorrhage, active and sustained antiphlogistic measures should be put in force, for the purpose of controlling the arachnitis commonly ensuing. Although this result is sufficiently sterile, the fault rests not with M. Prus; some totally new light must be thrown upon the physiology and pathology of the cerebral organs, before any solid improvement in the treatment of the affections to which they are subject can fairly be expected.

The essay we have analysed is a useful production;—it certainly contains no single fact or inference which might not have been proved with the aid of cases collected by persons who observed long before M. Prus; but it has brought together and placed in a fit condition for comparison various facts which had previously not been so strikingly arranged beside each other. The author is unjustly silent concerning the observations of MM. Cruveilhier and Durand-Fardel; and omits all mention of Dr. Carswell's clear description of intra-arachnoid hemorrhage, probably from the pressure of that species of literary ignorance, which, originating partly in vanity, partly in dishonesty, has become characteristic of the French school of medical literature.

III. *Essay on œdema of the glottis*, by M. Valleix. The essay of M. Valleix was composed in answer to a question put by the Academy of Medicine of Paris, to the following effect: What are the causes of œdematous laryngeal angina (œdema of the glottis); its mode of progress, successive symptoms and "differential diagnosis;" what is the fitting treatment of the disease, and what the advantages and the disadvantages of tracheotomy?

Forty-three cases, collected from different sources, form the basis of the direct description of the disease given by M. Valleix; and he has availed himself of nine other cases in examining the subject of diagnosis. A short inquiry into the literature of the subject, and into the claims of Boerhaave and Bayle as the first describers of the disease, precedes the account of its anatomy: Bayle is pronounced the original historian of the disease.

M. Valleix well observes that, with whatever justness the charge may frequently be urged against the pursuits of morbid anatomy, that the detection of anatomical changes explains but little of the nature and phenomena of diseases, such a charge is totally pointless in the case of œdema of the glottis. The anatomical changes here, in truth, constitute the whole malady: take away these, and the total disease is gone. Hence the importance of closely and thoroughly examining them.

The infiltration of the sub-mucous tissue of the glottis may be simply and purely serous, sero-purulent, or actually purulent—a fact showing the necessity of giving a somewhat different title to the disease than that in habitual use. Pure serous infiltration is comparatively rare; some cases of the kind are recorded in which the laryngeal œdema arose in the course of general anasarca. Now, in these cases, the persons who observed them were of opinion that the laryngeal œdema was a consequence of the general state, and had occurred quite independently of local inflammation. A case, transcribed from a weekly print by M. Valleix, contains a strong asseveration on the part of at least four competent persons, that not the

smallest trace of inflammation could be detected in the part. But the author, not content with this statement, proceeds to show, as he conceives, that inflammatory action might have been present, and left no anatomical evidence of its existence. We confess ourselves unable to descry the utility or philosophy of thus contending against the admission of plain and obvious facts.

In more than three fourths of the cases brought together by this author, the infiltrated fluid was either purulent or sero-purulent. In such cases the upper opening of the larynx is more or less completely plugged with a soft circular fold of tumid membrane, somewhat larger generally on one side than the other. The epiglottis is pushed upwards towards the base of the tongue, and its edges sometimes included in the infiltration. The mucous membrane may be perfectly pale.

With regard to the localization of the infiltrated fluid, M. Valleix finds that, in twenty-four of thirty-two cases the aryteno-epiglottic ligaments were distinctly affected; in the other eight cases no very accurate limitation of the affected parts is made, but in all the edges, the *lips* or the *periphery* of the glottis are said to have been implicated. In four individuals the infiltration had reached the epiglottis; in one, the uvula; in two, the pharynx generally. The abundance of fluid greatly varies; nor is it possible to supply any very just measure of its amount as a general guide. M. Louis has observed some cases which would appear to show that a certain quantity of fluid may disappear from this site either after death or during the closing moments of existence; at least, he has found the aryteno-epiglottic ligaments *wrinkled* in such manner, as to make it probable that they had previously been distended by fluid. At the opening of the glottis are seen two swellings, more or less rounded and prominent, and tending, where of large size, to fall downwards on the upper opening of the larynx. A remarkable fact concerning these swellings, and their tendency to fall downwards, is, that the air is readily enabled to escape outwards by pushing them aside, whereas, the more strong the effort to force air inwards, the more effectual becomes the morbid resistance to its passage. Experiments on the larynx after death in these cases have demonstrated these facts in the clearest manner, and their obvious connexion with some of the peculiar symptoms of the disease cannot be overlooked.

The mucous membrane of the larynx was healthy in *three* cases only, in the parts corresponding to the seat of the serous infiltration; in two of these cases, however, the cartilages were diseased. In *six* other cases the mucous membrane is simply said to have been discoloured; but in five of the six there existed either a morbid state of the cartilages, ulceration of the trachea, or inflammation of some part adjoining the glottis. In *all the remaining cases* the mucous membrane exhibited distinct anatomical alteration. It was red in one third of the subjects, ulcerated in about one sixth of them, and raised up, dissected, as it were, from the subjacent parts by pus. M. Valleix has in one instance seen it greatly softened. In one case greenish discoloration existed; in another three obvious patches of gangrenous character.

Next to the mucous membrane the cartilages were most frequently affected, suffering in sixteen cases, or upwards of one half of the whole. Six times they were carious, or even, in one case, destroyed to a certain ex-

tent. The cricoid cartilage is that most frequently affected. The various changes observed appertain to laryngeal phthisis, which is here, M. Valleix observes, a powerful cause of œdema of the glottis; but it may be observed that M. Louis met with but three examples of œdema of the glottis among all his post-mortem examinations of consumptive patients.

Passing over some other anatomical changes, less important on account of their rarity than those noticed, we may state, as the result of the analysis of facts undertaken by M. Valleix, that there are but a very small number of cases indeed, in which it can, even on first view, be supposed that œdema of the glottis had occurred independently of organic change, and that in these very few cases, close examination of the entire history gives motive for doubting the real absence of such change. Hence it would follow that the various morbid states enumerated are the real immediate causes of the infiltration constituting œdema of the glottis.

The ages of 36 patients were as follows:

Ætat.	Cases.	Ætat.	Cases.
0 to 10	2	40 to 50	8
10 20	5	50 60	5
20 30	8	60 70	3
30 40	4	71	1

The five cases set opposite the decennial period, ten to twenty, occurred in persons aged from 18 to 20 years. It is rendered probable by these facts, that from the eighteenth to the thirtieth years is the period at which the tendency to this disease reaches its maximum. Males and females suffer with different degrees of frequency; the former furnished 29 cases, the latter 11. The facts collected by the author, bearing upon constitution, temperament, and season of the year, as causes of the disease, are, as well from their small number as from other causes, completely without signification. This he admits.

The considerations into which the author enters on the subject of the state of health existing at the period of occurrence of the œdema are of practical interest. In four of forty cases the individuals were in the enjoyment of good health when the disease appeared; in other words, in one tenth of cases of its occurrence it is a phenomenon or effect of simple angina. In but one of these did the attack prove fatal. A case of the kind is related, in which life was saved by tracheotomy, timeously performed.

The cases of non-laryngeal disease, in which the œdema supervenes, were as follows:

During the course of or convalescence from typhoid fever	10
During convalescence from pneumonia	4
In erysipelas	1
After scarlatina	1
After lithotomy	1
During the treatment of a fracture, accompanied with fever	1
During convalescence from cerebral congestion	1
During an attack of pulmonary catarrh	1
In hypertrophy of the heart	1
In elephantiasis	1

The diseases particularly exercising their action on the larynx, or actually primarily affecting that organ, were these:

Laryngeal phthisis, without pulmonary phthisis	2
Laryngeal phthisis, following pulmonary phthisis	7
Cancer of the larynx	1
Syphilis	2

From all this it appears that: 1. Œdema of the glottis is most commonly due to angina, occurring in the course of some acute or (less frequently) chronic disease. 2. More uncommonly it arises at the termination of chronic laryngitis. 3. In a limited number of cases, simple angina, occurring in healthy persons, may entail an attack of œdema of the glottis.

The invasion of the disease is rarely what can be called absolutely sudden; but it is very often *rapid*—that is, being once developed, it reaches its maximum severity in one, two, or three days.

Pains existed in thirty-two cases in the region of the larynx—pain of great or of trifling severity, or merely constituting a sensation of fulness or of the presence of a foreign body in the part: it was really severe in but five cases. In three persons, fluids, which they attempted to swallow, re-escaped in part through the nose: in one of these the epiglottis was ulcerated; in a second very much infiltrated with serosity; in the third the epiglottis was not diseased at all. The latter case (extracted from M. Louis' work on Phthisis, case xlv,) is the more extraordinary, as nothing existed either in the pharynx or upper part of the glottis to account for the symptom in question.

Cough is only spoken of in fourteen of the cases analysed by M. Valleix. It was frequent in a few instances only; sometimes dry, generally accompanied with expectoration. In one person it was paroxysmal, in another brought on by pressure on the larynx. This symptom appears to have had but slight importance of any kind, except in one instance, where it assumed the *croupal* character.

Change in the character of the voice is among the most invariable symptoms: it is probably even constant. The tone is very rarely croupal, however, having been noted with this peculiarity in but one case,—a different one from that just referred to.

Obstruction of respiration, of greater or less intensity, is the main symptom of the disease: in two cases only was it slight; in all the others, at least from time to time, excessive. But the most remarkable phenomenon connected with respiration in this disease is the inequality of degree of obstruction in inspiration and expiration. M. Valleix has scrutinized the cases in respect of this condition with very particular care; the results are in the following wise. "In twenty-four cases in which the state of the first and second divisions of the respiratory act is carefully noted, *inspiration* is distinctly said to have been whistling or sibilant fifteen times. In eight cases it was noisy, and sometimes attended with peculiar characters; thus M. Legroux discovered a sound like the crow of a cock, the flapping of a valve, a rustling sound, &c.; other persons have found the inspiration hoarse, rattling, &c. In the remaining twenty-fourth case, inspiration is simply said to have been extremely laborious. *Expiration* is only mentioned in eighteen cases, all of them among those in which inspiration was noisy and laborious. In thirteen of them, expiration was free, easy, and unattended with noise, even where inspiration presented in a marked manner the morbid conditions just mentioned. In five persons only did expiration appear laborious; twice it was attended with a

noise similar to that accompanying inspiration. It is to be observed further, that in cases where inspiration and expiration were both noted as laborious, the latter was much less so than the former." This difference in the facility with which expiration and inspiration are effected, a character of the disease long known and of great importance in its diagnosis, is however not a pathognomonic sign.

The introduction of the finger has in some instances on record led to the direct discovery of the diseased changes in the form of the prominences caused by the infiltrated structure on each side of the glottis. It is an important fact, as noticed by M. Valleix, that the error of mistaking any other condition for that in question has not been committed by persons employing this procedure.

In all general descriptions of œdema of the glottis great stress, as M. Valleix observes, is laid upon the condition of the face, upon its discoloration, &c. Yet of all the narratives of cases he has analysed, six only contain any distinct announcement of the state of that part. In two of these six cases, the face is said to have continued constantly pale; in one individual livid, in a fourth red, in a fifth anxious, and in the sixth the alæ of the nose were dilated, the face and lips livid and blue. In three cases observed by M. Valleix himself, the face was pale, the lips livid, the features drawn, while the eyes, widely open and hagard, were expressive of anxiety and affright.

Like almost all affections seated in the laynx, œdema of the glottis has a remarkable tendency to paroxysmal course,—this is presumed to depend upon occasional irritation increasing the amount of infiltration, an explanation which cannot be esteemed satisfactory.

Of forty persons attacked with the disease thirty-one died: three only died during a paroxysm; seven in an interval of tranquillity, and under a condition of things where recovery might have been hoped for. In all the other instances death occurred in a state of semi-asphyxia, more or less alarming. One individual perished while tracheotomy was being performed; two others, ten and fifty-two hours after its performance. One person died suddenly.

The disease may last a few hours only, or, even in fatal cases, be prolonged to the twenty-second or twenty-sixth day.

Authors usually insist on the importance of general bleeding in the treatment of the disease. Yet, in some cases only was bloodletting actually had recourse to; and from these no inferences as to its real value can be drawn. The same may be said of the application of leeches. From some cases recorded by the author, it would appear that no doubt can be entertained of the great utility of large blisters and tartarized antimony in emetic doses.

Various surgical means have been employed for the cure of the disease. M. Lisfranc has successfully scarified the prominent rim of tumefied membranes, thus letting out the serosity sufficiently to allow free entry of air. M. Marjolin and M. Legroux have both successfully torn the same parts; the former with a piece of root of marshmallows, the other with the nail cut irregularly into points. M. Valleix next institutes a searching inquiry into the claims of tracheotomy as a curative measure, and concludes that:

"This is without question one of the most valuable modes of treatment which can be employed in œdema of the glottis; recourse should not be had to it until the other means, of which the occasional efficaciousness is established, have been found useless, while, on the other hand, in order to succeed, we should not wait until the patient's strength is exhausted, and until evident signs of asphyxia give evidence of a state of stupefaction of all the organs, from the afflux of non-arterialized blood."

Here, in fact, as elsewhere, the proper moment for operating is to be determined in individual cases by a variety of circumstances.

The practical value of this essay justifies, we trust the reader will admit, the lengthened notice we have bestowed on it.

IV. *Essay on the influence of hereditary transmission in the production of nervous irritability, on the diseases which result therefrom, and the means of curing them*, by M. E. Gintrac. This paper scarcely admits of analysis. Far from being deficient in evidences of talent and power of observation on the part of its author, it is nevertheless principally distinguished by its tiresome verboseness and its numerous grave disquisitions, wherein points utterly problematical are assumed to be settled beyond the reach of cavil. The paper is one redolent of by-gone days, and a system of reasoning which is, fortunately for the solidity of knowledge, fast losing all influence on those who take any active part in advancing the science of medicine.

It may not be without some sort of interest to the reader to have placed before him a list of various affections depending upon "nervous super-excitement:" they are arranged as follows by M. Gintrac, in the presumed order of their tendency to acknowledge hereditary influence. In the first series appear suicidal monomania, mania, lypemania (melancholia), and various nervopathiæ. In a second, convulsions, epilepsy, hysteria, hypochondria, œnomania (a learned term for habitual drunkenness), palpitation, gastralgia, merycism (*id est* rumination, an anomaly of the digestive process which has been observed in a few instances in the human subject, and which M. Gintrac refers to the neuroses of the organ). In a third, chorea, somnambulism, homocidal monomania, theomania, demonomania, choremania, (M. Gintrac does not include under this head the enthusiasts of the Polka and Cellarius Valse, though heaven knows they would to all seeming have a fair claim to be marked as choremaniacs,) neuralgia, asthma, spasm of the larynx, spasm of the œsophagus. In a fourth series appear "acute ataxic states," nervous apoplexy, and trismus of new-born infants. Lastly, pyromania (the mania of setting *our neighbours'* houses on fire, for we do not know that these gentry have any special love for witnessing the conflagration of *their own* property), zoomania, (the mania exhibited by individuals occasionally, from the earliest antiquity, of fancying one's self transformed into an animal,) catalepsy, and tetanus scarcely supply any evidence of hereditary influence.

The following fact is worth being known: A young man, labouring under epilepsy, consulted M. Dumas. The physician, learning that a fit invariably occurred whenever the patient drank spirits, ordered him to take a certain quantity every twelfth day. Having in this way rendered the epilepsy periodical, he cured it with bark.

V. *On the nature and development of adventitious products*, by Dr. C. Baron. This is one of the most painfully tedious productions we have for a length of time been called upon to peruse. The professed object is to show that all adventitious products are nothing more than "transformed blood;" the details are never-ending, the hypotheses startling, the proofs deficient. M. Baron wisely thinks that because people may see cancerous matter and blood mixed together, and because in certain coagula there is an *appearance* of gradual transition from fibrinous into encephaloid substance, that all cancers are nothing more than blood transformed. Such misinterpreted facts might in former days have furnished the respected basis of a popular theory, but in these very material days M. Baron must "charm more wisely," if he desire to rank any of his brethren as disciples of his creed. It has long indeed been felt that John Hunter's notion on the origin of products in the blood does not stand the test of observation; but a complete refutation of it may be found in Dr. Walshe's work (which has just fallen into our hands,) 'On the Nature and Treatment of Cancer.' It may not be out of place to quote the following words from his volume (p. 53), which appear to us most satisfactorily (especially when taken in connexion with the context) to explain the appearances which have deceived M. Baron, and the numerous persons who, *before* him, have held similar views. "It is clear that what has been spoken of by writers as the 'conversion of blood into encephaloid matter,' is nothing more than an appearance produced by the evolution of primary blastema or of absorbed cell-elements in the interstices of a coagulum; the idea of an actual change of blood-corpuscles into cancer-cells is an absurdity."

We confess that the admission of this paper into the Memoirs of the Academy seems to us to bear witness to a fact, already rendered sufficiently clear to the ken of the sharp-sighted by numerous other signs, that the palmy days of French morbid anatomy have passed away.

VI. *On the acute delirium observed in lunatic asylums*, by Dr. Brierre de Boismont. This is a very excellent and well-digested paper; the conclusions to which it leads may be put in the following manner: 1. Acute delirium (phrenitis of the ancients) is neither meningitis nor encephalitis. The etiology of the affection, its morbid anatomy and diagnosis, place this beyond question. 2. It must be regarded as a purely nervous disorder, resembling delirium tremens and traumaticum. Doubtless some modification exists in the cerebral substance, but its nature is as unknown as that existing in the various species of delirium and in various other nervous diseases. 3. The anatomical changes discovered in a certain number of cases are merely accidental or complications of other diseases, or perhaps merely the traces of the alterations of movement. 4. The limits which separate acute delirium from acute mania, from meningitis, and from meningo-cephalitis are not always easily established; indeed it may be said that, under some circumstances, these diseases pass into each other by insensible degrees. These peculiarities and difficulties have betrayed many observers into error, and led them to affirm that acute delirium was identical with meningitis. 5. The causes which produce acute delirium are most closely related to those which engender mental alienation. The influence of moral causes in its generation is likewise very obvious.

6. Acute delirium differs beyond all doubt from mental alienation by its symptomatology, its mode of progress, and its duration; but its similarity to acute mania is in some cases so great, that the observer is tempted to consider it as that affection. 7. Removal from home appears to be advisable, both on account of the dangers to which patients are exposed in their own homes, and on account of the cures which have been effected in asylums. 8. The mode of treatment put in force should vary with the case and the individual. It would be a serious error if, deceived by the febrile state and excitement present, the physician were to employ antiphlogistic measures only. In some cases, where the patient was left entirely to the management of nature, the result has proved favorable.

VII. *Case of extra-uterine interstitial pregnancy*, by Dr. Payan. We agree with the observer and narrator of this case, that although so many as twenty-five narratives of similar description may have been brought together by compilers, still the interest attached to their investigation is by no means exhausted. For this reason we append the chief particulars of the present history, interesting as they are, too, in a medico-legal point of view.

An unmarried woman, aged thirty-two, having already had one child, and again pregnant, occupied as a sort of labourer, was seized one evening after a severe day's work with violent pains in the hypogastrium, accompanied with fainting sensations, thirst, great lassitude, &c. Leeches were applied above the pubis; but the woman expired at two o'clock the following morning. The circumstances of her decease were obviously of a kind to require a judicial investigation. The body was opened in the presence of some of the civil authorities. Measures were taken to expose the parts thoroughly and without rupture.

"The vagina having been laid open at the anterior surface, a few spots of reddish brown colour were seen near the external orifice. There was no sign present of these spots being of recent origin. No trace of blood existed on any part of the mucous membrane of the vagina. The os tincæ and neck of the uterus were sufficiently extensible to allow of the introduction of the little finger. The neck and anterior wall of the uterus having been opened by incision, we discovered in the internal surface of the former a few small patches of reddish-brown colour, without solution of continuity or laceration of any kind. The uterus (which was as large as at about the second or third month of pregnancy) was also cut into at the middle of its anterior surface; the cavity of the uterus was thus laid bare, which was spacious and proportional in size to the development of the uterus; but it did not contain the ovum. It was lined throughout its entire extent, with a concrescible material, of some thickness, a sort of mucous fur, velvety, grayish, forming an imperfectly organized pseudo-membrane, nearly filling the cavity completely. No trace of hemorrhage was at this stage of the proceedings to be seen in the cavity of the uterus. Above this, the proper cavity of the uterus, appeared another cavity, evidently, to my eyes, formed in the substance of the wall of the uterus, and situated towards the upper and superior part of the organ, in the neighbourhood of the uterine extremity of the Fallopian tube. I was unable to assure myself fully whether the latter terminated in this cavity, in consequence of an incision made in the wall of the organ, which rendered it impossible for me to trace the course of the duct."

The cavity of the uterus did not communicate with the cavity in the walls of the uterus containing the fœtus; this seems to us (with the author) unquestionable, though certain persons, apparently with the desire to raise

difficulties, maintained that real communication did exist; the uterine tissue forming its wall was so greatly distended, that it had become translucid. Here lay the fœtus uninjured, with the placenta and some almost transparent fluid. The fœtus was of the male sex, about three months old. Death had obviously been caused by accidental rupture of some point of the walls of this cavity, (which was about as large as a duck's egg); a very *large quantity of blood was found within the peritoneum surrounding the uterus.*

This plain explanation of the facts was not accepted without demurrer by some of the medical practitioners present. One of them even started the following mode of accounting for the whole train of circumstances and appearances. This person held that "some foreign body, a gum-elastic bougie, for instance, had been passed into the neck of the uterus during life, between the internal surface of the body of the organ and the membranes, as far as the fundus; that there a perforation had been produced by twisting about the instrument; that next the uterus, thus irritated and excited, had contracted and expelled the ovum through the perforation; and that what I considered to be the fœtal mass in a state of integrity, was nothing more than the ovum rendered adherent to the substance of the uterus by coagula." The criminal attempt which was thus conjectured to have been made to destroy at least the fœtus, obviously had not actually been made at all: the description we have given suffices to show this, and still more satisfactory evidence of the fact may be found in the original.

VIII. *Summary exposition of experiments made upon animals, with the view of ascertaining if the secretion of urine be suppressed in acute and peracute poisoning with arsenious acid.* By Professor O. De Lafond. The experiments of M. Orfila tended to show that the secretion in question does continue under the circumstances mentioned; those of M. Flaudin and Danger would lead to the opposite conclusion. M. de Lafond has attempted to settle the point at issue, and with the following results:

"1. The duration of life, after the administration of the poison in horses, was one hour, one hour and twenty minutes, eight hours, twenty-one, twenty-nine hours, and did not exceed fifty-one hours. In dogs, this duration was five hours, eight hours, and did not exceed twelve hours.

"2. The symptoms observed during life, and morbid changes discovered after death, demonstrated positively the fact of acute or peracute poisoning. In some animals, the inflammation of the mucous membrane of the intestine was so violent, as to have excited, in the course of an hour, the formation of several feet in length of canaliculated pseudo-membrane, having the form of the intestine.

"3. In no one of these animals was the urinary secretion suppressed.

"4. The quantity of urine drawn from the bladder with the catheter, expelled naturally, or found in the bladder after death, varied according as the symptoms of poisoning were acute or peracute. The quantity varied in horses between two centilitres and three litres; in dogs, between three centilitres and upwards of a decilitre.

"5. The mean quantity of urine secreted in an hour by animals in good health, compared with that secreted during the same period by those poisoned animals, was, in the case of horses, as 347 : 100; in the instance of dogs, as 6 : 1. Hence,

although the secretion is much diminished in quantity, it is not, as advanced by MM. Flandin and Danger, suppressed.

"6. The urine does not perceptibly contain the poison, until evident symptoms of poisoning show that arsenious acid has been absorbed and circulates with the blood. The time intervening between the administration of the poison and its detection in the urine was not less than one nor more than seven hours.

"7. The arsenical stains, received from the urine by Marsh's tube, on china saucers, were, generally speaking, very brilliant, and presented all the chemical reactions which characterize them in their state of purity."

The experiments, with an abundance of detail (wherefrom the above conclusions are drawn) are related in the original, and may be examined by the sceptical or curious.

IX. *Essay on penetrating wounds of the abdomen, complicated with escape of the omentum*, by Professor Hippolite Larrey. A young man, received a wound (quarter of an inch long) in the abdomen from a knife. In undressing to go to bed, he felt something protrude; a piece of omentum, of the width of two thumbs, had made its way out. No appearance of internal hemorrhage, of gangrene, or serious inflammation taking place, M. Larrey judged it advisable neither to attempt forcible reduction, nor to cut off the part, either by ligature or with the bistoury. The piece of omentum gradually contracted as cicatrization advanced; on the fifty-first day after the receipt of the injury it had totally disappeared, the spot it occupied being covered over by the adhering lips of the wound.

The narrative of the case is followed by a critical examination of the common surgical modes of proceeding in such cases; and the author, in concluding, sagely expresses his hope, that his inquiries upon this question "may help to show, that, in surgery, something else than *cutting, tying, slashing, and in a word destroying*, may be done, and that that something is to temporize opportunely, and, by the simpler resources of art, aid the powerful curative efforts of nature."

ART. VIII.

The Nature and Treatment of Cancer. By W. H. WALSHE, M.D., Professor of Pathological Anatomy in University College, Physician to University College Hospital, and to the Hospital for Consumption and Diseases of the Chest.—*London*, 1845. 8vo, pp. 590.

IN our last Number we gave a very brief, and promised a more complete, notice of this work. We now redeem the pledge, by laying before our readers a full analysis of its first two divisions, containing the general history and treatment of cancer: the concluding portion will occupy our attention on a future occasion. The fulfilment of this duty has been to us a most pleasing task. Unruffled in temper, and improved in mind, we have gone on from page to page, ever acquiring some new truth, or finding old ones more forcibly and clearly impressed on our understanding. And most firmly do we believe, that when our readers have perused this abstract, or, better still, have for themselves studied the original, they

will return the same verdict that we have already delivered, and acknowledge it to be indeed "one of the completest and most valuable monographs of an individual disease that exists in medical literature."

The introductory chapter is occupied with the consideration of the place which cancer holds in the nosological scale, and the signification of the term itself. We shall notice these two points as briefly as may be.

1. Nosological position of cancer. Diseases may be arranged in two great groups—the *functional* and the *organic*. It is to the latter only that our attention is to be directed. In all these there is a demonstrable change either of form or of composition and texture of an organ, separately or conjointly. Congenital malformations and acquired changes of configuration occupy the first of these groups. The second consists of those in which there is a change in the physical or chemical attributes of the natural elements of the frame, and of those in which there is the addition of some new material to the original or fundamental tissues—in other words, an adventitious product.

Explained more fully, this term, "adventitious product," is held to mean, "any substance which, produced by or originating in connexion with the organism, neither forms part of its natural structure, nor results from its healthy manifestations of activity, while, at the same time, it differs more or less completely, in many or in few qualities, from all the natural constituents of the frame." The materials of which this substance is composed may be either physiologically inorganic or organic. To the former belong saline deposits, calculi, &c., and some proximate principles, such as oils, sugar, and albumen, which are exuded ready-formed from the vessels, and are either incapable by nature of assuming structure, or are rendered so by the special circumstance of their origin. The latter includes all those which are possessed of organization,—in a word, formations,—and are divisible into two sub-classes, blastemal and germ-formations, or parasites.

Blastemal formations depend for their existence on the immediate and direct access of nutritious matter from the blood of the parent organism, and are essentially characterized by cell-structure, their nature being determined by the attributes and end of the component cells. Thus, the cells may be incapable of maintaining their own existence, or of generating new cells previously to their destruction. They are consequently acted upon by the surrounding materials, dissolved in the fluids, or disintegrated and crumbled down. Cells of this kind may be called *evanescent*, and the formations they constitute *deposits*. Pus, tubercle, and colouring matters are examples of this kind.

Again, the primary cells may be able, before their destruction, themselves to generate, or indirectly to cause the generation of, new cells, which in their turn perform the same process. They may be called *non-permanent* and *indefinitely vegetating*, and their formations *growths*.

Lastly, the primary cells, though unable to generate others, may possess an inherent faculty of forming tissue, more or less closely resembling the natural structure and of continuing in this condition. They may be termed *persistent, non-vegetating cells*, and their formations *pseudo-tissues*.

We must speak now of the order of *growths*. The materials composing

them are generally accumulated in some point or other of the body, where they gradually increase in quantity, pushing aside the structures among which they are developed. But there are some which besides this have a tendency to spread among the elementary molecules of those tissues, to cause atrophous destruction of existing particles, and prevent the evolution of similar new ones. This process is known by the name of *infiltration*, and the genus of growths which exemplifies it is *cancer*. In the following pages the truth of this position is attempted to be established.

With regard to the *signification of the term*, CANCER or CARCINOMA, we shall merely remark, that Dr. Walshe (while recognising three distinct species, encephaloid, scirrhus, and colloid, with several varieties) applies it generically to every formation of this class, whatever be its external appearance and in whatever condition of development or destruction it may be. In this, we think, he has done wisely. To multiply names is but to introduce confusion. The reader will find a very useful table at p. 10, exhibiting a synopsis of the species and varieties of cancer, with a list of synonyms; from this he may learn that names are even more abundant than real varieties of the disease. Having thus prepared the way for his inquiries, our author commences his task by the consideration of the *anatomy of cancer*.

ENCEPHALOID. This substance, so named from its resemblance to the brain, presents, on section, the appearance of an almost homogenous matter, of an opaque milky colour, variously dotted with pinkish spots. Its consistence is about that of the healthy adult brain, and, when torn through the surface, has a coarsely granular aspect. It consists of two materials, a stromal, or containing, and an intra-stromal, or contained. The stromal substance, generally denser than the other, divides the mass into loculi, lobules, and lobes. Its different divisions do not intersect, but describe curves, circumscribing spaces of a somewhat spherical form. It is occasionally of fibrous consistence, but most usually of a delicate cellular texture. But in almost all cases some parts of a tumour of this kind are harder and some softer than that above described, and this diversity is caused by the mode of arrangement of the cancerous structure; the hardest part being radiated, or finely granular, that of less consistence largely granular or lobulated, while the softest is collected into large lobes of almost diffluent pulp.

The pink tint is produced by the presence of numerous blood-vessels, varying in diameter from that of a hair to a line and upwards. The vessels are not confined to the stromal part; they penetrate the contained substance also. In some successful injections the whole mass has appeared to be formed of a vascular plexus.

Encephaloid is met with, either as a distinct tumour, or infiltrated among the elementary molecules of the tissue. Dr. Walshe doubts its being ever truly encysted, though it is occasionally surrounded by a pseudo-cyst of condensed cellular tissue. The tumours usually incline to a spheroidal shape, but this admits of many exceptions. The infiltrated, which is the less common form, is often seen in the liver, and is observed in all instances where the disease extends by continuity from one tissue to another. Encephaloid cancers acquire a larger size than almost any

other morbid formation. On the other hand, the extreme of smallness is sometimes seen.

Varieties. There are several varieties of encephaloid, which are distinguished by the following names: *a. Mastoid,* the section of which presents the aspect of the boiled udder of the cow. *b. Solanoid,* which much resembles a sliced raw potato. It is of not uncommon occurrence in the liver. *c. Milt-like tumour:* this is rare. *d. Nephroid,* so-called from its resemblance on section to the kidney. In a post-peritoneal cancerous tumour, presenting all the characters of this form, as described by Recamier, microscopical examination showed the mass to be composed of granules, cells, and peculiar fibres, precisely as in Müller's *fasciculate* carcinoma, a proof that Dr. Walshe was correct in his views of the close alliance of these two varieties,—the nephroid and fasciculate. *e. Hematoid,* when the vessels are unusually abundant, the brain-like character being still retained. *f. Fungus hematodes.* This term should only be applied to tumours in which interstitial hemorrhage has produced sanguineous infiltration, or irregular accumulations of blood through its substance; and when, the integuments being ulcerated, a rapid development of fungating growths takes place from the exposed surface. As a name for the entire species it is most objectionable.

Scirrhus. This common form of cancer consists also of a stromal and an intra-stromal substance; but in many cases it requires close inspection, in different lights, to distinguish between them. The white intersecting bands, so often spoken of, are an accidental condition produced by thickening and cancerous infiltration of some element of the affected organ, such as lymphatics, or, in the mamma, lactiferous tubes. The stroma has a tendency to an irregularly rectilinear arrangement, whence the loculi incline to the rectangular shape. They are of small size. Dr. Walshe is confident that those writers who have described the loculi as large and spheroidal have mistaken fibrous tumours for true scirrhi.

The proportion between the stromal and intra-stromal matter varies according to the period of development of the tumour. In a very early stage the former predominates, and the mass resembles fibro-cartilage—so closely indeed that M. Cruveilhier believes the appearance of the cancerous juice on pressure is the only diagnostic mark. According to our author, the two may be distinguished by the arrangement of the stroma—rectilinear in scirrhus,—curvilinear in fibrous tumours.

The section of a scirrhus has a peculiar semi-transparent glossiness; it is of a bluish-white colour when the mass is most firm—of a lilac-yellow, pale dirty fawn, or grayish, when there is much intra-stromal matter.

One of the chief characteristics of this form is its extreme hardness. This is most decided while the tumour is still *in sitû;* after removal it often becomes soft and somewhat flaccid from the absence of blood and the *turgor vitalis.* The fluid which may be expressed from very solid masses is thin and watery; that from the less firm is thicker and more opaque.

Scirrhus is not strikingly vascular. It has even been denied that it possesses any vessels; but this is a mistake. The supply, however, is irregular and unequal. It is deposited either as a tumour or infiltrated.

The latter form is not uncommon in the brain, and is its most usual state of existence in the uterus and the bones. Dr. Walshe has never seen a scirrhus tumour invested by a true cyst. The morbid growth rarely acquires a large size.

Anatomical varieties. 1. *The pancreatic sarcoma* of Abernethy is regarded by Dr. Carswell and some other pathologists as a variety of scirrhus. Our author has great doubts regarding the true nature of the tumour referred to, and believes the term would be more correctly applied to some form of encephaloid, or to the pultaceous alveolar carcinoma of Cruveilhier. 2. Dense scirrhi, of an unusually shining aspect, and bluish-white colour on section, are distinguished by Recamier by the name *chondroid.* 3. The term *lardaceous tissue,* much employed by French pathologists, is applicable to the infiltrated form of scirrhus, where a section of the diseased part resembles the boiled rind of bacon. 4. In those rarer scirrhi, which present on section an evident appearance of bands, the surface occasionally resembles a cut turnip, and such are denominated *napiform.* 5. The adjective *apinoid* is applied to those which, when divided, resemble the section of an unripe pear; the similitude being produced by the dissemination of comparatively opaque, almost buff-coloured, spots through a more translucent ground of a very pale yellowish-lilac tint. As development proceeds, the opaque matter gradually increases and alters the appearance of the surface. It is the most ordinary condition of mammary scirrhi. Müller has described it as a distinct species, under the name of *carcinoma reticulare.* 6. Lastly, in some rare cases scirrhi are found in the *hematoid* condition, and these present the same peculiarities as that variety of encephaloid.

Colloid. This form of cancer presents on section the following appearance:—The surface is divided into a vast number of distinct loculi, of an oval or rounded shape, varying in size from that of a grain of sand to the largest pea. The septa are fibrous and of a pretty uniform thickness. The loculi sometimes form shut sacs, sometimes communicate with each other. They are filled with a semi-transparent, tenacious, and clammy matter, which has the physical properties of soft jelly, and gives a greenish yellow colour to the section. The consistence of the tumour is usually about that of firm cheese, but it may be much harder. It is a remarkable character of this structure that wherever and in whatever form developed, the appearance of properties of the gelatiniform substance are exceedingly uniform in every part of its extent, so long as destruction has not set in. Little is known regarding its vascular condition. Colloid is found as a distinct tumour, and infiltrated. The latter is most common in the stomach, the organ retaining its natural shape. It is also met with in the intestines and omentum. Disseminated nodules never occur in the parenchymatous organs. Colloid growths acquire considerable bulk. In the infiltrated form it sometimes extends over more than two thirds of the anterior and posterior surfaces of the stomach.

It can scarcely be said that there are any true varieties of colloid. The contained matter is somtimes pearl-like, from the presence of a peculiar kind of cholesteric fat, called by Müller *cholesteatoma.* M. Cruveilhier describes specimens in which the matter was opaque, of yellowish hue, and tallow-like aspect, with granular fracture and feel, and the chemical con-

stitution of casein: this constitutes his *areolar pultaceous cancer;* but Dr. Walshe considers that it is more closely allied to encephaloid. Colloid is sometimes developed in a basis of scirrhus. In cases of this kind the appearance presented has been mistaken for softening of the scirrhus mass.

CHEMISTRY OF CANCER. But little yet is known upon this point, though the inquiries of different observers, and especially of Müller, have assigned cancer a place among the protein formations. It belongs essentially to the albuminous variety, though both fibrin and casein enter slightly into its composition. There is some difference of opinion as to the presence of gelatine; but the weight of evidence leans to the negative. Its occasional discovery has been very plausibly attributed to the intermixture of ordinary cellular tissue. Scirrhus is distinguished from encephaloid by containing a less quantity of albumen, by being wholly deficient in "osmazome," and by having nearly twice as large an amount of inorganic salts, the proportion being 37·25 per cent. in scirrhus, 19·10 per cent. in encephaloid. Müller discovered casein in his carcinoma reticulare, and considers it a very uniform constituent of mammary carcinoma. He conceives that its presence does not depend upon the contents of the lactiferous tubes, because it also exists in the morbid product in other organs. Spirits of wine, which renders scirrhus and encephaloid opaque, has no effect on the transparency of the jelly-like matter of colloid. Müller could not find any trace of gelatine in it; nor does it contain casein, excepting in the so-called pultaceous variety. There is some osmazome; but the chief constituent is albumen. There is also present a small quantity of a matter analogous in many respects to the salivary principle (ptyalin).

The ultimate microscopical cells of cancer are insoluble in cold and boiling water, and are not seriously affected by acetic acid—characters distinguishing them from the red and white corpuscles of the blood, and from pus corpuscles. Crawford found the *ichor and discharge* alkaline, and ascertained the presence of free ammonia; he also detected sulphuretted hydrogen. Ploucquet states that the matter sometimes effervesces with alkalies, sometimes with acids. The question seems yet undecided.

PHYSIOLOGY OF CANCER. This is, perhaps, the most interesting portion of our inquiries. The Physiology of Cancer includes the consideration of its Origin, Growth, Decay, Elimination, Cicatrization, and Local Regeneration.

I. *Origin*. Our space will not permit us to follow Dr. Walshe in his review of the different theories—some most absurd, others more specious and truth-approaching, which have, from time to time, been promulgated regarding this mysterious subject; though it is most ably executed, and will well repay an attentive perusal. We must confine ourselves to an exposition of the author's own peculiar creed.

There are three points really distinct in nature, which have invariably been more or less confounded by investigators of this complex question. These are—A. The nature and properties of the formative material in which the new mass originates; B. The seat or locality in which it originates and germinates; C. The process by which this formative material is itself generated.

A. *Nature of the formative material.* Cancer, as we have seen, originates in a fluid denominated blastema. Its characters when first produced are unknown; as existing in the substance of growths already evolved, and forming the matrix for additions, it in nowise differs from the blastema of pus, or of the natural tissues—being fluid, slightly viscid, semi-transparent, homogeneous, and totally devoid of solid particles. Its chemistry has not been ascertained. In all probability it is essentially composed of liquor sanguinis modified in its vital qualities. What the precise nature of this modification is we know not. How and where the change is effected is also doubtful; probably the peculiar attributes are mainly impressed upon it, while still mixing with the circulating fluid, though in some degree they are acquired during the process of filtration through the vascular parietes. Whether extravasated blood, unaltered in its apparent physical characters, is capable of acting as a blastema for cancer, is yet undetermined.

B. *Seat of germination.* There are three possible situations in which cancer-blastema, being derived from the blood, may be evolved, viz., in the interior of the vessels, in the substance of the vascular walls, or outside the vessels. In very few cases is there any evidence in favour of the first two; the last is by far the most common locality; and in the vast majority of instances, the site of the deposit is the inter-vascular interstices of the various tissues and organs. Where extra-vascular tissues are destroyed by the advance of adjacent cancerous disease, Dr. Walshe believes the process is effected by the imbibition of the blastema thrown out by the vessels of juxta-posed vascular tissue; the fluid which has thus penetrated, subsequently passing through the same series of changes as if infiltrating a vascularized tissue.

C. *Nature of the process by which the formative material is generated.* The account given above sets at rest the *vexata questio* of the nutritive or secretive origin of cancer; for the exudation process, by which cancerous blastema, as all other unhealthy materials, is thrown out of the vessels, is neither one of secretion nor of nutrition, but may be a substitute for both.

II. *Growth.* The ultimate solid constituents of carcinoma are granules; nucleated cells of spherical, elliptical, irregular, or caudate shape; free nuclei, and fibrillar substance: with these is associated a variable quantity of the semi-transparent blastema. Capillary vessels are always discoverable; and fat globules too are constantly present. Compound granule corpuscles, melanic matter in the form of cells and free granules, saline matter, and certain other substances, occur as accidental and contingent elements. For a full description of these, we must refer to the original; only observing, with regard to the vessels, that they are of two classes, some being found by the looping or extension process, from the vessels of the surrounding tissues, and others originating *de novo* in the morbid substance; these contained blood, exhibiting an oscillatory movement, independent of the general circulation. This last fact is of importance as showing, what has been hitherto entirely overlooked, that out of the same fluid blastema, and at one and the same time, are produced cancer-cells, and cells destined to form structure closely similar in all its properties, to one of the natural tissues of the body, viz. blood-vessels.

How, then, are the cells formed? Two methods of development are described. In one, the cell is a molecular vesicular body from the first, and produces within it a secondary body, the nucleus, which is the embryo of a future cell: in the other, granular matter is first precipitated from the blastema, and this constitutes a nucleus, around which the cell forms. Dr. Walshe believes that both may be seen in the cancer. Caudate cells are produced by elongation of opposite points of the circumference of the spherical cells. They eventually pass into the state of filaments, and become the elements of fibrous structure. Our author believes that the diagnostic importance of these cells has been greatly exaggerated, and that some at least originate from non-cancerous spherical cells.

The tumour being thus formed at first, its *enlargement* is effected in the following manner:—It is supplied with nutritive materials by vessels precisely in the same way as the natural tissues, and from the matter thus afforded, there is a continuous cell-generation, the mode of which differs in different cases. Thus, in some, the primary cells contain within them the nuclei of a second generation of similar bodies, which, in their turn, possess the like procreative faculty: and the process may be carried on *ad infinitum*, so long as there is a supply of blastema. Of this, the *endogenous mode*, we have an example in colloid. In other cases, as in Scirrhus, and most commonly in Encephaloid, the germs of secondary cells are not formed within the primary ones, each succeeding generation being formed external to preceding ones. This is the *non-endogenous mode*. It is probable that the locular arrangement, and the general shape of the whole mass, are caused, in some measure at least, by this manner of increasing.

III. *Decay*. A tendency to destruction is inherent in all formations of this genus, but the way in which it is produced has been differently explained. According to some, it results from gangrene, produced by obstruction of the venous trunks; while others regard inflammation as the exciting cause. Dr. Walshe believes that both views are occasionally correct, but that in some instances, the softening process can only be explained by considering the change, similar in nature to that which has been shown by Mr. Gulliver, to occur in fibrine stagnating in the vessels. Colloid is in a different position; its destruction is produced, either by a gradual wearing out of the stromal matter, and subsequent escape of the contained jelly, or by the rupture of the containing parts from extremely active growth of the sub-cells.

We must pass by the three remaining sections of this chapter; nor can we do more than direct attention to the succeeding one, which describes the local changes to which cancerous tumours are liable, and the effects they produce on surrounding parts. But the next chapter, on the pathology of cancer, is one of such importance, as to demand a more full investigation.

General Pathology of Cancer. It has been long known, that some parts of the body, as the uterus, female mamma, stomach, liver, &c. are more prone than others to cancerous disease, but we have as yet scarcely any numerical estimates. The reader will find two documents of this kind in the work before us (pp. 92-3). The causes of this predisposition have been the subject of much speculation; but the question

remains totally undecided. It is quite evident that age and sex exert a great influence on the localization of the disease; and it is a curious fact, that particular parts of organs are more liable to be attacked than others, as, the upper third of the œsophagus, the pyloric end of the stomach, the middle lobes of the brain, &c.; and that, when one only of double organs is affected, it is most usually the right.

Primary cancer has been found in all organs and tissues of the body, excepting tendon, ligament, the intervertebral tissue, probably articular cartilage, and the spleen. Still, more extended observation may prove, that even these can afford a primary nidus to the disease. Dr. Walshe has not met with any cases of *secondary* development (excluding those in which it occurs, from extension of the disease by continuity of tissue) in the eye, ear, or penis; and it is rare in the testis, mamma, uterus, tongue, lips, ovaries, prostate gland, and the entire tract of the alimentary canal. The parenchymatous viscera, the lymphatic and venous systems, the bones and the skin are its chosen seats; and it should be particularly noticed that it is in the peripheric portions that the secondary deposit takes place, the disease, when primary, choosing a deeper-seated locality; and that, in respect of double organs, both are almost invariably implicated at one and the same time, which rarely, if ever, occurs under other circumstances.

The species of the disease also influences its localization. *Encephaloid* has the widest range, and the seat of scirrhus is nearly as extensive, while it is doubtful if colloid has been really found in other situations than the stomach and adjacent glands, the small and large intestine, the omentum, female mamma, and spinal marrow. The different species affect, likewise, a special preference for certain localities. Encephaloid is most common in the testis, lungs, kidneys, spleen, and meninges; scirrhus in the uterus, female breast, stomach, lower lip, and skin; scirrho-encephaloid is *per eminentiam* the hepatic cancer; while colloid infests the alimentary canal and omentum. The reason of all this is utterly unknown. In many cases cancer exists in several parts of the body at the same time; but the observations of Dr. Walshe have led him to believe that this occurrence is less invariable than is commonly imagined. Where it does take place, the dissemination may have been produced by a successive, or by a simultaneous process of development. We need only notice the first of these, for the second requires no explanation, and is probably more common than is generally thought.

Successive dissemination. Here the disease originates in a single organ or tissue, whence it spreads to a multitude of parts, which are then said to be affected with *secondary* or *consecutive* cancer. When the parts thus attacked are circumjacent, the process is affected by what is called infiltration, and this may take place through tissues naturally continuous, or artificially rendered so by adhesions, but it has been also observed where the parts were merely in juxtaposition or close proximity. The infection of distant organs must be explained in another way. When direct lymphatic communication exists, it is through these vessels that the contamination is transmitted; when there is no such communication, it is more than probable that the veins are the conducting channels, as in cases of secondary purulent deposits. This doctrine has been opposed on various

grounds, but we believe it is essentially true, and conceive that our author, to whose pages we again beg to direct the reader's special attention, has most successfully refuted every objection. Of the three species of cancer, encephaloid is perhaps the one which has the greatest tendency to affect other organs, though its pre-eminence in this respect is disputed by scirrhus. Colloid certainly holds the lowest rank. Secondary carcinoma is, in the majority of cases, of the encephaloid species.

State of the solids and fluids generally. There is no particular condition of the *solids* sufficiently constant to deserve the name of a "secondary lesion." But in protracted cases of the disease the mass of the solids diminishes. Louis has ascertained that the mean size of the heart is less than in persons who have died of any other disorder, and that the aorta at its commencement has also a smaller diameter. More is known regarding the fluids. The quantity of blood gradually decreases, and it acquires a peculiar clamminess or stickiness. Andral has detected pus-corpuscles, and in one case elliptical, finely-granular lamellæ, much larger than pus-corpuscles, and with a far more regular outline than mere patches of albumen. As the disease advances the proportion of fibrin increases, while that of the red corpuscles diminishes. These changes produce a tendency to passive serous effusions. Andral has detected pus-corpuscles in the chyle of the thoracic duct. The urine does not undergo any specific alterations.

Symptomatology. It remains yet to be ascertained whether there exist any *prodromata*, and if so, what is their nature. When cancer is once formed, the *local symptoms* are tumour of variable size and form (this exists in all cases, even when the morbid matter is infiltrated): alteration of the shape of organs and parts; pain, but this is exceedingly uncertain, being often trifling, and sometimes altogether absent; hemorrhage, in some cases the first appreciable sign of the disease; and derangements of functions, which of course vary according to the organ affected. In a few exceptional cases the malady runs its course without the appearance of any *general symptoms;* but most usually the constitution suffers in a marked and peculiar manner. There is fever of low type; gradually failing appetite; distressing thirst; often nausea and vomiting; irregular action of the bowels; progressive emaciation; a most peculiar straw-coloured and waxy appearance of the skin and mucous membranes, with semi-transparent puffiness; extreme debility; and insomnia. The bones are often affected, and become remarkably fragile. The intellectual faculties lose their clearness towards the close of the disease; the ideas are sad and gloomy; and the temper more or less morose. Such are the more prominent characteristics of the *cancerous cachexia;* but they do not appear with perfect uniformity. Their causes must probably be sought for in some morbid condition of the blood, though different from that which produces the cancerous diathesis. Were it the same, both phenomena would invariably coexist, which is not the case.

Course. Cancer is essentially a chronic disease, but occasionally it proves fatal in a very short period. Dr. Walshe relates at p. 131 a very interesting case in which death occurred in two months from the outset. The progress is usually gradual and uniform, but it is sometimes inter-

mittent, giving rise to false hopes. In some instances the disease remains latent throughout its entire course.

Terminations. Death is the most common end; but now and then a more favorable result is brought about, the disease being arrested for an indefinite period, or cured. Of cure as the effect of treatment we shall speak elsewhere; in this place it is merely necessary to observe that it sometimes takes place spontaneously—the tumour undergoing resolution, or being destroyed by suppuration, or mortification. In a few very rare cases there has been a perfect and permanent cicatrization of the ulcerated parts.

Duration. We have few data as yet to determine this point. Our author has collected 56 cases, MM. Herrick and Popp 50, and M. Leroy d'Etiolles 1172. These three series give a mean duration of 39·43 months. Cases interfered with by surgical operation are not included in the above. Of course the commencement of the disease is dated from the first appearance of symptoms, and the results are, therefore, merely approximative. Of the three species encephaloid runs the shortest course. Sex does not appear to have much influence. Its progress is slower in old than in young persons.

Frequency and mortality. From the Registration Reports during the last 5 years, it appears that a mean number of 2644·66 persons die annually of cancer in England and Wales, or as closely as possible 176·83 of every million living; and that 8045·83 per million of all deaths are due to this disease. Mr. Wylde gives 1 in 305·23 of the deaths from all causes as the proportion in Ireland during ten years. The tables furnished by our author seem also to show an increase in the number of deaths during the last five years, but he is inclined to attribute this appearance to the greater accuracy of the registers, and the more perfect diagnosis of the disease. It does not appear that *season* exerts any influence on the mortality.

Etiology. Specific causes. There is no proof that the disease has ever originated from infection or contagion. It cannot be produced by inoculation, and the repetition of Langenbeck's experiment of injecting cancerous matter into the veins has given only negative results. *Predisposing causes.* It is the generally received, and probably correct, opinion that the disease is transmitted from parents to their offspring, but we have no positive proof of the fact. Our author gives some valuable tables illustrative of the influence of age, from which it appears that the mortality in both sexes steadily increases with each succeeding decade until the eightieth year; thus showing the inaccuracy of the common opinion that the tendency to cancer reaches its maximum between the 35th and 50th years. This idea has probably arisen from the sudden and enormous increase in the deaths of females in the last-named period (see a most instructive diagram, p. 50)—a result perhaps correctly attributed to the declining activity and cessation of the genital functions. As corroborating this view, Dr. Walshe quotes a curious document, showing that in Sweden and Finland the intensity of fecundity, which had been pretty uniform from æt. 25 to 40, suddenly falls to $\frac{1}{3}$th of its previous amount from æt. 40 to 50. Encephaloid is the most common species in early life; but its pre-eminence in this respect has been much exaggerated. Of the influence

of *sex* there can be no doubt. The mortality among females is about $2\frac{3}{4}$ths greater than among males. Our author believes that persons of the *sanguineous temperament* are most prone to the disease. It is impossible to decide with accuracy upon the effect of marriage or celibacy on the generation of the disease. Previous maladies, depraved habits, and too sustained intellectual labour, have all been assigned as causes, but without sufficient proof. There are, however, some grounds for believing that mental affliction has in many cases had some effect in inducing the malady. The influence of different occupations and stations in life is also yet to be determined; but Dr. Walshe has clearly shown that it is a mistake to imagine that a country life confers any immunity (p. 163). Cancer is most common in Europe; it is much rarer in Asia and tropical America, and is specially infrequent in Africa. Has, then, civilization any effect in producing the disease? Data are wanting for a satisfactory answer; but the bias of evidence is towards the affirmative. Mechanical injury is often one *exciting cause* of the disease; but the relative frequency of its operation is unknown. In some cases a state of irritation precedes the appearance of the disease; but it must ever be remembered that without a constitutional predisposition all these agents are inoperative; for, as our author elsewhere most truly remarks, "a cancerous tumour under all circumstances, even should it remain single and stationary for years, is but the local evidence of a general vitiation of the system."

AFFINITIES AND NATURE OF CANCER. In our last number we quoted in full Dr. Walshe's most philosophical views regarding the latter of these points; and in reference to the former we have only space to observe that the reader will find in the original a clear exposition of the characters which separate cancerous productions from all other growths; with some of which they have been often confounded. And this brings us to what is, after all, the most important branch of the whole subject—the treatment.

TREATMENT OF CANCER. Little can be said regarding *prophylactic* treatment, for the simple reason that we know scarcely anything of the prodromata of the disease. But it is quite evident that individuals who belong to a family, any members of which have become the subjects of cancer, should adopt all those medical and hygienic rules which experience has shown to be most efficacious in invigorating a debilitated constitution.

Curative treatment. Before referring in detail to the various expedients which have been adopted for the cure of cancer, we may premise once for all that almost every remedy, every plan has been extravagantly lauded, or unduly depreciated. Of these extreme opinions we shall take no notice; our object being to lay before the reader a succinct account of the conclusions to which our author has arrived, after most careful investigation and much experience; and which, moreover, we think exhibit the evidences of sound judgment and impartial discrimination.

Drugs. Under the long-sustained use of *conium* the progress of the disease has, in some cases, been permanently arrested; but it is difficult to persuade patients to persevere in its employment. In Dr. Walshe's hands it has failed to do more than alleviate pain and irritability. *Belladonna, aconite, henbane, prussic acid,* and other narcotics are less worthy

of trial, excepting as palliatives. *Sedum acre* merely quiets irritability, and often disagrees. Benefit may sometimes be derived from the local and internal use of *antacids,* especially *ammonia.* In many cases, local relief and decided general improvement of health have resulted from the employment of various preparations of *iron.* The *iodide* is perhaps the salt which offers the greatest promise of advantage. *Gold* has been much used in France, and occasionally with good effect. *Copper* and the *chloride of barium* seem unworthy of trial. *Mercury,* given so as to affect the gums, is a decidedly hazardous remedy; in minute alterative doses it is more safe, and has been well spoken of. *Iodine* is a much more eligible remedy, of the alterative class, and when combined with arsenic, in the form of the iodide of that metal, is perhaps the remedy which, in conjunction with external treatment, especially compression, offers the most chance of success. When given in doses of $\frac{1}{15}$ to $\frac{1}{12}$ grains twice daily, two hours after eating, it is well borne, and may be continued without risk for several months. Its effects are—unusual perspirations, with dryness of the fauces and alimentary canal; sometimes, but rarely, slight headache; decrease of pain; arrest of growth in the tumour, if not actual diminution of bulk; and general improvement of the health. *Animal charcoal, the flesh of the gray lizard,* and *cod and skate liver oils,* have been recommended. The latter are worthy of trial.

External remedies. Leeches are only useful in the very earliest stages (chiefly to help in the diagnosis), and to remove accidental inflammation in the adjacent textures. A continued reiteration of the process is a most mischievous and senseless proceeding. When a tumour is adherent to the skin, the practice is quite inadmissible. Ligature of the nutrient arteries has been occasionally advantageous. Blistering is a hazardous expedient; and the evidence in favour of electricity and galvanism is quite defective. Dr. Walshe has no experience of the effects of mercurial inunction and plasters; but he speaks favorably of the iodide of lead ointment, and prefers it to all other preparations of iodine. M. Tanchou strongly recommends the application of many of the substances above noticed in the dry form in *sachets.* When too strong in themselves, he mixes them with some inert powder. The only advantage of this plan is the change in the form of our applications. Of *compression,* the best external method of treatment, and one, moreover, which does not interfere with any other outward application, we have already spoken in our last Number, and to it we need not recur.

Subcutaneous incisions have been performed by M. Tanchou, with a view of preventing extension of the disease and promoting absorption. The only possible effect of such a proceeding is to make matters infinitely worse than they were before. Various applications have been used for the purpose of altering the character of exposed cancerous surfaces. M. Senebier's experiments show that *gastric juice* is capable of producing considerable temporary amendment. In one case under its employment an ulcer, which originally measured three inches in diameter, was reduced to the size of a sixpence. He recommends the gastric juice of the ox, as being always readily procured. *Carbonic acid* is another agent of this kind, but of inferior efficacy. *Lead* has been highly vaunted; but its powers appear to be merely sedative and anodyne. As exercising this

action in a marked manner, Bayle recommends a combination of six drachms of litharge of gold, six drachms of vinegar, and two ounces of olive oil, to be spread in a thin layer on the ulcerated surface. *Tincture of iodine* improves the aspect of the sores and the character of the discharge, but can do no more. *Petroleum, tar-ointment,* and *creosote* are of less value. *Turpentine* was at one time much employed, but has deservedly fallen into disuse; and the *terchloride of carbon* has been found to have no active influence of any kind.*

The employment of *escharotics* in the cure of cancer is of very ancient date. The actual cautery was the first used, but is now rarely had recourse to; and of all the potential caustics, *arsenious acid* and the *chloride of zinc* are those which most deserve attention. Not a few cases are on record in which the disease has been effectually removed by the former; but its application is not unattended with danger, especially when it is requisite to repeat the process. For this reason the latter should, we think, be made choice of; and the cases in which it can be advantageously employed are, superficial cancerous ulcers of the skin, carcinoma appearing on the cicatrix after operation, and when some suspicious substance has been accidentally left behind.

Palliative treatment. The objects sought by palliative treatment are, modification of the course of the disease locally and generally, and relief of symptoms. Cold applications and evaporating lotions are indicated when the affected part is hot, full, and tense. Hemlock poultices, bruised carrots, the aqueous solution of opium, fomentations of stramonium leaves, and other sedatives may be used to allay pain. Vegetable charcoal and the chlorides of lime and soda will remove fœtor. Decoction of cinchona with tincture of myrrh is serviceable when the surface is flabby and disposed to bleed. The temperature of these dressings may in a great measure be regulated by the feelings of the patient; but it should be remembered that *warm* applications are really detrimental. If severe hemorrhage set in, and do not yield to the internal use of acetate of lead, gallic acid, or ergot of rye, combined with the application of cold or the oil of turpentine externally, more active measures must be resorted to. When depending on capillary effusion, *pressure* is the most likely means of stopping the flow of blood; when from the perforation of an artery the vessel should be tied outside the morbid growth. And while these local measures are enforced, the patient's health and strength should be kept up to as high a standard as possible, by the use of general means. Pain must be soothed and sleep procured, for nothing wears the sufferer out so much as want of rest; and for this purpose opium or morphia is the best means; but the other narcotics may be employed as a change. The *diet* should be simple and nourishing, but unstimulating, except under particular circumstances. The starving system has been recommended and rigidly pursued; it is, however, a cruel and most useless torture—the body will waste, while the growth is luxuriantly flourishing. The *hygienic condition* of the patient must be carefully attended to. Regular moderate exercise, cheerfulness of mind, avoidance of intellectual labour, and the cares of business, should as far as possible be secured. In some cases,

* We have employed this substance both internally and externally in many cases, with the view of alleviating neuralgic pains, but have invariably failed in producing any effect. (REV.)

change of air and scene may be beneficial; and in the early stages of some cancers, especially of the breast and uterus, a course of mineral waters, taken at their source, as the Kraenchen-brunnen at Ems, and afterwards the Paulinen at Schwalbach, would be advisable.

Treatment by ablation. The removal of the morbid mass may be accomplished by cutting instruments, ligature, potential caustics, or congelation, or by inducing sphacelus. The first method is the one that has been most generally adopted, and concerning which it is of the first importance that we arrive at some definite conclusion. Our author accordingly devotes considerable space to its investigation, and we can state that nowhere will the student or practitioner find so able an exposition, or so critical and judicious an examination of this much agitated question as in the pages now before us. To accompany him throughout his whole chain of reasoning would occupy more space than we can spare; but we beg the reader's special attention to the following series of propositions, which, as it appears to us, he has logically deduced from his premises. They refer, be it observed, to the operation as a *curative measure*. We give these propositions in the author's own words; but it is due to him to state, that we do not here give *all* his words. To almost all the propositions are appended explanations, illustrations, and qualifications, which give additional force to the conviction their simple enunciation must produce.

"1. The removal of a cancerous tumour *may* cure the disease.

"2. No combination of circumstances ensures the patient, who submits to the excision of a cancerous growth, from return of the disease.

"3. Scarcely any combination of circumstances renders recovery from the operation, and prolongation of life, a matter of absolute impossibility.

"4. The existence of cancerous contamination of the lymphatic glands renders failure of the operation as nearly as possible a matter of absolute certainty.

"5. Where any portion of the morbid growth is accidentally left behind, the disease runs a materially more rapid course than if it had not been interfered with surgically.

"6. Should the cancerous diathesis exist, (no matter whether its manifestations have become actually apparent or not,) removal of a tumour inevitably determines its outbreak.

"7. When cancers in a state of active growth are cut out, the invariable result is reproduction and increased energy of progress of the morbid mass.

"8. Cancers, the exciting cause of which has been some local injury, are probably less prone to return after operation than those which have sprung up spontaneously.

"9. The success of excision in individuals belonging to a family of cancerous taint is even below the average.

"10. Reproduction of the disease becomes almost inevitable in cases where the skin or textures intervening between the tumour and integuments participate in the cancerous affection, or where these structures are, though not cancerously, still morbidly affected in consequence of the juxtaposition of the new growth.

"11. Even simple adhesion of the new formation to the part in which it grows, or to the subjacent textures, renders failure almost a matter of absolute certainty.

"12. The existence of the cachexia renders perfect and permanent recovery infinitely improbable, if (prop. 3) not impossible.

"13. With each repetition of the operation in the seat of original formation, the chances of return of the disease increase to an incalculable degree.

"14. The removal of two cancers simultaneously, or immediately after each other, may be considered a procedure which will almost of necessity be followed by return of the disease.

"15. The probabilities of effecting a cure are said to vary with the species of cancer.

"16. The removal of cancerous growths is more prone to be followed by relapse when the disease occupies certain organs or tissues than others.

"17. In cases where cancerous substance is simply superadded to preexisting encysted structure, the chances that relapse will not occur are above the average, provided the entire of both morbid products be removed.

"18. Cancers which have spontaneously, or through the influence of treatment, ceased to enlarge, to cause local suffering, or general disturbance—in fact, become quiescent—have sometimes been cut out, and the operation been followed by rapid reproduction of the disease, with aggravation of the local and general symptoms and early death.

"19. Organs, as for instance the breast, have sometimes been removed for affections presumedly cancerous, but in reality of the most innocuous character (e. g. cysts, and even abscesses): in such instances the operation has occasionally proved fatal." (pp. 231-6.)

We give also, in the author's own words, the general inferences which seem to us, as well as to him, to flow of necessity from these premises:

"First: in as much as the number of permanent recoveries is infinitely small, and as no combination of circumstances, however favorable, protects the patient from relapse,—*the operation cannot in any individual case be recommended as likely to cure the disease.* Secondly: in as much as no operation by excision is performed without the chance of some of the diseased structure being left behind, an accident which hastens the progress of the malady;—in as much as absolute certainty of the freedom of internal organs from the disease is unattainable;—in as much as the dormant cancerous diathesis is sometimes roused into activity by removal of a tumour;—in as much as cancers in a state of active growth acquire increased energy of vegetation, if reproduced after extirpation;—and, lastly, in as much as the operation itself has not very unfrequently proved both the occasion and the cause of death; *excision cannot be undertaken without imminent risk of placing the patient in a worse condition than he or she was previously to the use of the knife.* Thirdly: as a corollary from the first and second inferences; and in as much as the disease has unquestionably been cured, or arrested in its progress, by milder measures;—and in as much as the disease does not by any means inevitably and unfailingly run a course destructive of life;—and in as much as quiescent cancers have sometimes been succeeded by most active and virulent growths in consequence of untimely extirpation; and inasmuch as life has sometimes fallen a sacrifice to operations for the removal of putative cancers; *the operation should, as a general rule, be abstained from.*" (pp. 236-7.)

It will, indeed, in our opinion, be a happy day for the honour of surgery and the interests of humanity, when the rash use of the knife is for ever banished to the place of things which are not.

But, setting aside all expectations of cure, may we not operate in the hope of *prolonging the patient's life?* Let the following facts, adduced by Dr. Walshe, answer the question.

In 85 of Benedict's 98 cases of amputation of the breast, death was *hastened* by the operation. From the experience of Dr. Macfarlane, whose operations were executed under the most favorable circumstances, it appears that, of 32 cases, two terminated fatally from the immediate effects of the excision, and reproduction occurred in all the others within the following periods:

No. of Cases.	Period of Relapse.
9	Between 6 weeks and 3 months.
13	Between 3 and 9 months.
4	Between 9 and 12 months.
3	Within 24 months.
1	Nearly 36 months.

Whence it follows, that the mean period of relapse is something less than eight months and a week; and that the chances are as seven to one that extirpation either proves fatal at once, or is followed by reproduction within twelve months. And that the mean duration of life is not increased by operating is shown by the records collected by M. Leroy d'Etiolles. From the first series of these facts, comparing the duration of life after the sixth year in persons operated on and not operated on, the subjoined table may be formed:

Total duration of Life from outset of Disease.	Of 1172 Patients not operated on, there reached these periods.	Of 801 Patients operated on, there reached these periods.
Upwards of 30 years	18	4
From 20 to 30 years	34	14
From 6 to 20 years	288	88

Whence it follows, that the commencement of the sixth year of a cancerous disease will be reached by 13·2 per cent. only of those operated on, by 29·7 per cent. of those who have not been submitted to the knife.

It were needless to adduce more proofs of the melancholy fact, that extirpation of cancerous growths with the knife can neither be regarded as a means of cure, nor of prolonging existence; and to our minds the conclusion is irresistible, that, excepting under very peculiar circumstances, such as the threatening of immediate death for incontrollable hemorrhage, or complete occlusion of the rectum in carcinoma of that viscus, the surgeon who cares for his own reputation and the real welfare of his patient should refuse to subject him to the danger and torture of an useless operation.

The other methods of ablation will not detain us. Cures have been effected by the enucleation of tumours by caustics (and for this purpose the chloride of zinc is perhaps the best and safest), and even by the induction of sphacelus by inoculation of the matter of hospital gangrene. But the experiments have not been conducted on a sufficiently large scale to warrant any satisfactory conclusion; and, while better and less hazardous means are in our power, we should hesitate long before advising a course so painful.

Here, for the present, we must pause with the remark, that in all that precedes we have given a mere epitome of the author's views. These seemed to us so just, that we found no ground for cavil or criticism. Our analysis is avowedly meager and dry, but we believe it will be found accurate.

In our next Number we shall hope to accompany our readers through the remainder of this truly valuable work, and help them to form a closer acquaintance with this formidable disease in the various organs and tissues which it infests.

ART. IX.

Déontologie Médicale, ou des Devoirs et des Droits des Médecins dans l'Etat actuel de la Civilisation. Par le Docteur MAX. SIMON.—*Paris,* 1845.

Medical Deontology; or, the Duties and Rights of Medical Practitioners. By Dr. MAXIMILIAN SIMON. 8vo, pp. 590.

MEDICAL ethics have never attracted so much attention from laymen as medical theology. The opinions of physicians on questions of faith have generally been held to be of doubtful orthodoxy; their morals have usually been found unimpeachable. Trustworthy as men, they have absurdly enough been deemed irreligious. In this respect, however, they have but endured the common fate of all philosophers. From the earliest period the student of nature has been thought little better than an infidel; and, doubtless, this popular belief in one sense has always been well founded. How is it possible to imagine that Anaxagoras, Epicurus, Hippocrates, Socrates, or other like-minded men, ever expressed an earnest faith in the mythology of their age? Or can it be supposed that the modern geologists (holding the cosmogonical opinions they avow) participate in the popular belief as to the creation of the world? The Dean of York, although wrong alike in his theology and his philosophy, is true to the instincts of the ignorant multitude whose sentiments he expresses. Philosophy will ever war with mere dogmas, and medical science is philosophy *par excellence.*

And yet we venture to assert that there is not a more truly and sincerely pious, nor a more earnestly moral class of men than our own much calumniated profession. It ill becomes any profession to laud and magnify itself; but the avowal and defence of truth should not and shall not be sacrificed to the paltry fear of being thought vainglorious; or of being considered as emulous of those canting knaves who use religion as a cloak for pride, ignorance, or avarice. The cultivators of medicine, equally at least with the members of any other profession, may, without usurpation, to use the words of Sir Thomas Browne, assume the honorable state of a Christian, and we dare assert that none disgrace that honorable name less than medical practitioners.

If proofs of these assertions were wanting, they could not be more amply or aptly supplied than by the book before us. We subjoin an example, and we do this with the most perfect confidence that none but the most narrow-minded bigots can fail to recognize in it the pith and essence of Christ's teaching,—the marrow of that new law of mutual love which he left to mankind.

"With the man who is responsible to God alone for his decisions and actions regarding a thing so precious as human life, the thought of God should be ever present. A philosophy through which the sap of this fruitful thought does not circulate will be powerless to guide the physician at all times in the midst of the numerous quicksands he must meet with in the exercise of his profession. Medicine may be traced to God through the sympathy which the appearance of suffering awakens in us; but as a science of so high origin it only completes its work by asking from charity its love and its devotion. The physician who takes

the light of this dignified philosophy as the guide of his conscience may fail, but his fault will only be imputed to the imperfections of science. Understanding the dignity of human nature and the profound objects of life, he will devote himself entirely to the study of a science which can influence so decisively the individual destiny of men. Prudent and circumspect, he will not be seen to adopt hastily those unripe theories which sometimes pass over a generation like a destructive epidemic. In those cases, in which both theory and experience refuse their teaching, he will adopt the method of a prudent expectant treatment. To whatever rank of society he may belong, the sufferer will be his brother by the double relationship of pain and hope. Beneath the most hideous rags of misery he will recognize the indelible marks of the sufferer's heavenly origin, and with tender solicitude heap upon him the most devoted attentions. If in practice he meets with one of those difficult cases in which one means only can save the patient, and the use of those means may destroy his practice, if it fail, he will know how to do his duty, and endanger his reputation and success. Good and kind to all, he will not use his amenity of language as a privilege to gain wealth or rank. He will be aware that suffering spiritualizes a man, if the expression may be permitted, and gives for the moment to the most unpolished natures a delicacy and sensibility which rudeness of manner deeply hurts. He will be as affable and agreeable at the truckle-bed of the poor as in the perfumed boudoir of the rich; and thus both fulfil a rigorous duty and secure the action of those curative agents which science may direct.

"But conscience abandoned to her own guidance may stumble in the dark paths along which she might lead us: she is accessible to passion, and is led by fantasies, as is every power not attached to something fixed and immovable. It is necessary to mount higher and find a surer guide; it is necessary to ascend to that Christianity itself which affords infallible instructions suited to every station of life—to that Christianity which, expressing its doctrines in the one word CHARITY, is wonderfully allied to a science of which the essential object is the solace of human suffering. The men who have left behind them the most glorious names in the history of our science, Bayle, Van Helmont, Stahl, Sydenham, Boerhaave, Hoffman, Van Swieten, Winslow, Esquirol, &c., have well understood that it is in Christianity only the physician should seek that light and strength of which he has need, to maintain him equal to his difficult mission." (Introduction, p. 24.)

M. Simon at the outset, thus takes the sublime morals of Christianity as the fundamental principles of his medical ethics. Christian morals are beyond all controversy, whatever may be thought of Christian dogmas.

DEONTOLOGY is a word adopted from Bentham, who used it to signify morals, or the science of duties. M. Simon uses it in the double sense of duties and rights, observing that the true meaning of the root of the word, δέον from δεῖ, oportet, warrants this double use of the term. Doubtless all duties are reciprocal; the rights of medical practitioners are the duties that society and individuals owe to them, as their duties to society and individuals are the dues or rights of the latter. A general view of these duties and rights is given in a lengthened introduction; it is a recapitulation, in fact, of the contents of the work.

M. Simon divides his subject into four books. The first treats of the duties of practitioners to themselves and to science; the second of their duties to the sick; the third of their duties to society; and the fourth of their rights, or, in other words, the duties of society and of the sick to them.

In treating the subject of the first book, he first inquires what motives ought to guide a practitioner in the cultivation and practice of his profes-

sion? Our author well observes that the liberal professions sink lower and lower in public estimation. Medical practitioners, in particular, are full of complaint of the little honour and regard shown them by society. We should, however, be inclined to doubt whether the present generation croak more or in a more melancholy strain than the last; or whether the profession is really in less esteem than formerly. M. Simon believes this decadence to be real, and thinks the reason thereof is manifest enough. Few practitioners are guided by the principle of action which can alone secure them the regard and esteem of society. They are not devoted to the common interests, but to their own. They live *out of* society, and not *for* society; so society owes them nothing, except a watchful guardianship that they shall not under the sacred garb of the profession act the medical Tartuffe, and destroy rather than restore. There is nothing new under the sun; men are always equally selfish; the object of desire may differ, their greediness is ever the same.

Not long ago we happened to turn over several volumes of the earlier medical journals, and we were struck with the identity between the croaker of the existing and the last generation. In the earlier years of the present century medical reform was vigorously agitated by Sir Joseph Bankes, Dr. Beddoes, Dr. Harrison, and others. Much discussion was excited, the colleges were disturbed from their slumbers, the ultima Thule of the profession aroused. And what was the great characteristic of the discussion? Utter, barefaced selfishness. The London College of Physicians sent advertisements to the political journals, denouncing Scotch graduates as illegal practitioners; the King and Queen's Colleges of Physicians of Ireland answered the call for reformation by complaints regarding the encroachments of surgeons and apothecaries, and by a demand for further monopoly and more restrictive powers. Then as to the general practitioners, Dr. Beddoes, in his commonplace book (date 1808) observes, "the quantities of medicine ordered drive people to quack medicines. People can drop the latter when they please, but *verecundia erga medicum* makes them keep on longer with the former than either their stomach or their purse can well endure. To rescue the public from the stomach part of this evil, it will be proper to enact that the apothecary shall charge for more draughts than he supplies, as used to be the case with post-horses in France." In 1803, a country surgeon writes to a journal, "It is a melancholy consideration that whilst almost every class of men are in a state of improvement, the Faculty are certainly going retrograde; I speak to their revenue; competition has reduced the profession to the lowest ebb, consistent with comfortable support. . . . . . In short, such is the state of the country practitioners, that empiricism must soon supersede science, if every facility is not afforded him to procure information!" Complaints were loudly uttered not long ago against the *drenching system;* but, bad as it still is, we are sure that never did it flourish less than at this moment.

There is more hope, then, of a moral regeneration of the profession than croakers will allow; and there is the more hope, because the selfish are beginning to discover that a thorough devotion to professional science and duty is the surest, if not the shortest, way to wealth and esteem. They even doubt the wisdom of those public advisers who speak of the incomes

of quacks as so much taken *from* the pockets of regular practitioners. Although we think M. Simon too enthusiastic, we would not despise the high moral feeling which demands this devotion for the sake of virtue and for the love of God. But the incentive of a personal advantage to be obtained is a true and right incentive. How does the Wise Man incite to the love and pursuit of wisdom or virtue? By describing the advantages she can confer. "Length of days are in her right hand: in her left hand are riches and honour." Our author's sentiments are as follow:

"Follow the physician in imagination through all the situations in which the requirements of his laborious profession place him; follow him to the palace of the rich, or to the hut of the pauper; among the poor artisans who want air, or the peasants who have that alone; follow him amid the epidemics which decimate the populations around him; amid the contagion whose deadly miasm he may absorb at every pore; to the courts of justice, where his scientific words may advance or injure the most important interests of society, or of the individual; in short, follow him in the numerous directions where his office necessarily calls him, and you will always find him surrounded by the most imperious obligations. Now, the abstract idea of duty and the absolute principle of moral obligation are alone able to maintain him constantly in strength for his duties. He only who directs his conduct by this higher motive will make art equal to science, and know not the hesitations of devotedness which are nourished from sources less pure. The man who submits to this moral regimen contracts a habit of virtue—of self-denial—which will render his duty easy whenever it shall be opposed to his own numerous passions." (p. 53.)

The duties of medical instruction and authorship are to be regulated, our author thinks, by the same high principles of unselfish devotedness. The greater his influence, and the wider his sphere of action, the more incumbent it is upon the teacher to purify the sanctuary of his conscience, and generously expel every interested motive. In medical science false ideas and crude theories may easily develop a dangerous method of treatment. Vanity, pride, avarice should be so chastened as not to obscure and deform the scientific page. M. Simon might have referred to a tribe of crawling scribblers, whose vocation is the pursuit of a miserable notoriety by the publication of unmitigated falsehoods. These men report cases they never witnessed, assert cures they never performed, record facts that never occurred. We promise ourselves some day the pleasure of impaling a few specimens of these creatures on our critical pens, and placing them with outstretched wings in our bibliographical museum.

What is the effect of medical studies and practice on the morals of the practitioner? M. Simon thinks they have a baneful effect. The pursuit of descriptive and pathological anatomy familiarizes the practitioner with man in a state of degradation. The majesty of death teaches him nothing, and affects him not; the corpse before him is a "subject," and nothing more. The infirmities of diseases and the nudities of death are necessarily ever present with him, and life only appears as a veil thrown over them. If the feelings thus excited are not counterbalanced by an abiding conviction of the dignity of human nature, the tender sympathy and the nobler instincts of humanity will be injuriously weakened, and with them the mental powers themselves. An impudent cynicism is excited, a looseness of language and deportment follows, and the student becomes the Don Juan of the dissecting-room.

The religious opinions of the practitioner suffer also, and he becomes affected with infidelity. On this point M. Simon dilates considerably. The charge of heterodoxy and atheism, as we formerly remarked, has been brought against philosophers, and especially physicians, from time immemorial. The "Beware of Philosophy" of the priest is much more ancient than the "*cave canem*" of the Romans. It is quite true that many philosophers (and medical practitioners are to be classed with them) hold opinions differing from the popular belief, and on this difference the charge of atheism has been ignorantly founded. Of those absurd dogmas which medical practitioners, especially in Catholic nations, are categorically required at once to believe and explain, Sir Thomas Browne, in his 'Religio Medici,' humorously remarks: "There are a bundle of curiosities, not only in philosophy, but in divinity, proposed and discussed by men of supposed abilities, which indeed are not worthy our vacant hours, much less our serious studies. Pieces only fit to be placed in Pantagruel's library, or bound up with Tartaretus de modo cacandi." The materialism of the medical practitioner is often no materialism at all, but only a dissent from the pagan doctrines that have crept in amongst the Christian. His habit of exact dissection; his knowledge (of the most demonstrable certainty) that the phenomena of mind, so far as we are able to understand them, are dependent upon organization; the constant proofs he has of a far more profound scheme of creation than the generality of divines teach, or than he can fathom; and the frequent illumination of that vast abyss of life which science has disclosed to him by her own lightning flashes, all tend to impress his mind with the greatness of his ignorance and the necessity of faith. Where fools rush in he fears to tread. "We are men, and we know not (scientifically,) how: there is something in us that can be without us, and will be after us: though it is strange that it hath no history."

The repeated witnessing of pain in our fellow-creatures has necessarily a tendency to blunt the sensibilities, and in so doing to render us less useful in administering solace. This is true in some degree, and ought to be combated by a sedulous kindness of manner; but much depends upon the natural character of the practitioner. Many are born with tender and compassionate hearts, and to such the repeated contemplation of suffering acts only as a stimulus to greater efforts. On the other hand, the medical practitioner or student is peculiarly liable to suffer from hypochondria. The study or practice of his profession effects the same change in his nervous system as is induced in the layman by the perusal of medical books. There, we suspect, are few practitioners of eminence who have not been consulted by students or young practitioners for some imaginary disease or other, but most frequently, perhaps, for supposed pulmonary phthisis, or disease of the heart.

M. Simon advocates the propriety of a greater respect for the dead than is usually exhibited by the profession, especially by students. We confess that we have not witnessed without horror the profane desecration of the human remains brought into the dissecting-room. We trust the prosectors of the present day have changed all that, and humanized the student. It is impossible to escape altogether from the benumbing influence on the sensibilities which familiarity with corpses develops, but

it may be moderated and counteracted. One absurd means recommended for this purpose by M. Simon is, that the walls of the anatomical theatre and dissecting-room be covered with short philosophical sentences. In hospitals respect for the dead is really an imperative duty. The dying, in witnessing the treatment of the lifeless corpse, witness the mode in which their own will be shortly treated. They not only fear to die, but they fear any indignity to their remains. It is possibly a selfish superstition, but it is strong and universal. How many gorgeous tombs and monuments has it not raised? How often has the high-minded and brave Greek or Roman, touched by the poetic mythology of his age and nation, feared less to die than to remain unburied?

> " Hæc omnis, quam cernis inops inhumataque turba est;
> Nec ripas datur horrendas, nec rauca fluenta
> Transportare priùs, quam sedibus ossa quiêrunt."

M. Simon strongly objects, and we think with reason, to the sale of bodies practised at the French hospitals. In England, he observes, this disgraceful practice was carried on only by a few infamous men: in France society is an accomplice in the profanation, by tolerating and legalizing it.

M. Simon devotes a chapter to the consideration of the intellectual and moral qualities which the practitioners should possess or acquire. The more human life is valued, the more valued will the practitioner be. Independently, however, of any estimate placed on his services by society, feeling, as he must feel, how sacred a thing human life is, the conscientious practitioner will spare no labour so to qualify himself for his duties, that the assistance he may tender shall be the most effectual possible.

The first requisite is a continued mental activity—a habitual course of mental gymnastics. The practitioner should be *always* a student, not merely of the practice of medicine, but of those kindred sciences which develop the mental faculties, and give them harmony. This method will prevent him degrading his profession into a matter of mechanical routinism; will enable him to afford a just and not a short measure of skill to the patient who places his confidence in him; and will render the fulfilment of his onerous duties a pleasure, and not a task. This love of his art will further develop in him a love of his science, and thus he will be incited to those labours of investigation by which science can alone be advanced, and to the scrutiny of the facts and theories which continually come from the prolific laboratories of medical philosophers. However extensive his experience may be, he will still have need of the experience of others. The physiognomy of disease is so changeable that no nosography can accurately define its ever-varying phases; the causes of diseases are so numerous and novel, that no therapeutics can unravel their complexities, and eradicate or check them. The habit of close and accurate observation should be diligently acquired as the best means of developing that medical hyperæsthesia of the senses, termed tact. It has been much the custom to decry the skill and tact of scientific as opposed to empirical practitioners, and we suspect that this disparagement of the former has been founded upon the different circumstances of the two classes. While the one has used more assiduously the perceptive organs, the other has put

the intellectual or reflective into greater exercise. The latter has indulged in the generalization of phenomena; the former in their analysis and relations. It is, therefore, of great importance to the scientific practitioner that he should seek after truths that may be applied rather than after abstract truths; that he should minutely investigate pathological phenomena day after day, either to illustrate known principles, or to discover unknown; that he should experiment less in the laboratory, and study more the experiments of nature. We will answer for the success of this method in forming a sound and successful practitioner. Nothing can compete with that union of the reflective and observing faculties which we have just recommended.

We have always thought a mature middle age the ripest period of life in a physician. To the young man we would say beware of precipitancy and self-confidence. The art is not so sure as the lecturers and professors lead you to imagine. Their systems are necessary for the development of their subject; they teach you what will cure rather than what will slay; the latter they leave you to discover yourselves—forgetting, perhaps, that your ardent imaginations differ widely from their mature experience. But if youth has its unbridled energies and undarkened hopes, age has its fears and cowedness. The one is really as dangerous as the other; it is as disqualifying to hope nothing from medical aid as to hope everything; we may kill by tardiness and inertness as effectually as by precipitancy and the so-called heroic treatment. Whether it be preferable to die *secundum artem*, or *secundum naturam*, may be a nice question, and, indeed, is perhaps only a matter of taste; most people prefer not to die at all. If a man be naturally of a timid temper he is scarcely fit for the duties of medical practice: decision and energy are as necessary at the bedside as on the field of battle. If such a man wish a sacred calling, let him put on the priestly gown. Even if by a strong effort of study he be deeply convinced of the mighty powers of medical art, still his natural temper will continually stay his hand while wielding those powers. The meditated blow will be checked! the act that will inflict present pain, but secure future ease, will be undetermined so soon as decided; the practitioner will be a vacillating doubter. But to the man who has from his organization, or his training, acute senses, quick perceptions, a comprehensive mind, steadiness of temper, firmness, and decision—to such an one the science and art of medicine must present a field for happy and contented employment that no other profession can offer.

It cannot be denied that the minds of some practitioners are imbued with an abiding scepticism as to the power of medicine as an art, and its reality as a science. When a contemplative and naturally doubtful mind glances through its history, it necessarily revolts at the numerous contradictions afforded. There is to be seen a continual conflict of the most opposite principles and modes of treatment. Now it is true the one theory or method is in the ascendant, but by the time it attains its climax it has called forth the antagonist that will surely supplant it, and rise to eminence in its turn to be again supplanted by another. And what is true of general doctrines is also true of special remedies. There is a mode or fashion in physic: a conventionalism is established, and men of medium powers lisp its phrases, thinking their wisdom oracular, until the ebbing

tide leaves them ashore amidst the debris of their favorite methods and theories. It may be doubted, however, whether theories have much *permanent* influence on practitioners in general, and on the treatment of diseases. The therapeutics of the Hippocratic writings do not so widely differ from those of the present day as might be expected from the lapse of two thousand years; and the difference is still less obvious if the beginning of the last century be made the point of comparison. The animists, the intromathematicians, the Brownists, and the Broussaists, are but a little more lifeless than the "liver doctors" and the "heroes" of the present century. Certain principles and methods of treatment reappear or arise amidst the flux and reflux of medical opinions, however deeply they may have been overwhelmed. These must be true principles, having stood the test of time; and, consequently, scepticism is unjustifiable. The history of medicine should form part of the student's course; not a history of dates and names, but a history of evolution and development of doctrine—the embryology of medical art. This would be the best safeguard against the foolish conviction that there is no help in it.

M. Simon devotes a chapter to the literature of the profession, and takes occasion, in the first place, to impress on his readers the necessity of cultivating a good style. He objects to the dry, costive method of communicating facts; he would have the language elevated, noble, dignified, picturesquely elegant. Great facts must be described in great language, or they are overlooked, or appropriated by the more elegant writer. Many forms of disease can only be described in elaborate and forcible sentences; the style must be eminently descriptive. The scribblers of the profession are a true plague; they fill the journals with "cases," the truth of which is often questionable, and when not questionable are so imperfectly narrated as to be useless. The object aimed at by them is a short and valueless notoriety. This attained, these medical Bombastes care not how they fill up the columns of the weekly journal with their rubbish, for which the honest hard-working practitioner has to pay; or how they load the index through which the true littérateur has to wade. We certainly think the editors of weekly journals should exercise a stricter discrimination in the admission of communications, as well for their own sake as for the sake of their subscribers. Lies and lumber they should carefully eschew.

A good style can hardly be attained (except by the genius) without a good education; and genius itself often combats in vain with the defects arising from an imperfect literary education. The expression of great physical truths requires as much cultivation of the intellect as the expression of great moral truths—nay, we think more; for the former is addressed almost exclusively to refined minds, while the latter, being studied by every man, readily finds a welcome and an echo in every man's heart. This intellectual cultivation would develop more of sentiment and poesy in medical writings.

M. Simon justly animadverts on the absence of anything like sympathy for human suffering in the works of modern writers on pathology. The study and practice of pathological anatomy seem to have frozen up the springs of human feeling. Sufferings are described with as much icy coolness as the characteristics of a fossil reptile; or rather with more; for

the geologist often warms with enthusiasm while he writes. He pictures the unwieldy creature in detail; he measures its teeth and jaws with undisguised interest; describes its tail, its claws, its eyes; he sees it in imagination living and gambling in its native haunts; now raising its long snout above the warm mud in which it is snugly ensconced, now cleaving with frightful velocity the tepid waters of its home. Even in our northern clime, he pictures skies above it of tropical splendour; flowers, and shrubs, and trees around it of tropical grandeur; birds and insects flitting near it of tropical gorgeousness. But the pathologist having recounted his case, it may be of hydrophobia, arachnitis, or tetanus, with a deliberate minuteness, winds up with a detail of the post-mortem examination, and a self-congratulation at being able to communicate particulars of such an "interesting case." There is not one word expressive of sympathy or kindness.

There can be no doubt that this cold though truly sciential style impresses the lay public more unfavorably, and is more injurious to the dignity and honour of the profession than many would willingly allow. M. Simon seeks not from the pathologist a funeral oration, or sepulchral poetry in the form of medical prose. He would present as models of a suitable style the Enchiridium of Hufeland, the Practical Medicine of P. Frank, the Pathology of J. Frank, or the Essays of our own Fothergill, especially that in which he describes pulmonary phthisis.

Chastity of language is another point M. Simon dwells on as necessary in medical literature. He thinks a writer on subjects which were formerly thought to demand the use of the Latin language should go direct to his subject, and use none but strictly technical words and phrases. Chlorosis should not be "fièvre d'amour;" the pubes is much better called pubes than the *mons veneris; syphilis* is the pox, and not the ladies' fever. Many of M. Simon's remarks on this head are much more applicable to French literature than to our own, if we except 'The Silent Friend,' 'C. . . . . on Marriage,' and similar bestialities of typography. For these, however, courts of justice are answerable, not the profession.

A chapter is devoted to truthfulness as a duty to science. In discussing this point, M. Simon severely animadverts upon those plagiarists, the pirates on the high seas of literature, who not only rob their neighbours, but disfigure and pervert to base uses their goods. The duty of truthful observation is strongly set forth; and especially in all cases the duty of showing the negative as well as affirmative side of every case or discussion.

The passions of medical men have a special notice, and first comes personal rivalry. No science lulls selfishness to rest—medical science as little as any. Satirists have always delighted to picture the jealousies and personalities of medical practitioners. A dialogue from Molière may be paralleled even now:—

"*M. P.* Je soutien que l'émétique tuéra.

"*M. des F.* Et moi, que la saignée la fera mourir.

"*M. P.* C'est bien à vous de faire l'habile homme!

"*M. des F.* Oui: c'est à moi; et je vous prêterai le collet en tout genre d'érudition.

"*M. P.* Souvenez-vous de l'homme que vous faites crever ces jours passés?

"*M. des E.* Souvenez-vous de la dame que vous avez envoyée en l'autre monde il y a trois jours?"

Nothing can be more destructive to the character and influence of the practitioner than scenes of this character. Yet the most polite, the most placid, and the most urbane may be so soured by the impudence and ignorance of some vulgar pretender, that, in spite of himself, he may be a principal actor in a scene like this. We believe it quite impossible to exclude men of low minds from the profession; the only remedy is for the high-minded to eschew them. An offence against good-breeding is sufficient ground for a professional excommunication. If the offender cannot be driven from the field, let him have it to himself. It is better in every way rather to flee than strive; the public ultimately judges right. The remedy for professional jealousies is frequent intercommunication. A good dinner once a quarter at the "Royal" or other hotel would heal the professional feuds of a large town. The man of science who practises his profession for the love of it may smile at the sensualness of the means, and truly it is not the remedy *he* would require; but most practitioners are men of the métier, and they like a trades-union, as well as any class. We wish there was a medical guild in every large town, with an ample dinner-fund; good fellowship, we are certain, would the more abound, and, with that, unity of purpose, honour, and public esteem.

Medical criticism is the next subject discussed by our author. He justly remarks that the practitioner who writes for the love of the truth, and to advance his art rather than himself, will willingly submit to fair criticism, and readily acknowledge errors. Prejudices and errors may be ridiculous only in many departments of science; in medicine they are murderous. Impartial, independent, and strict criticism is a duty incumbent on the medical critic; he cannot yield to personal animosity, or cowardly complaisance, without incurring a serious responsibility. As the vigilant sentinel of true science, he must stop alike the intruder and the foe, the hurtful and the useless, the fanatic and the selfish. We will boldly state, because we can honestly state, that such have been our critical canons. Often have we controverted views and facts, when approval would have been a pleasanter task; often have we applied the lash of our satire with regret that our pages should be devoted to the ignoble purpose of punishing the quack and pretender. Often have we desired to encourage the vigorous growths of science, while we endeavoured to repress the superabundant vegetation—rooting up weeds while we thinned the brushwood.

M. Simon refers with impartiality to the false patriotism of those critics who see no merit except in their countrymen's writings. The French and Italians are more remarkable than either the English or Germans for this narrow feeling of nationality. M. Simon refers specially to the reiterated attacks of the French philosophers upon Newton in favour of Descartes as a memorable example; but he thinks, on the other hand, that the discovery of Laennec has been treated with neglect in other countries;—the English and German physicians, he remarks, still contest its value. Surely M. Simon is little acquainted with English medical literature or practice, to make this assertion with respect to England. It may be true that neither Hufeland nor Frank have adopted the stethoscope, but the neglect of that valuable instrument by them should be rather attributed to the infirmities of age than the prejudices of country.

Literary comradeship, M. Simon thinks, has a baneful influence on fair

criticism. The mutual admiration displayed in literary cliques is more ridiculous than dangerous; at least in England, where we have so many societies, sections, parties, and organs of parties. The repulsion of incongruous minds counterbalances the attraction of the congruous so soon as it is maudlin. If this were not so, there is, we believe, no better remedy for the spirit of cliqueism than an independent journal. The impudence of some men at professional reunions is astounding; first, in praising their superiors, and, secondly, in thinking their praises worth having.

What are the duties of the practitioner towards the sick? The first and the most imperious is, never to refuse medical aid to any one desiring assistance. And this he must do impartially, not having respect to persons. The only rule of preference should be that based on the degree of suffering. Nor is it by any means right that the practitioner should unequally regard the rich and poor, the good and the bad. Like the sun and rain of Providence, he must vivify and cherish both the just and unjust. We know that there are many puritanical persons who think themselves entitled to judge others, and who would relieve only the "respectable;" thus is their self-righteousness breaking one of the first rules of Christian charity. The true-hearted and really good practitioner will never allow such motives to influence him. Where shall inquiry into character begin?—where end? The distribution of medical relief, if made according to this rule, should be at least just, and the secret wickedness should be estimated even as weightily as the open. The undetected criminal is often the consummate hypocrite. The question scarcely, however, admits of serious argument.

The patient, M. Simon observes, whoever he may be, should be treated with unvarying kindness. In particular, his own history of his sufferings, even though wearying and prolix to the listener, should be heard patiently. The patient hearing is of itself no small solace to the sick man. Numerous engagements may not always permit a prolonged audience, especially when the patients are poor. *Il faut vivre.* Still, if the sick person be cut short in his, to him, touching history, it may be done so kindly that he willingly acquiesces. A kind word or two, a pleasantry adapted with perfect good taste to the man and the moment, a friendly glance, or pressure of the hand, may each and all be made efficient substitutes for a long sitting. Our own Fothergill is again quoted as an example of unwearying pleasant kindness to the poor. The great secret is to take an interest in the patient's case, and to exhibit an earnest desire to be useful. This manifested in the mode by which gentlemen are accustomed to display their feelings, will be sure to succeed in gaining the good will and confidence of the patient. A pleasant, soothing, kindly manner may thus, from day to day, bring solace to those labouring under even the most incurable diseases. Of course M. Simon has no sympathy for the rough-spoken imitators of Abernethy or Dupuytren. Firmness may be conjoined with gentleness; *medicus sit sermone potens;* persuasion should dwell on his lips.

Discretion is another quality necessary in medical practice. How many break the golden rule of not talking about their patients! These babblers go about from house to house—a long list in their hand—repeating the names and diseases of their patients with no other object than to gratify a prurient

desire for scandal in themselves and their hearers. We scarcely know anything in worse taste than this sort of conduct. Secrets of families are thus often hinted at or improperly revealed, and the practitioner is a traitor to the interests of those who have confided in his judgment and discretion. It may be a duty, however, to reveal the secrets of a patient, as in a case of undetected criminals. This is a question on which much nice casuistry has been displayed, and the limits of which have not yet been defined. In fact, the practitioner must take his own conscience as a guide in the matter. For ourselves, we would denounce a murderer, but not a poacher.

A chapter is devoted to a consideration of the duties which the practitioner owes specially to the sex. For his own interests he will endeavour to secure the good opinion of the ladies, as well by his kindliness of heart as his modesty of deportment. M. Simon dwells on this subject with all the grace and courtesy to be expected from an accomplished French gentleman. Women look upon their doctor as their friend: they are zealous for his interests; they are enthusiastic in his praise. This warm feeling of friendship renders them a little, but most excusably, *exigeant :* submission with a good grace is a duty. Rarely are females medical sceptics; rarely do they scoff at the art or the science, like those strong, bullet-headed men, who revel in the enjoyment of physical strength, and believe not in medical skill, because they know not disease. M. Simon would not, however, encourage the whimsical, the capricious, or the lascivious; the little revenges practised by a few spasmodic movements induced *apropos* are not to be ministered to, nor the thousand absurdities of ladies with weak nerves.

M. Simon thinks special regard should be had towards old men. They fear death; they are cowards in suffering; they are querulous, obstinate, imperious; and they are, therefore, the more likely to induce a feeling of disgust which may lead to cruel neglect, or a thinly disguised routinism. In old men the love of life is so strong, that if they heard of a country where people never die, to that they would go (the bull is M. Simon's,) to end their days. Nothing is more easy than to flatter and gratify this love of life by proper attentions, not forgetting sage hints from time to time that man, and especially an old man, is mortal. The warnings his sufferings give should be cautiously re-echoed; one word in conversation, a doubtful answer, or a reluctant opinion, will be quickly understood, and produce better effects than a course of the most orthodox sermons. It is a remarkable circumstance that those old people who talk most frequently of their age and infirmities think them of the least importance. They have usually been invalids through life; have had much experience of physicians; and have one or more favourite theories by which they explain away the most threatening symptoms. M. Simon refers to the sensual desires of old men, and the love, in particular, of sexual pleasures they display. Of course such desires should not be pandered to; they are, we believe, in many cases morbid phenomena, and significant of disease in the nervous centres.

M. Simon professes the most generous and sublime views as to the conduct of the practitioner in times of pestilence. We must confess that we have no sympathy with that heroic self-devotion he would have the practitioner

display. We know by experience that society has always been most unjust towards those who have laboured at the greatest risk of their own lives, and of the lives of those dear to them; and who have regarded no labour or expense during a fearful epidemic. Our experience of the cholera pestilence is too recent to doubt that it is equally just and politic that society pay the practitioner in proportion to the risk he incurs, and the labour he undergoes. It is sacrifice enough if he keeps his post in the hour of danger. And yet how often were the most arduous services to the public, during the prevalence of the cholera, rewarded with a stingy and empty vote of thanks! We should be glad to see an efficient organization of the profession, if it were only for this:—that it might compel society in troublous times to be *just*.

M. Simon advises that when a fatal epidemic disease breaks out, the practitioner should immediately declare it to be non-contagious. We should be sorry to sanction such a step; our English notions of truth forbid us to counsel a lie. It is always impolitic, too, to do evil that good may come. M. Simon further states, that as one attack of some epidemics affords immunity from a second, this principle in pathology should be made popularly known, and applied even to those diseases to which it is not applicable. Courage and hope, he argues, would thus be sustained, and selfishness defeated. This is again to counsel a falsehood, and such counsel, we repeat, is as abhorrent to our principles of moral rectitude as to our views of sound policy.

A chapter is devoted to the question of circumspection in the use of remedies. We have from time to time denounced that mode of treating diseases which has been absurdly enough termed heroic, but which more properly should be termed murderous. The conviction, however, of the dangers and destructive effects of such a method is now becoming more and more general. M. Simon, we need scarcely add, on this point, is in accord with ourselves. But too great circumspection may degenerate into timidity, and this be really as injurious as heroism. "Saltem non nocere" is a better maxim than "melius anceps quàm nullum remedium;" but a passive inertness must not be confounded with careful medication.

M. Simon defines medical esoterism to be that mysticism with which the practitioner performs his daily duties, and which he is compelled to adopt by the prejudices and ignorance of his patients. He deceives in pity for the weaknesses and timidity of mankind; he practises dissimulation that he may the more effectually administer relief. M. Simon thinks that he may, for these reasons, be a hierophant without blushing. A long chapter is devoted to esoteric medicine, and the first topic introduced is, what sort of conduct should be adopted in the management of patients with incurable disease. It is well known to experienced practitioners that few persons who so suffer are willing to abandon all hope, and as soon as the truth is announced to them by their attendant, they seek for remedy elsewhere, and often become the dupes and prey of unprincipled charlatans. Many practitioners think it their duty to tell the truth plainly and bluntly, regardless how they hurt the feelings of their patients, or sacrifice their own interests. We believe this is usually done from a highly honorable principle of action, although occasionally it is mere tyranny; but we question whether, even when done with the best motives, it is eminently wise.

We hold, in the first place, that no one has a right to pronounce a disease to be absolutely incurable, and, thereupon, to cease treatment. So long as the patient gives the practitioner his confidence, so long is it his duty to attempt every means for the cure that his reading, experience, or meditation may suggest; taking care that the patient be made no worse, nor his family nor fortune injured. It may be, and we believe it often is, that the patient knows the general opinion as to the incurability of the disease from which he suffers, and yet he hopes for a cure in his own case, because he has selected, as he believes, an eminently skilful, experienced, and acute practitioner, from whom much may reasonably be expected. Besides, just as men think all men mortal but themselves, so the incurables think all like cases incurable except their own. Now, as we do not remind men of their inevitable mortalness, so we ought not to remind incurables of their inevitable incurableness.

M. Simon criticises at length the opinions of certain writers (amongst whom is Gideon Harvey, physician to William III) who have advocated systematic deception and chicane in the practice of medicine. Harvey's book was a refined satire on the therapeutics of his day. He knew what *all* men of ordinary penetration now know, that the conflicting theories and modes of treatment adopted in the profession have nearly similar results; that Nature cures; and that the triumphant pæans of the pretender are raised for a victory not his own. The use of placebos has been legitimate from time immemorial. They are often necessary to save the patient from himself. The hypochondriac is morally insane, and must be treated by the soothing system so successful in other forms of mental derangement. M. Simon thinks that we ought to reason with patients of this kind, an observation, we may remark, rather indicative of the inexperienced practitioner—inexperienced at least in this class of cases. The fact is, we might as well reason with lunatics; hypochondriacs, however experienced in extravagant medication, never learn wisdom; actually knowing the evils that drugs inflict upon them, they persist in seeking and swallowing more. In all such cases a disinterested anxiety for the patient's good shonld be the rule for deciding what is right and proper.

We think we need not dwell on the subject of the next chapter—namely, the question what remedial means should be absolutely forbidden. No honorable practitioner will place his art at the service of the worn-out debauché, or minister to the criminal passions or fears of the seducer. It is to be regretted that venereal diseases, and diseases originating in excessive venery, or self-pollution, have fallen into the hands of excommunicate practitioners, and ignorant, unprincipled quacks, because we hold that the principle is bad which estimates the importance of diseases by their origin. All diseases whatever, originate either directly or indirectly, from our natural depravity. Their origin, it is true, may be a matter of taste, and it may be difficult to decide whether it be more honorable to treat the results of unbridled gluttony and drunkenness than of careless fornication; but there can be no doubt as to the principle. All questions of treatment and of hygiene, connected either directly or indirectly with the sexual system, can be dignified by the practitioner. His personal conduct and the dignity of his object will give them a real or a doubtful import-

ance. Certainly all ribald jests should be avoided, as well as all prudery; an honest, manly, business-like firmness should be the rule of conduct: going straight to the object with few words, unless reproof be necessary. When we remember how extensive the relations of the sexual functions are, and how imperious the instinct, and the more imperious when the man is the most inexperienced, there must be a large mass of human suffering that the prudish practitioner never has the opportunity to alleviate. We believe the venereal and obscene quacks gather the richest harvest of any mediciners.

An interesting question arises in considering how far experiments in medication may be carried. M. Simon thinks only the few who are properly qualified by opportunities, previous studies, and experience, should undertake the difficult duty of filling up the chasms in therapeutics. Experimentation is unjustifiable (except in incurable cases) by the ordinary practitioner. He must be content to follow, and not aspire to lead. M. Simon seems to object strongly to the use of arsenical preparations. He would have their use absolutely forbidden. In this point we differ much from M. Simon. Given with due care and attention, much good may be done by them.

We pass rapidly over the remainder of the volume. A chapter on the use of intimidation in treatment and prophylaxis is followed by another on euthanasia, and the treatment of the dying; and this ends the second division of M. Simon's subject. The third division considers the duties of practitioners towards society. The fourth is occupied with the *rights* of the profession, and comprises medical organization, and civic status, privileges, immunities, and rewards. As the questions discussed belong to hygiene and medical polity, and are thus only incidentally a branch of medical ethics, we will here hold our pen. In conclusion, we may honestly say of M. Simon, as Sir Kenelm Digby observes in his critique of Sir Thomas Brown, "His wishes and aims, and what he pointeth out, speak him owner of a noble and generous heart."

ART. X.

The Descriptive and Physiological Anatomy of the Brain, Spinal Cord, and Ganglions, and of their Coverings. Adapted to the Use of Students. By ROBERT BENTLEY TODD, M.D., F.R.S., Professor of Physiology in King's College, London, &c.—*London*, 1845. Small 8vo, pp. 284. With 38 wood-engravings.

THIS little work is a reprint of the article "Nervous centres," in the 'Cyclopædia of Anatomy and Physiology,' with several additions, and some alterations, suggested by its reperusal as it again passed through the press. It owes its appearance, as a separate volume, to the frequent complaints which have reached the author, from students and others, of the want of a description of the Brain and Spinal Cord, embodying the most recent observations on the anatomy of these important organs. These complaints we regard as well-founded, but we are not quite sure that the present reprint is the means best fitted to allay them. We are

confident that there is no part of the human body, which so fully needs all the illustration and aid to be derived from comparative anatomy, as the nervous system requires; and we are sorry, therefore, that any treatise, expressly devoted to it, should exclude the valuable information afforded by the researches, that have been recently made upon other types of structure. As an exposition of the structure of the nervous centres in Man, however, we are disposed to regard it as the most full and accurate that has yet appeared in this country, and to recommend it to the attention of the student accordingly.

Prefixed to the anatomical details, which constitute the bulk of the volume, is a series of "Aphorisms respecting the nervous system," which presents, in a very clear and concise form, the chief truths which have been ascertained in regard to its functions. We must take exception, however, to the language in which some of them are couched, as tending to mislead the reader; though we cannot believe that Dr. Todd's ideas on the subject are really different from our own. "The nervous force," he justly observes, "is a polar force, resembling electricity in the instantaneousness of its development, and in the rapidity of its propagation, but differing from it in several important features. It is a peculiar feature of the nervous force, not only that it may be developed under the influence either of a mental or of a physical stimulus, but that, being excited by a physical stimulus, it is capable of affecting the mind." So far, so good. The nervous force itself is recognized as the same, whether excited by the will acting through the brain, or by an irritation applied to the trunk of the nerve; and this is in accordance with sound philosophy, which forbids us to regard as different the conditions into which the nerve is thrown by these two agencies,—the sole manifestations of those conditions, exhibited through the muscles, being precisely the same. But, continues Dr. Todd:

"14. Hence nervous actions may be conveniently distinguished into two classes, according as the mind participates in them or not; namely, mental and physical.

"15. Mental nervous actions are those which originate in, and are excited by, an act of the mind, as all voluntary actions; or which, originating in a physical impression, produce an affection of the mind, as in all sensations.

"16. Physical nervous actions are those which take place without the *necessary* intervention of the mind, and which result from a physical exciting cause. Of this kind are all those actions which are caused by a physical change, frequently morbid, in a centre or in a nerve, and those which result from the excitation of the vesicular matter in which a motor nerve is implanted, by the stimulation of the peripheral fibres of a contiguous sensitive nerve. These latter have been designated *reflex actions;* and although most of them, in health, are attended with consciousness, still this mental state is in no way necessary to the perfection of the nervous act, and should be regarded rather as an incidental accompaniment to it, than as an essential part of it." (p. xii.)

Now we cannot but regard the terms "mental nervous actions," and "physical nervous actions," as utterly meaningless, or as involving ideas altogether incongruous. If it be admitted that the "polar" condition of the nervous system itself is the same, whether it be excited by a mental or a physical cause, where is the distinction between the actions which are the manifestation of that condition? Should we say of the fall of a

stone to the earth, that it was a case of "mental gravitation," if it were let go by an act of human will, but of "physical gravitation," if it were thrown down by the wind, or detached by a stroke of lightning? The idea that the "polar" condition of the nervous system is itself different,—according as, in the one class of changes, it is destined to produce sensations or to excite reflex actions,—or as, in the other class, it originates in the will or is produced by a physical cause,—appears to us quite unsupported by evidence; and we are satisfied, from the previous aphorisms, that Dr. Todd has no intention of propounding it. What, then, can be the meaning of the terms "mental nervous," and "physical nervous?" The *nervous* action is one thing, the *mental* action another. The nervous action is always physical; that is, it is comparable in its nature (so far as this is known to us), and in the conditions on which this is dependent, with other physical actions. Of the nature of mental action we know nothing, it is completely unlike everything else, "none but *itself* can be *its* parallel." But we know that it is connected, in some way that we cannot comprehend, with the proper physical actions of the nervous system; being stimulated by the latter, when the part that is in action is the one that has the power of exciting sensations in the mind; and in its turn being the stimulus of the latter, when the will, or an emotional or instinctive impulse, is directed to the production of muscular movement. Thus, then, nervous action, itself physical, may be produced or stimulated by *physical* causes, or by *mental* causes; and it may be productive of *physical* or of *mental* changes, according to the part of the nervous system concerned. But such a difference in the *causes* or in the *effects* of the action does not warrant the combination of terms employed by Dr. Todd; the only excuse for which would be looked for in a difference in the action itself, which is not admitted by Dr. Todd any more than by ourselves.

We shall make a few quotations from the anatomical portion of the work, which may give an idea of the clearness and amplitude of its descriptions. After entering at some length into an account of the cranio-spinal fluid, to which particular attention has been drawn by Magendie, Dr. Todd thus continues:

"Practical men are too much in the habit of attributing morbid phenomena of the nervous system to the influence of the pressure of a liquid effusion upon the brain or spinal cord. Many facts tend to show, that, in a large proportion of cases, especially in the adult, the occurrence of an increased quantity of fluid, either around those centres or within the ventricles, is *a result*, and that it is probably a result *of a conservative kind*, consequent upon a morbid change which depresses the general nutrition of those organs themselves. We have seen how the universal decay of the tissues, which characterizes old age, favours the increase of the cranio-spinal liquid, when it affects the brain and spinal cord. In examining the bodies of habitual drunkards, patients who die of delirium tremens, or of cirrhosis of the liver, the quantity of fluid is always found to be considerable, and the brain shrunk. In bed-ridden persons, who have ceased to exercise their faculties for some time, whether for mental or bodily exertion, the same phenomena are witnessed. When there has been much anæmia, as in cases where death has terminated a protracted illness, in phthisis for example, or in persons who have died of hemorrhage, or after excessive venesection, the nervous centres will be found to be small, and the liquid in large quantity. In extreme cases of

lead cachexy, in which the nutrition of the nervous and muscular tissues is materially diminished, I have observed similar appearances. And, when any partial atrophy of either the brain or the spinal cord has occurred, there will invariably be found, at a point corresponding to it on the exterior of the organ, a local accumulation of fluid, occupying a depression on its surface, which has been caused by the giving way of the nervous substance within.

"On the other hand, an increase in the quantity of the nervous substance, or an enlargement of the brain or spinal cord, consequent on an undue injection of their blood-vessels, is invariably accompanied with a diminution in the quantity of this fluid, or with the total absence of it. In hypertrophy of the brain, no fluid is found in the subarachnoid space, and very little or none in the ventricles. In cases of tumour of the brain encroaching upon the cranial cavity, we find no fluid; and the same is observed where chronic inflammation of the brain has given rise to a new deposit, which increases the bulk and the density of the cranial contents. In all cases where a considerable quantity of fluid has accumulated *within* the ventricles, that upon the surface is either greatly diminished or entirely disappeared. In the ordinary hydrocephalus internus of children, fluid is never found on the exterior of the brain.

"When an arrest in the development of any portion of the cerebro-spinal axis has taken place, the space which ought to be occupied by the organ of imperfect growth is filled by liquid. In examining the heads of idiots, we always find a considerable quantity of sub-arachnoid fluid, either general or partial, if a portion only of the brain be deficient." (p. 50.)

We fully agree with Dr. Todd in his general deduction, that the preternatural increase of this fluid should be usually regarded as secondary to the diminished size of the cerebro-spinal centre itself, or to the diminished quantity of blood in the vessels, and that it has little or no direct connexion with the manifestation of peculiar symptoms during life, in the great majority of instances. The complete investment of the encephalon by an unyielding bony case, obviously places it in a different condition from any other organ, since any diminution in its bulk must leave a vacuum (which Nature, *here* at least, seems to abhor), if the space be not filled up with fluid. But it would be absurd to deny that the cerebro-spinal fluid occasionally undergoes a preternatural increase, whilst the brain experiences no diminution in bulk, or itself becomes enlarged. This increase, however, rarely takes place in the sub-arachnoid fluid of the exterior of the brain, being much more common in the fluid within the lateral ventricles; the walls of which acquire, after they have been long subjected to its pressure, a preternatural degree of firmness, so as not to collapse when laid open. In these views Dr. Todd accords with the opinions first brought forward by the late Dr. Sims, in his valuable paper on serous effusion in the brain.

Dr. Todd has minutely examined the structure of the Pacchionian bodies, and considers them as morbid structures originating in a deposit of lymph among the vessels of the pia mater, which pushes the arachnoid before it as an investing sac. In some instances the granular mass is only partially covered, and then it causes only a slight projection on the surface of the visceral layer of arachnoid; but in others the mass is completely covered, and a stalk is gradually formed; and when several granular masses have been deposited immediately contiguous to each other, they may all be attached by a cluster to the same stem. The membranous sacs of some

of the bodies have epithelial particles upon their surface, whereby the derivation of these sacs from the arachnoid membrane is made apparent.

"The great frequency with which these bodies are met with in the various situations above mentioned, has induced many, even in the present day, to regard them as normal structures, the physiological office of which is as yet unknown. But there are many facts which strongly militate against such a conclusion. In the first place it may be observed, that the Pacchionian bodies never occur in the earliest periods of life. In the course of a long experience in anatomical investigations, I have never seen them at a period antecedent to six years. The brothers Wenzel, who made a series of special examinations, with a view to determine this question, make the following statement:—In children, from birth to the third year, these bodies, if they ever occur, must be very few. From the seventh to the twentieth year, they sometimes are numerous. From the latter period to the fortieth year, their number is considerable, and the nearer we approach the fortieth year, the greater does it become. Lastly, from the fortieth to the one hundredth year, these bodies are found in great numbers.

"It must further be remarked, that even at those periods of life when the Pacchionian bodies are found in greatest numbers, cases frequently occur in which no trace of them can be found. There is likewise the greatest variety, as to their number and size, in different individuals of the same age. It has always occurred to me to find them most numerous in cases, where I had reason to know that the brain had been subject to frequent excitement during life. In persons addicted to the excessive use of spirituous liquors, in those of irritable temperament, and who had frequently been a prey to violent and exciting passions, they are almost uniformly highly developed. The Pacchionian bodies are peculiar to the human subject, nothing similar to them has been found in any of the inferior classes of animals.

"In reference, then, to the question, what is the nature of these bodies, I have no difficulty in stating my opinion that the evidence greatly preponderates in favour of their morbid origin: that they are the product of a chronic, very gradual, irritation, due to more or less frequent functional excitement of the brain itself. It is not unlikely that the friction to which the opposed surfaces of the arachnoid are continually subjected in the movements of the brain, especially when they are of a more rapid and violent kind, as under states of cerebral excitement, may contribute to the development of many of the appearances connected with these bodies. The opaque spots, which are of such frequent occurrence upon the surface of the heart, may be quoted as an example of a morbid change, very commonly met with, and resulting probably from the friction against each other of opposed serous surfaces. Were the Pacchionian bodies normal structures, they would not be so frequently absent from brains which afforded every other indication of being in a healthy state; nor should we find opacity of the arachnoid (a decidedly unhealthy condition) so commonly coexistent with the full development of them. Again, were they a necessary part of the healthy organism, we might expect to find them more constant as regards size, number, and the extent of surface over which they were placed." (p. 60.)

This last argument is, perhaps, the weakest; since we *do* find an almost equal variation in the number and situation of the curious *Pacinian* corpuscles; which can hardly be regarded in the light of morbid structures.

The description of the vesicular nervous matter, which is peculiarly full and lucid, closes with the following well-expressed queries:

"The great simplicity in the form of the elements of the gray nervous matter is one of its most remarkable characteristics. That a tissue, which plays the

prominent part in the nervous actions, whether they are prompted by mental change or are purely corporeal, should exhibit scarcely any more complexity of structure than that which is found in the simplest animal or vegetable textures, or in structures that have not passed their earliest phase of development, is an anatomical fact pregnant with great physiological interest. Have this simplicity of form and delicacy of structure reference to the celerity of the nervous actions? or to that proneness to change which must be induced by the constant and unceasing round of impressions, which the gray matter must receive from the ordinary nutrient actions that are going on in the body, as well as from the continual action of thoughts? If, according to common acceptation, we admit that the mind has some immediate connexion with the cerebral convolutions, it may well be imagined that no part of the frame can be the seat of such active change, from its being, on the one hand, the recipient of impressions from the body, and, on the other, from an association with the psychical principle so intimate that probably, under ordinary circumstances, an affection of the one cannot occur without being communicated to, and producing a change in the other.

"Another curious fact, in connexion with the intimate structure of the gray nervous matter, is the large quantity of pigment or colouring matter which exists in it, and which appears to form one of its essential constituents, more abundant in some situations than in others, but present in all. We are utterly ignorant of the design of this peculiarity of structure. If this pigment bear any resemblance in chemical composition to the colouring matter of the blood, *hæmatosine*,—and it is not improbable that it does,—an increased interest attaches to the practical importance of minute attention, on the part of practitioners, to avail themselves of all the means which are capable of improving that important element of the nutrient fluid both in quantity and quality; for it is most reasonable to presume that the pigment of the nervous matter would derive its nutriment from that of the blood." (p. 72.)

In regard to the connexion between the nutrition and waste of the vesicular matter of the nervous system, on the one hand, and the degree of its vital activity, on the other, our own mind has long been completely made up; and we have, on several former occasions, expressed our conviction, with some of the reasons on which it is founded, in the pages of this Journal. The idea started by Dr. Todd, in regard to the relation between the pigment-granules, which seem to form an essential constituent of the cells of the gray matter, and the hæmatosine, which is contained within the red corpuscles, and which is manufactured (so to speak) by them, appears to us to be one of peculiar interest; and it derives increased weight from the disturbance of the functions of the nervous system, which invariably manifests itself, when there is any considerable deficiency in the proportion of red corpuscles in the blood.

By way of sample of the descriptions of the structure of the brain itself we may quote the following passages relative to the Mesocephale; a term which may be new to some of our readers, but which was first suggested by Chaussier, as applicable to that part of the encephalic mass, in which the fibres of the different parts come into close relation.

"The pyramidal and olivary columns may be readily traced, as already explained, from the medulla oblongata up to the cerebral hemispheres, the former becoming united chiefly with the corpora striata, the latter with the optic thalami. In that part of their course which is intermediate to the medulla oblongata, these columns become mingled with certain transverse fibres, and with more or less of vesicular matter, and with them contribute to form a mass which is the connecting link between all the segments of the cerebellum, and

may be compared to a railroad station, at which several lines meet and cross each other." (p. 180.)

We think that this simile may be carried a little further, since the mesocephale is not only a point for the intercommunication of different tracks, but is also a centre from which *new* trains may start, in virtue of the vesicular matter which forms a part of it. We are constrained to notice that the preceding extract does not afford a very favorable sample of Dr. Todd's style. We are at a loss to attach any distinct idea to the words, "intermediate to the medulla oblongata;" we know that one thing may be intermediate between *two* others, but to say that it can be intermediate to *one* other, is surely rather Irish than English. Again, we cannot but think that Dr. Todd meant to say that the mesocephalic mass is the connecting link between the segments, not of the cerebellum alone, but of the encephalon generally. We have noticed more than one important error of this description, which is obvious enough to the well-informed reader, but which is calculated to mislead the student. For example, in the description of the corpus callosum (p. 234-5), we find that, by dissection in a hardened brain, "it may be detached from the substance of the hemispheres as far outwards as the external border of the corpus *callosum* and optic thalamus." It is evidently the corpus *striatum* which is referred to in the last clause.—To return, however, to the mesocephale. After a full description of the several parts that enter into its formation, Dr. Todd continues:

"From the preceding description of the mesocephale, it may be concluded that two classes of elements enter into its formation. These are *intrinsic* and *extrinsic*. The former consists of the masses of vesicular matter, with which the fibrous matter, whatever be its course, is intimately connected. Such are the gray matter of the quadrigeminal bodies, that light gray matter which surrounds the olivary columns in their upward course, the darker matter which intervenes between the transverse fibrous lamellæ, and, more in front, that which forms the locus niger of the crus cerebri.

"The extrinsic elements are those which pass through this segment, being continuous with some portion of a neighbouring segment, or serving to connect the gray matter of the mesocephale with the hemispheres of the cerebrum or cerebellum, or with the medulla oblongata. The fibres which form the inferior layer of the pons are perhaps the only element that does not connect itself in some way with the gray matter of the mesocephale, since they seem simply to pass across from one crus cerebelli to the other. The deeper transverse fibres, the pyramids, the olivary columns, the processus cerebelli ad testes, all connect the neighbouring parts with the intrinsic matter of the mesocephale.

"It is plain, then, that anatomy affords abundant grounds for the conclusion that the mesocephale must be regarded as a distinct centre, connected by numerous bonds of union to the other segments of the brain." (p. 187.)

Correct as this description is in regard to the human brain, when considered independently, we still think it highly objectionable to include the corpora quadrigemina as a part of the mesocephale; since we learn from comparative anatomy that their analogues in the lower animals are as distinct from all the other divisions of the encephalon, as these are from each other. In regard to the Olfactory lobes, Dr. Todd fully develops the correct view, and justly remarks that it is high time for the designation of olfactory *nerves* to cease, as applied to those processes of cerebral

matter, which lie upon the upper surface of the cribriform plate of the ethmoid bone. He points out their distinctly ganglionic character, as evinced by the intermixture of vesicular with fibrous structure; and he notices the ventricle contained in the olfactory bulb, which is evident enough in the lower mammals, as well as in the human fœtus at an early period. Why, then, if it be desirable to impress on the student correct notions regarding the nature of this body, does he include it among the encephalic nerves? Surely, in a scientific description of the brain, it ought to occupy as conspicuous a place as the mesocephale and the optic ganglia. And, we would add, is not the gray matter of the olivary columns really to be considered as forming auditory ganglia, connected as those columns are with the implantation of the auditory nerve?

We have looked with especial interest at those portions of the Treatise which relate to connexion between the spinal cord and its nerves; a question of particular importance, in regard to the physiological inferences to be derived from anatomical research, and one upon which there is still much discrepancy of opinion. Many of our readers doubtless recollect the time, when the spinal cord was commonly taught to be little else than a bundle of nerves proceeding from the brain, and when it was supposed that *all* the fibres which enter it from the nerve-roots pass upwards to the latter organ. This always seemed to us to be Sir C. Bell's view of its nature. When Mr. Grainger first enunciated the fact, that *some* of the fibres of each root lose themselves in the gray matter of the cord, his statement was received with suspicion. Yet now this seems abundantly confirmed; and with some anatomists the tide of opinion sets just the other way, their question being, whether *all* the fibres of the roots of the spinal nerves are not thus lost in its gray matter, and whether *any* have a direct connexion with the brain. Dr. Todd states that he is satisfied, by his own dissections, of Mr. Grainger's accuracy, in regard to the passage of certain fibres of the roots of the nerves into the *gray* matter of the cord; but he seems very doubtful as to the possibility of tracing the continuity of any of the nerve-roots with the *white* strands, as represented by Mr. Grainger, and consequently as to the existence of a distinct set of cerebral fibres. Of the former he says:

"From a careful review of the preceding statements, it is plain that a large number of fibres pass into the gray matter of the cord, and probably form some intimate connexion with its minute elements; and this fact is favorable to the supposition that the spinal nerves derive their origin, at least partly, from the gray matter. It must be admitted either that these fibres unite in some way with the vesicles of the spinal gray matter, or that they pass through it up to the brain, in the gray matter of which they become implanted; the former seems the most reasonable supposition, and more consistent with the apparent oblique or transverse direction which the fibres take in the gray matter." (p. 273.)

We may then regard the title of the spinal cord to the character of a distinct ganglionic centre, to be anatomically as well as physiologically proved. For a solution of the difficulty under which Dr. Todd labours, in regard to the connexion between the roots of the spinal nerves and the brain, through the white or fibrous portion of the cord, we would again refer him and other anatomists who experience the same difficulty,

to the ventral cord of the articulata, which differs in no other obvious particular from the spinal cord of vertebrata, than in the want of continuity of the ganglionic portion, so that a number of distinct centres are formed by it, instead of one long tract; and this is in strict accordance with the general character of the group, which is that of segmental independence. We cannot, from our own investigations, feel the least doubt that a portion of the roots of the nerves of those animals is directly continuous, through the fibrous tract, with the cephalic ganglia, which represent the brain of vertebrata, whilst another portion has a distinct connexion with the ganglia of the cord. The fact is the more valuable since it was first announced by Mr. Newport, without any reference to the physiological inferences which are *now* founded upon it.

We now take our leave of this Treatise; cordially recommending it to the student as being, in spite of its defects, by far the best guide to the study of the Anatomy of the Human Brain which we possess. When it arrives at another edition, we would suggest to Dr. Todd whether it might not be advantageously increased to a small extent, by the introduction of a little more comparative anatomy,—not in a systematic form, but in such a shape as to illustrate some of the obscurities, which are inseparable from the isolated study of the human brain, and which are still numerous enough, when all the light has been thrown upon it that can be collected from extraneous sources. We may hint, also, that the wood engravings, some of them very elaborate ones, which add much to the value of the descriptions, would be rendered more useful, as well as more pleasing to the eye, by a higher style of presswork. It is lamentable to see so many good drawings, which have all the appearance of being carefully engraved, treated with such injustice by the printer.

Art. XI.

Vermischte Abhandlungen aus dem Gebiete der Heilkunde, von einer Gesellschaft practischer Aerzte zu St. Petersburg. Sechste Sammlung. —*St. Petersburg,* 1842.

Medical Essays. By a Society of Physicians in St. Petersburg. Volume the Sixth.—*St. Petersburg,* 1842. 8vo, pp. 396.

After a period of seven years another volume of Transactions has been published, by the Association of German Physicians, in St. Petersburg. The editors claim the indulgence of the profession for this long delay, on the plea that the printing of German works is encompassed with so many difficulties in the Russian capital, and, also, that unremitting professional labours left to the physician scarce any time for the scientific elucidation of his practice.

The volume before us is the sixth of the series, and without imparting anything very new or extraordinary in medicine, it may yet be considered valuable, as affording a fair criterion of the state of the healing art in St. Petersburg.

I. *Statistics and Treatment of Insanity at St. Petersburg;* by Dr. Herzog. This is an interesting account of the Asylum for the

Insane in the Russian metropolis. The treatment, and the general management of the unfortunate inmates, appear to be conducted on the most humane and scientific principles. From the ample tables given at the commencement it appears that here, as elsewhere, the bachelor is more liable to insanity than the married man; while, at the same time, the proportion of the insane is much greater in the higher and in the educated classes of society, than among shopkeepers and the artisans and labourers of a still lower grade. More than one half of the cases proved incurable. Of those who recovered, the great majority belonged to the lower classes, and complete returns to health were especially frequent among those individuals who, at an early period of their disorder, had disturbed the public peace and had been in consequence transferred to the asylum. The richer classes, of course, refrain as long as possible from placing their friends and relatives in such institutions, and continue to hope for a cure under imperfect treatment at home, till recovery becomes almost impossible. A full and lengthy account is given of the entire management of the institution, and we here extract a few lines as to the employment of the insane.

"The female lunatics are chiefly busied in the household duties, while the males, during the summer months, are busily occupied with the hay harvest, and in winter, when the severity of the season confines them to the house, they manufacture thousands of pill-boxes, and articles in pasteboard for the supply of the shops of the apothecaries. The more educated are occasionally employed in illuminating manuscripts, and some write to the dictation of others."

In the second portion of this paper, entitled, "Considerations on the treatment of the insane," Dr. Herzog develops his own views of mental disease, and we must allow that they are expressed with that candour and consideration for the opinions of others which is characteristic of a truly scientific mind. With most writers of the present day, Dr. Herzog denies that congestion or inflammation of the brain is necessarily present in insanity, and, if it should occur, he looks upon it rather as a complication and consequence of the malady than as its original cause. Nor does he allow of any close connexion between the suppression of hemorrhoidal or menstrual discharges and the outbreak of insanity. He enumerates the different causes of mental disease, as follows:

1. A certain hitherto unexplained condition of the nervous system characterized by great indifference to external agencies, as to heat, cold, &c. &c., and which cannot be traced to disorder of any particular organ. A similar state of the nervous system is often observed in constitutions worn out by drunkenness or by debauchery.

2. The brain, as the centre of the nervous system, must necessarily be looked upon as intimately connected with insanity, yet inflammations of this organ are not frequent at the commencement of mania, nor are we to regard the supervention of strong pulsations in the carotids, headache, brilliancy of the eyes, or sleeplessness, as indicating inflammatory action at a later period. But Dr. Herzog does not deny that secondary inflammation may arise in the brain, as a consequence of pathological changes which have taken place in that organ, through the influence of maniacal excitement. We must confess that the distinction here drawn is open to objec-

tion, and that we find here, as everywhere, the difficulty of defining the precise limits between irritation and inflammation.

3. Disorders of the digestive organs certainly exercise a very important influence on all cases of hypochondriasis and melancholia; the mental affection is constantly connected by the patients themselves with abdominal derangement, but no such relationship can be traced in other forms of insanity.

4. Diseases of the sexual system undoubtedly occasion certain varieties of mental disorder, and the most prominent of these is nymphomania. To this class also Dr. Herzog refers the melancholy cases arising from unrequited or restrained affection (Wahnsinn aus Liebe), which are but too frequent among females of the purest life and manners.

5. The sudden suppression of the lacteal secretion has occasionally a similar effect, though the disorder so well known as puerperal mania may occur quite independent of such causes. Dr. Herzog justly cautions us against interfering too vigorously with the numerous local maladies which may arise in the course of insanity, though he repeats that the cerebral irritation, so constantly accompanying the disease, may sooner or later occasion organic changes in the brain, and should, therefore, be combated by antiphlogistics. Still, he thinks, there is greater danger to be feared from excessive depletion than from over-caution in the use of the lancet, and the tact which must guide our hand in this respect can only be acquired by long experience.

The principal diseases which ensue as complications of insanity, are spasmodic and paralytic disorders, and derangement of the organs of digestion. Dr. Herzog insists strongly upon the danger of confining our practice to the routine treatment of these maladies, and affirms that those remedies alone will be successful which are directed against the original disease. In this assertion we need hardly say we cannot entirely concur, nor is it made so positively as altogether to exclude these secondary disorders from our consideration. In treating of the use of medicines in disorders of the mind, Dr. Herzog divides them into two classes, viz.:

A. Those which directly act upon the nervous system, as intoxicating and narcotic drugs, the employment of which, however, is greatly limited by the cerebral irritation which so often accompanies these maladies. Still in those cases of insanity, and they are by no means so rare as has been asserted, where no cerebral irritation exists, opium in the hands of medical men, and alcoholic liquors administered by empirics, have been found of the greatest benefit. This latter mode of treatment the author states to have been far too much neglected by the profession. The evil consequences that may have occasionally ensued from its employment, would rather indicate that it requires to be directed by a skilful hand than to be entirely thrown aside.

B. The second class of medicines are those which act directly upon the intestinal canal, or upon the external skin, as purgatives, emetics, and warm baths. Our author has seen but little benefit derived from counter-irritants, as issues, or the tartar emetic ointment, although the frequent outbreak of the malady on the suppression of accustomed discharges would seem to indicate their employment.

To direct our external means so as to influence the disordered *mind* of the patients is then the chief indication in mental disease. We must endeavour, in the first place, to act upon their feelings and sensibilities, without addressing ourselves solely to their understandings; but at the same time it should be remembered that the moral treatment is not always to be directed according to the former character of the individual, as this may, and usually does, undergo a total change at the very commencement of the malady. The habits, mental and physical, of early life continue, however, for a long period in the insane, and it is by means of these that we can hope to obtain the greatest influence over the patient. The agencies and circumstances which attract the attention, without unduly exciting the mind of the insane, are the only ones which should be employed in mental disease, where everything should be avoided which might confuse or give a shock to their reasoning or moral faculties.

In certain cases our author has found it necessary to separate some patients from others, and in rarer instances he has had recourse to complete isolation—a mode of treatment, however, which can with difficulty be completely carried out in large lunatic asylums.

All harsh language or reproaches towards patients are useless, if not injurious; and yet, as Dr. Herzog very justly observes, quite as much injury may be done by the opposite extreme of excessive tenderness and sentimentality, which is equally if not more foreign to the altered feelings and sensibilities of the patient.

To attempt a cure by reasoning with a patient upon the folly of his actions, or by opposing in strenuous argument the fixed ideas which constitute his delusion, will only aggravate the disorder. Dr. Herzog here adds some excellent observations upon the degrees in which we should yield to the patient's wishes and desires for luxuries, solitude, study, or amusements. Much here depends upon his previous habits, as also upon the question whether these desires are the mere offspring of a disordered imagination, or the consequence of the previous circumstances of the individual. "A maniac who when in health was accustomed to walk alone, may justly be allowed to pursue the same course when diseased; nor should we consider it as a favorable symptom if he were willingly to join the crowd of patients in their daily promenades." The author warns us carefully against forcing work, manual or mental, on the insane when they exhibit no disposition thereto; and he believes that employment is chiefly profitable when they begin to show symptoms of returning health.

Recreation. In the asylum at St. Petersburg rich and poor are, by a government ordinance, treated alike; and this produces the most injurious consequences on the wealthier classes of the insane. These ought to be surrounded as much as possible with all the comforts and conveniences of their homes; and Dr. Herzog calls earnestly for an amelioration which has already been partially effected in this country and on the continent, viz., to exclude as much as possible all semblance of confinement within the walls of a prison, such as our lunatic asylums formerly were. Music and painting have been much extolled as recreations in mental disorders; but it is curious that they have never produced any beneficial effects—at least this is asserted by Dr. Herzog; and we may perhaps ascribe a good deal of this bad success to the over-excitement produced by such pursuits, and

also from a greater degree of freedom being required for their due cultivation and influencing of the mind, than can be allowed in a large public institution.

The author next discusses the long agitated question of the advantage of change of scene and of position to insane patients; and he concludes that for restless and irritable individuals no alteration should be made, while in the milder description of cases it is decidedly beneficial.

Of religious exercises our author speaks very cautiously. No good, he thinks, can result from forcing religious contemplations or prayers upon the insane; but the favorable impression produced upon some by the solemn ceremonies of the Russian and Catholic churches is unquestionable. Of all modes of treating an insane patient, that of placing them in a private establishment of their own in the country is to be preferred; but from the great expense this necessarily entails, such an advantage can fall to the lot of a very few. The very fact of being consigned to an asylum must always be injurious to those who have any feelings of dignity or self-respect remaining. In many well-conducted asylums considerable attention is now paid to separating the slightly affected or the convalescent from the furious maniac or the idiot; but much yet remains to be accomplished before this most necessary separation can be completed in a satisfactory manner. Dr. Herzog suggests that to each asylum there should be attached a house of recovery, to which convalescents could be transferred; and as the number of those likely to recover, according to all writers, cannot be carried higher than the fourth or fifth part of the inmates of asylums, so the additional expense and maintenance of such a house of recovery would be proportionately small.

Our analysis of Dr. Herzog's essay, which occupies eighty pages of the original volume, has been somewhat extended; but we offer no apology to our readers for laying before them the results of many years of careful observation, by one eminently well calculated for the management of the insane. Dr. Herzog's essay exhibits a sound judgment, and a healthy moral feeling runs through the whole, which assures us that the amelioration he suggests will be beneficial to this, the most unfortunate, class of our fellow-creatures.

II. *Cases of Insanity from Wounds of the Head;* by Dr. Herzog. These seven cases are in themselves by no means curious, being all merely slight sabre-cuts or gun-shot wounds of the face or head; and in one case there was a slight fracture of the cranium, while in the last instance the disease ensued after the operation for strabismus. Four of the patients died, and in three an examination of the head was obtained, but *no morbid appearances could be discovered.* From these somewhat imperfect data Dr. Herzog has drawn some singular conclusions. "Medical men in general," says he, "would assert that the blows and contusions received by these individuals, though not severe enough to cause immediate violent action on the nervous system, yet might produce that commotion of the cerebral functions, which, though suppressed for a while by the vis medicatrix naturæ, would sooner or later manifest its destructive ravages." Against this opinion Dr. Herzog urges the following arguments: 1st. That no organic lesion, as a consequence of the injuries received, months or

years before, could be discovered on dissection. 2. That in three or four of the cases no possible injury or concussion of the brain could have taken place, [?] as the wounds were not of a nature to effect this, and yet, nevertheless, insanity ensued.

To this partial reasoning we object that most of the cases occurred in old or invalided soldiers, who, from the want of employment and from their idle and dissipated habits, are peculiarly liable to insanity. Nor does it naturally follow that the mental disease would not have been developed in these individuals without any previous injury having occurred. Moreover, six out of seven of the patients above referred to had been wounded in the head; and though the majority were merely flesh wounds, still the nervous centres may have been more contused and disturbed than we are aware of. In all the seven cases convulsions and symptoms of paralysis occurred at an earlier or later period, along with the mental affection. Dr. Herzog then asks, "May not superficial injuries of the head sooner or later induce serious alterations in the nervous system? and may not the germ of the malady be formed in the wound, and after a long period of time develop itself in connexion with the nervous system?" Dr. Herzog asserts that insanity rarely occurs after *very severe* wounds of the head. In short, Dr. Herzog would make us believe that slight injuries of the cranium and of the nervous centres may, as in the case of the bite of a rabid animal, leave in the wound an undeveloped germ of future disease, from which at a later period may arise insanity, as in the other case hydrophobia. We cannot coincide entirely with his views, but the occurrence of mental disease after slight wounds is well worthy of the investigation of pathologists. It would be interesting to ascertain exactly the proportion of individuals thus secondarily affected. We believe that they would be found very few in number, and not enough to alter our suspicion that insanity might have occurred in the cases before us, had no wounds at all been received.

III. This article contains details of five *cases of sudden death*, by the veteran Professor Busch. It is, however, manifestly imperfect, as no post-mortem examinations of the cases are recorded; nor is it mentioned that they were sought for. We leave them, therefore, unnoticed.

IV. This article is from the same author, and contains details and observations on two remarkable cases which terminated fatally. We must, however, here, as well as in the previous instance, regret the scantiness of pathological details; for, in the first place, the head alone was opened, and in the second no post-mortem examination was made at all, though, from the details of the symptoms in both instances, the stomach was at least as probably the seat of disease as the cerebral system. Both patients were young females of the higher classes, and in each the approach of the menstrual period, though the discharge had never yet appeared, was indicated by the usual premonitory signs. The patients had retired to rest in good health, and awoke about 3 a.m. with vomiting, which after continuing incessantly for some hours, was succeeded by furious delirium. The usual revulsives and counter-irritants were employed, and the symptoms of excitement subsided, but were succeeded by a state of

quiescence, which was occasionally interrupted by general convulsions until death occurred on the third day. Slight congestion of the blood-vessels of the brain was found upon dissection. In some respects these attacks resembled the epidemic cholera, but many of the characteristic symptoms, such as the rice-water evacuations, and the blue, pinched appearance of the countenance and of the extremities, were wanting here. Professor Busch's conclusions, drawn from the symptoms alone, are by no means satisfactory, and he believes that life was extinguished by the vital energies of the ganglionic and cerebral system becoming totally exhausted from repeated vomiting. The cause of the malady he does not however satisfactorily elucidate.

V. and VI. These two articles are also by the same author, but are devoid of interest. The former is that of a brother and sister whose maladies were almost identical, and in the latter we find the details of a case of insanity, which appears to have resulted from a venereal affection.

VII. *Report of the Imperial Foundling Hospital of St. Petersburg, from* 1834 *to* 1840; by Dr. Philipp Doepp. Much improvement has recently been effected in the condition of the foundlings of the Russian capital. The new Lazareth, as it is termed, for the older female infants contains 160 beds, and is a building of two stories in height. A similar edifice has recently been erected for male foundlings, and the utmost care has been taken to render this great institution in every way serviceable to the end for which it was designed. The organization of this hospital seems to be nowise inferior to its external appearance. The beds of the foundlings and their nurses are all placed against the transverse or partition walls of the wards, so as to avoid all danger of cold, or currents of air from the neigbourhood of the windows. So ample and complete are its accommodations that many parents have sought and obtained admission for children born in wedlock, thus permitting their legitimate offspring to be stamped with the brand of bastardy, for the selfish end of procuring them an excellent gratuitous education. By this abuse the number of foundlings became alarmingly increased, till an ukase of the Emperor, bearing date of the 25th of June, 1837, ordained that all children reared in the foundling hospital should, on attaining the requisite age, be apprenticed to artisans or to peasants, or if they themselves particularly desired it, should be enlisted as common soldiers. The effect of this prudent though somewhat harsh measure, was that 860 children were withdrawn by their parents, and 324 others were taken away during the succeeding years. By a further order of the 15th of May, 1839, the custom of delivering a ticket of reception (Empfangschein) to the parents was abolished, so that from 1840 the institution has been restricted to its original intention, viz., that of affording an asylum to illegitimate children *really* deserted by their parents. This latter ordinance has diminished the number of foundlings by one fifth.

Dr. Doepp has found that the observations upon diseases occurring in this institution, and which were published in the preceding volume of these transactions, have been fully corroborated by his seven years' additional experience.

1. Diseases among the older children of the foundling hospital:

Morbilli sine catarrho, or false measles. An epidemic of this kind was observed in 1836 and again in 1839. The eruption appeared exactly like the regular disease, but without any febrile or catarrhal symptoms, and it vanished from the surface in about four days. Above 100 children were thus affected in the institution, and the epidemic was observed also in many cases in the capital. Many instances of this kind also occurred during the great epidemic of genuine measles in 1839, when many children were attacked with the regular disease, who had previously suffered from the non-catarrhal form above referred to.

Genuine measles. This epidemic was one of the most severe and malignant that Dr. Doepp had ever witnessed. The symptoms were at first inflammatory, and became subsequently nervous. Of the children in the institution, 202 were attacked, but only three died, one of *phthisis florida,* and the other two of pericarditis. The peculiar odour, resembling that of goose feathers, first noticed by Heim, was not detected in these cases, even though so many affected individuals were congregated in one apartment. Belladonna and sulphur were found perfectly inert as preservatives.

Scarlatina. Only *two* cases have occurred in the foundling hospital during the last seven years, and one of these proved fatal. Dr. Doepp cannot well account for the freedom of the institution from this dangerous malady, during a period when it has been repeatedly epidemic and extraordinarily fatal in the city of St. Petersburg.

Scrofula. Dr. Doepp has latterly used, with very excellent results, the cod-liver oil in scrofulous affections of even very young children. Nor was he deterred from its employment by diarrhea, or mesenteric disease: in both cases a favorable termination generally ensued. He is also a firm advocate of iodine, but insists much upon combining its employment with free exercise in the open air.

The mortality in this division of the institution is extremely small, scarcely exceeding one per cent. during the last six years. Tubercular phthisis and chronic diseases of the intestines were the chief disorders that terminated fatally.

2. Diseases occurring in new-born infants:

Ophthalmia neonatorum. Dr. Doepp does not believe a bright and intense light to be a cause of this disease, as those children that lay nearest to the windows were not more affected than others. It never became epidemic in the hospital, and was certainly, in most cases, occasioned by cold.

Erysipelas neonatorum. This is still a frequent disease, especially after vaccination. Dr. Doepp states expressly that he has found erysipelas most frequent in infants when they had been vaccinated with lymph taken from a highly inflamed pustule or vesicle. And he has been able to affirm this with greater certainty from the results of the practice he has long followed, of vaccinating each child in both arms, but with different lymph, and he constantly produced erysipelas in that arm which had been vaccinated with lymph from a vesicle highly inflamed. The erysipelas was best cured by free and repeated scarification.

The epidermis, or its succeeding scab, is occasionally torn off from the

pustules of vaccination, and the latter are then very prone to change into erysipelatous ulcers, from whence there flows a quantity of an acrid secretion. Dr. Doepp's practice, in such cases, is to fill the excavation with finely pulverized flores zinci, which forms an artificial crust, and can be easily renewed by the same process, if torn off before the part beneath has become completely cicatrized. But at times these ulcerations will not heal at all, and continue obstinately to spread, when we should probably be justified in considering them of a syphilitic origin, and that more unequivocal marks of that disease will speedily appear.

Syphilis neonatorum. Richter, Henke, Jörg, and others have denied the existence of this disease, but the experience of Dr. Doepp has convinced him that it is by no means uncommon. Several cases of true syphilitic eruption appeared in various infants very soon after birth, and yet the mothers, to all appearance, were quite free from any venereal taint. In such cases Dr. Doepp suspects, with Dr. Evanson and many other writers, that the infection has been derived from the male parent. Small doses of mercury were most useful in effecting a cure, but no benefit was obtained thereby if the child was taken from the nurse's breast. Sooner or later the nurses themselves became affected with syphilitic sores and eruptions, which required for their cure a vigorous course of corrosive sublimate. The non-mercurial plan of treatment totally failed.

Cephalæmatoma neonatorum. In a private practice extending over the long period of twenty-six years, Dr. Doepp has observed this remarkable affection only thrice, while in the foundling hospital, during the space of eleven years, it has occurred in not less than 262 cases, and in all, excepting two, the parietal bones, near their superior portion, were the seat of the disease. The average amount of cases was 1 in 190 of the whole children in the hospital: 11 post-mortem examinations were made, including 8 who died of other disorders when thus affected. The results of the dissections were the following:

a. In none of the 11 cases was any perforation of the skull observed, nor any effusion of blood upon the inner surface thereof.

b. All the 262 patients were brought to the hospital with the affection already existing, though many were not above a few hours old. But the tumour was often observed to increase in size after the arrival of the children at the hospital, at least during the first three days, after which no further enlargement took place, except in those cases where the tumour having been opened had again become distended.

c. It was only in nine cases of the whole number that Dr. Doepp could ascertain the nature of the labour. All the nine had been easy and every way natural, and the mothers were strong, healthy, and not overburdened with fat. Three were primiparæ; the previous children of the other eight had been perfectly free from any signs of this affection.

d. The cephalæmatoma seemed to be always beneath and separate from the scalp; it was a *tense* fluctuating tumour; the integuments were of normal colour, and could, even in the most distended condition of the swelling, be freely moved backwards and forwards over its surface. The pulsation noticed by Naegele and others was never observed, nor did pressure on the tumour seem to cause pain to the infant. Its form was usually oval, but was sometimes altered by two or three tumours being placed

in close apposition. The largest tumour that was observed in the hospital measured about four inches by three, and several were seen not larger than a hazel nut.

e. The hard ring around the tumour was never wanting, though at an early period of the affection it was less observable than afterwards. It was *most distinct* whenever the tumour was emptied by incision, and gradually disappeared then in about five days' time. Dr. Doepp carefully dissected two cases for the express purpose of ascertaining the true nature of this callous ring. After removing the external integuments, he found that the tumour was formed by the pericranium distended with blood. Upon incising the swelling and washing out the contents, he observed the callous ring to consist *solely* of a layer of coagulated blood, which formed all round at the point where the pericranium had been upraised from the bone. By a continuous stream of water this layer could be dissolved away, and with it the callous ring entirely disappeared. The pericranium was everywhere healthy, as was also the bone beneath. No appearances of inflammation could be detected in either.

Dr. Doepp does not, however, deny that, in certain cases when the tumour has been opened, and much suppuration has ensued, the bone may become carious, and thereby the exterior table of the skull with the diplöe may be destroyed. In such instances the circumference of the carious portion would necessarily present a sharp unyielding edge. In one skull in Dr. Doepp's possession the absorption of the diplöe appears to have commenced first, and the external table over the absorbed portion was depressed in the centre, but elevated at its circumference, so as to form a callous ring of true bone. The termination of this affection was almost always favorable. Two children died out of 265, and one of these from violent phrenitis, perhaps occasioned by cauterizing the interior of the tumour with nitrate of silver. The cause of cephalæmatoma Dr. Doepp confesses himself unable to explain, and he is quite dissatisfied with all theories hitherto promulgated.

The treatment appears to have been simple and sensible. Fomentations were applied at first, but if no disposition to absorption manifested itself before the end of the second week the tumour was opened by a small incision, four or five lines in length, and the extravasated blood was pressed out. The fear of hemorrhage, so dangerous to a new-born weakly child, deterred Dr. Doepp from emptying the tumour at an earlier period, and he recommends that the operation, on the other hand, should not be delayed beyond the fortnight, as absorption of the bone will then probably commence.

Hermaphrodism. In the list of deformities observed in the institution, a singular case of female hermaphrodism is related. The clitoris was above an inch (English) in length, and possessed a perfect glans and præputium. From its interior surface a groove extended to a small opening in the perineum. The integuments hung down on either side of this orifice, exactly like a divided scrotum into which the testicles had not as yet descended. No urine flowed from the above-named orifice in the perineum, but still it might have been considered justly as the orifice of the urethra, as the child was born with a hernia of the completely retroverted bladder. An inflammatory attack in the bowels carried off

the infant at the age of ten months, and upon dissection a double vagina and a double uterus were discovered. The other internal female organs presented nothing abnormal.

Mortality. The deaths in the institution continue at the rate of 50 per cent., annually. The annual mortality in the excellent Foundling Hospital at Vienna is not less than 66½ per cent., a superiority of mortality which Dr. Doepp attributes to the children in the latter establishment being almost entirely brought up by hand. More than once has this plan, on account of the enormous expense of wet-nurses, been proposed in the hospital of St. Petersburg; but, were it adopted, Dr. Herzog says not one twentieth of the children would be saved.

VIII. *History of an operation for varicose aneurism*; by Dr. Doepp. The child, a female, was two years and two months old, and the tumour occupied the right cheek, being in size equal to the clenched fist of a man. The operation, which Dr. Doepp considers to be new, consisted in transfixing the tumour with a needle four inches long, to which was attached a firm double ligature. The needle was carried through the tumour and the ligature then firmly tied over a hand compress. Suppuration soon came on, and a perfect cure ensued without any bad symptoms. Our readers know that this operation is not new; it has been frequently, we believe, performed both in this country and abroad.

IX, X, XI. These articles are, respectively, cases of *encysted tumour of the brain*, of *heart disease*, and of *abdominal emphysema*. The first is related by Dr. Doepp, but as its details throw no additional light on the pathology of the cerebral system they may be passed over. The second case, by Professor Lichtenstädt, is merely one of hypertrophy of the heart, with ossification of its valves. The third, by the same author, is interesting only in so far as the abdominal emphysema occurred as a complication of erysipelas of the face, and the distension of the intestines was apparently occasioned in a great measure by occlusion of the ilio-cæcal valve from a melanotic tumour situated in that portion of the intestine. The cæcum was found deeply ulcerated, and there was also a stricture of the sigmoid flexure of the colon.

XII. This is a short paper by Dr. Wolff on *transfusion*, which contains nothing interesting.

XIII. *Report of the Children's Hospital in St. Petersburg*; by Dr. Weisse. This establishment was founded in 1834, and consisted then of only 60 beds. It now affords accommodation for 102 children. The report embraces a period of six years, from the foundation of the hospital to January 1841. The internal arrangement of the building and its domestic economy are given at great length, but we have only room for the second part of the report, wherein is given a history of the more remarkable cases of disease that have occurred there during the abovementioned period.

Gangrene of the genitals. Two cases of this affection in the female organs have been observed. One of these gave rise to an accusation of

rape against an individual of the higher rank in St. Petersburg; the disease, indeed, seemed to have been quite unknown to the practitioners in that capital, till their attention was called to its existence by Dr. Weisse, who, however, candidly acknowledges that he himself only became acquainted with the disorder by casually observing in Horn's Archiv, a translation of Isuard Cevoule's Essay on Gangrene of the Labia in female children. Dr. Weisse considers this malady to be identical with noma, and proposes to designate it *noma genitalium*. It has occasionally, he says, been observed along with that disease in the same individual.

Measles. Dr. Weisse in five or six cases observed the eruption of measles to supervene on the convalescence from typhoid fever. When patients labouring under scabies were attacked with measles the former malady disappeared, during the prevalence of the exanthema, but returned in full force after the latter had passed away.

Ascites. In three cases Dr. Weisse has tried parcentesis, but without any permanent benefit; indeed, in one case, death was directly accelerated by the operation.

Caries of the frontal bone. This arose from a slight but neglected wound over the right parietal bone, from which there gradually ensued an enormous suppuration beneath the scalp. Many pounds of pus were pressed out through the original very minute wound on the first day that the child was seen by Dr. Weisse. The frontal bone was carious to a considerable extent, and an immense discharge flowed for some time from the wound, but the child eventually recovered, after remaining four months and a half in the hospital.

Hydrocephalus acutus. Dr. Weisse, in an almost hopeless case of this disease, made trial of corrosive sublimate internally, as recommended by Dr. Spiritus, in Hufeland's Journal. A twelfth of a grain was given every two hours in distilled water. The patient, a girl of five years of age, eventually recovered, but many small abscesses appeared during convalescence beneath and in the scalp, and which occasioned much pain, though they probably acted beneficially as counter-irritants. Of course not a shadow of proof exists that the disease was cured by the mercury. The reverse is much more probable.

Bifurcation of the aorta. A boy of fourteen died with the usual symptoms of heart disease, and acute pericarditis was discovered on dissection. The aorta shortly after its origin from the left ventricle divided into two branches, which after a short course again united, forming a dilated sac at the point of reunion. The left carotid and subclavian sprang from the left branch of the aorta, from the right branch there arose only the right subclavian, the right carotid springing from the divarication of the aorta.

Permanent contraction of the limbs in children. Dr. Weisse has observed several instances of this disease, but does not add much to what has been already written on the malady by Guersent and by Baudelocque. Dr. Weisse does not believe the symptoms to arise solely from cerebral disease, but he has more frequently observed it as a consequence of the irritation of worms, of masturbation, or of the approach of the menses in young girls.

Exanthematous diseases. In the space of four years Dr. Weisse has observed sixty-one cases in which an acute exanthema has supervened

upon some other cutaneous disorder, or upon another disease. In forty-two of these the supervening exanthema was measles; in eight, scarlet fever; in six variola; in three varioloid; and in two varicella. Five of the patients suffered, each in quick succession, from three different exanthemata. The comparative rarity of smallpox after other diseases is easily explained by the influence of vaccination, but against scarlatina no such protecting influence is known to exist. Dr. Weisse suggests that, as no case is known (?) where scarlatina has supervened upon true variola, the tendency to the former may be destroyed by genuine smallpox, and thus perhaps may be explained the great increase of virulence in the scarlatina of later years. "Most practitioners of the present day," observes Dr. Weisse, "will admit that our scarlatina is no longer the smooth red blush formerly described, but is on the contrary almost always the miliary form of the disease." With Dr. Jahn, of Meiningen, he considers the vesicles of our present scarlet fever as the residuum of the tendency to smallpox (pocken-anlage), existing in the constitution. Dr. Weisse has even seen some of the vesicles surrounded by a distinct areola, and filled with a fluid resembling pus.

The acute exanthemata are more easily communicated to those labouring under chronic than under acute disorders. Patients suffering from fever, are not liable to the exanthemata until their disease is subsiding.

The mortality in the children's hospital has of late years been only 14 per cent., while in similar institutions in Berlin, Paris, and Vienna, the deaths have amounted to 24 per cent. annually. Into all these other hospitals, however, it must be remembered that children merely labouring under scabies are not admitted.

XIV. *On the uses of calendula and of fuligo splendens in the diseases of women*; by Dr. Ockel. Both these remedies, the plant and the root, were employed by ancient physicians. Dr. Ockel administers the extract of calendula in a certain condition of the uterus, much resembling that described by Professor Simpson and others as hypertrophy of that organ. The uterus descends low down into the pelvis and is much increased in size, thickened and indurated, though still to a certain degree elastic, and possessing none of the hardness of scirrhus. The uterus thus enlarged fills the entire pelvis, and the patient suffers from constant dragging pain, while the os uteri becomes extremely tender to the touch. The bowels in this disease are almost always much constipated, and require free evacuation by the usual purgatives, after which the ext. calendulæ should be given, with glauber salts three times a day. The cure occupies generally the space of five or six weeks. Dr. Ockel has occasionally observed this condition of the uterus to be connected with violent metrorrhagia, and in such instances he believes the calendula to be the only efficient remedy.

Fuligo splendens is extolled by Dr. Ockel as an excellent remedy against abortion.

XV. This is a history of *a case of chorea*, treated and cured (?) by the muriate of tin, in the dose one sixteenth of a grain for some weeks, by Dr. Person.

XVI. *Report of the Private Ophthalmic Institution of St. Petersburg, from May,* 1833 *to May,* 1841; by Dr. W. Lerche. Four cases of confirmed cataract were treated by galvanism, or rather by galvano-puncture. Three of the patients completely recovered their sight, but the vision of the fourth was little, if at all, improved. The history of these cases has been already published in several of the continental journals, and also in our own.

The volume concludes with a number of chirurgical cases by Dr. Salomon: none of these, however, are of any great interest. Dr. Salomon is evidently a pains-taking and prudent surgeon, and seeks rather to enlighten his brethren by recording his experience, than to astonish the medical world by the boldness and success of his operations.

As an appendix, we have the address presented to Professor Busch upon his fifty years' jubilee as a practitioner. In this address is contained an excellent recapitulation of the transactions of the German Medical Association, from its origin until the present period; the noblest tribute that could be paid to one who may justly be regarded as the founder of that Society.

A few typographical errors occur in this volume; but upon the whole it is a fair and good specimen of Russian typography. We sincerely hope that seven more years may not elapse ere we are favoured with another volume from our northern friends.

Art. XII.

An Essay on the Properties of Animal and Vegetable life; their dependence on the Atmosphere, and Connexion with each other, in relation to the Functions of Health and Disease. By Edward James Shearman, M.D., &c. &c.—*London,* 1845. 12mo. pp. 175.

Our readers are well aware that we have always drawn a broad line of distinction between those works addressed to the general public which tend to enlighten them with regard to the healthful regulation of their bodily functions and the avoidance of causes of disease, and those which profess to bring down the rules for the diagnosis and treatment of disease within the popular capacity. Of the former subjects they cannot, in our estimation, know too much, and every particle of fresh information has its use. In regard to the latter class, we regard the old adage "A little knowledge is a dangerous thing," as pre-eminently true; there being, in our minds, no safe medium between that entire abstinence from all interference with nature's own method of curing diseases (the access of which is due, in nine cases out of ten, to some infringement of her laws), befitting a state of entire ignorance of her operations, and that kind of interposition which is justified by extensive observation of the phenomena of disease, and full acquaintance with the most successful methods of treatment which experience can dictate.

The little volume, whose title we have quoted, fairly belongs to the first of these categories, and it has merits which distinguish it from other works of its class, in the more comprehensive survey which it takes of

the general phenomena of life, and in the introduction of numerous facts derived from the most modern sources of information. But, on the other hand, we feel in duty bound to state that it is disfigured by numerous errors of a very grave description, some of which display either a positive ignorance of the elements of science, which we should have scarcely imagined possible in a well-educated physician, or a lamentable want of power of accurate expression, both of which faults are alike injurious to an author who addresses the general public, or indeed to any author whatever; whilst others result from a want of scientific discrimination, which leads to the enunciation of crude theories as admitted scientific facts. A few short extracts will serve to justify our criticism; we shall not tire the patience of our readers by much comment, nor by lengthy quotations.

"It is an established fact in natural philosophy that transparent convex lenses absorb, and opaque concave lenses reflect, the rays of light." (p. 13.)

We should like to know where Dr. Shearman learned his optics.

"The mind of man evidently depends upon the organization of the brain, and when the organs cease to act, in death, the brain, and consequently the mind, is, as will be discovered as we proceed, converted into gas, to supply the wants of vegetable matter, in the same way as the rest of the animal creation." (p. 14.)

Although "imagination may trace the noble dust of Alexander, till we find it stopping a bunghole," yet our author's powers far transcend those of the poet; for, by some undescribed process, he seems to detect the solid mind of a philosopher in the corn and potatoes of the next generation (a new relation which we recommend to the consideration of the political economists); whilst the lighter mind of the poet may find a fitter habitation amongst roses and violets, carnations and lilies; the mind of the physician, we presume, will furnish food to medicinal plants, and that of the architect or artisan will aid the development of timber-trees. Two pages further on we have the following novel theory of calorification:

"Whenever hydrogen and oxygen meet in the chemical laboratory, under the influence of galvanic action, they are decomposed, water is formed, and heat is set free. The nerves supplying the lungs sufficiently account for the galvanic battery." (p. 16.)

"Which fully accounts for the milk of the cocoa-nut," we are tempted to add by way of parallel. It would be difficult, we think, to put together anything more incongruous than the ideas contained in the above quotation. In our simplicity we had always imagined oxygen and hydrogen to be undecomposable substances; we were further taught in our younger days, that whilst an electric spark would cause these two gases to unite and form water, a galvanic current decomposed water, and resolved it into its component gases. Moreover, we are at a loss to perceive how "the nerves supplying the lungs account for the galvanic battery;" unless the author means to assert that they convey galvanic influence from the nervous centres, which no one has yet succeeded in proving, all evidence on this point being decidedly negative.

In endeavouring to explain the nature of the respiratory process, and to harmonize the rival theories of Liebig and Mulder, Dr. Shearman involves the subject in a most perplexing jumble, through which it must be difficult, if not impossible, for any uninformed reader to find his way to

the simple truth, that oxygen is imbibed in the lungs in exchange for carbonic acid given off, the reverse process taking place in the tissues; and that the blood is the carrier of these gases between the pulmonary and systemic capillaries, in all cases in which the air does not come into direct relation with the latter.

In a subsequent chapter, on secretion, we are told that "the arteries, when supplied with proper nerves, have the power of secreting that peculiar fluid from the blood which the various organs require; the real cause of this principally depends upon that peculiar galvanic power which the nervous system has over the various organs." (p. 32.) Here again we find an ignorance of the tendency of recent investigations, which we should not have expected in a writer who seems to be on many other points "well up" with modern science. Everything indicates that the secreting process is no more dependent upon the nervous system in the animal than it is in the plant, that its immediate instruments are the same in both, namely *cells*, and that the *vessels* are only subservient to it, by furnishing a constant supply of the requisite material.

In the same chapter, under the head of perspiration, after learning that about thirteen ounces of watery fluid are excreted from the skin of a healthy adult in the form of insensible perspiration every twenty-four hours, and that this is loaded with nitrogen, "which is taken into the stomach with the food, in the form of nitrogen gas of the air" (of which last statement we must confess ourselves unable to comprehend the precise meaning), we meet with the following novel and startling announcement:

"But the skin has the power of excreting a large quantity, not only of fluid, but of nitrogen. Eleven grains of matter are exhaled from the skin per minute, or thirty-three ounces in twenty-four hours; and of this eighty-eight per cent. is solid,—principally nitrogen." (p. 47.)

Eighty-eight per cent. of thirty-three ounces is precisely twenty-nine ounces, a quantity greater than the average amount of solid matter daily taken in by an adult as food. Where *did* Dr. Shearman find such a statement as this? Certainly not in the works of Billing, Bird, Brongniart, Carpenter, Dumas, Johnson, Liebig, Müller, and Mulder, to whom he refers as his physiological authorities. That the above assertion is not a mere clerical error, but expresses the author's convictions, appears from a repetition of it in the appendix (p. 161), where it seems to rest upon the authority of Müller. The source of the author's mistake is evidently to be found in the idea that the analysis of the *solid* matter of the cutaneous exhalation,—which does not average above 1 or 2 per cent.,—represents that of the entire secretion, of which 98 or 99 parts are water. How any man in his sober senses could make such a mistake, we are at a loss to conceive.

We are sorry to feel called upon to express so strong an opinion as to the demerits of this treatise. Dr. Shearman may be, and we gather from many of his pages that he is, a judicious practitioner and an estimable man. But he has assuredly much mistaken his vocation, in committing himself to authorship. The general plan of the book, as we have already stated, seems to us to deserve much commendation. It is in its execution that the author has so lamentably failed; and we cannot but regret that we have felt ourselves called upon to expose his errors.

ART. XIII.

Fauna Antiqua Sivalensis; being the Fossil Zoology of the Sewalik Hills, in the North of India. By HUGH FALCONER, M.D., F.R.S., F.L.S., F.G.S., &c. &c., and PROBY T. CAUTLEY, F.G.S., Captain in the Bengal Artillery, &c. &c. Part I.—*London,* 1845. 8vo. pp. 64. With Twelve Lithographic Plates, large folio.

ALTHOUGH this is but the first number of a work which will not be completed in less than twelve, and which may very probably extend to more, we think it right to bring it thus early under the notice of our readers; both in order that we may do what in us lies to make it extensively known, and that we may enjoy the pleasure of paying our humble tribute of praise to its extraordinary merits, or rather to the merit of those researches of which it is one of the results or exponents,—a still more brilliant result and a yet more satisfactory exponent being the magnificent collection of fossil remains deposited in the British Museum, and now in course of arrangement under the superintendence of Dr. Falconer.

"The object of this publication," to use the words of its authors in their prospectus, "is to make known, in a connected and complete series, the numerous fossil animals which have been discovered in the North of India, by the authors and other inquirers, during the last twelve years; and to develop the bearings of these discoveries on the physical and geological history of India, during a great part of the tertiary period. The fossil fauna of the Sewalik range of hills skirting the southern base of the Himalayahs has proved more abundant in genera and species than that of any other region yet explored. As a general expression of the leading features, it may be stated, that it appears to have been composed of representative forms of all ages, from the oldest of the tertiary period down to the modern; and of all the geographical divisons of the Old Continent, grouped together into one comprehensive fauna in India. Of the forms contained in it may be enumerated, in the *pachydermata,* several species of mastodon and elephant, the hexaprotodon hippopotami, merycopotamus, rhinoceros, anoplotherium, sus, and three species of equus; in the *ruminantia,* the colossal genus sivatherium, peculiar to India, with species of camelus, camelopardalis, bos, cervus, and antelope; in the *carnivora,* species of most of the great types, together with several remarkable undescribed genera; in the *rodentia,* several species; in the *quadrumana,* several species; in the *reptilia,* the gigantic tortoise (colossochelys), with species of emys and trionyx, and several forms of crocodile. To these may be added the fossil remains of birds, fishes, crustacea, and mollusca."

Of all these remains those of the colossochelys are perhaps the most *wonderful,* since the idea of a tortoise twenty-two feet long (over the arch of the carapace), from the snout to the tail, is more completely foreign to our ordinary notions regarding that group of animals, than would be the description of an elephant standing thirty feet high, or of a hippopotamus measuring as much round the belly,—accustomed as we are to connect the very names of these animals with the idea of gigantic size. But the most *interesting* of these remains, to the zoologist and comparative anatomist, are such as tend to fill up the hiatuses left in various parts of the existing series, to indicate affinities where no near relationship previously seemed to exist, and to show how every conceivable union of organs, that could possibly coexist, finds its realization in nature. And to the geo-

logist, the mode in which these various remains have been imbedded,—the cause of the wonderful aggregation, not merely of individuals but of species, and not merely of species that may be presumed to have co-existed, but of many that must have succeeded one another at very remote periods of time,—and the past condition of the part of the Eastern Continent in which they present themselves,—are problems of the highest interest, towards the elucidation of which we have reason to believe that the authors will be enabled to contribute a vast amount of novel and important information, from the varied stores which they have collected during their laborious and protracted inquiries.

The authors further tell us, that "they have been induced to undertake the work by the belief that the scientific reputation of this country and the credit of the Indian service are concerned in bringing to light researches embracing so many new facts, and bearing so importantly on the past physical history of the vast possessions of the British Empire in India. They are not insensible to the difficulty and extent of the subject, but they hope that they are in some measure prepared for it, by previous investigations extending through several years."

In this truly modest spirit do they allude to a series of researches, carried on, we have reason to know, under almost every possible disadvantage, with the most indomitable perseverance. At a distance of many hundreds of miles from those sources of information which the metropolis of Anglo-India might afford, with no copy of the 'Ossemens Fossiles' to guide their identifications, by the light which Cuvier had struck out, with no Museum, such as that of the Jardin des Plantes, or of our own College, to which to have continual recourse for the comprehension of the nature of their fast-accumulating treasures, they were thrown upon the book of nature, and upon the living museums of the neighbouring woods and plains, for the determination of these fossils, by the aid of their existing congeners; and their own guns, whilst providing daily food for their corporeal wants, supplied also their intellectual provender,—namely, the dry bones, which to all else, save the fierce hyæna, were valueless, but which to them were in place of books and plates and professors skilled in the lore of comparative anatomy. We rejoice to learn that the British Government and the Directors of the East India Company have thought fit to accord such an amount of aid *in limine*, as will ensure the successful progress of this truly national undertaking; and we also rejoice in the hope that the two hundred chests of Sewalik fossils, which have been presented by Capt. Cautley to the British Museum, will be speedily arranged there by Dr. Falconer, and made fully accessible to the public.

The style in which the work is produced, and the exceeding beauty of the illustrations, do great credit to the enterprising publisher, and to the artists who have been employed upon them. The lithographs will bear the closest inspection, and challenge comparison with any that have proceeded from continental presses, distinguished as the latter have always been for correctness of drawing and beauty of finish.

Following the order of the 'Ossemens Fossiles,' the group of proboscidea is first brought under review; and its selection is the more appropriate, as it most signally displays the numerical richness of forms, which

characterizes the fossil fauna of India; whilst the materials collected by the authors are most valuable, as tending to settle many points which have hitherto been subjects of grave discussion. Palæontology made its first great advance in the exact determination of the mammoth of Siberia, and of the mastodon of North America, which we owe to Cuvier, and the new forms which have been discovered since that time, have been the subjects of attentive examination by Owen, Blainville, and other eminent anatomists.

"But notwithstanding the vast amount of observation on the subject during late years, a great difference of opinion has prevailed among comparative anatomists and palæontologists, down even to the period when we now write, in regard to the degree of affinity and the generic relations of the different species of mastodon and elephant. The majority of late authorities, including Cuvier and Owen, have regarded them as constituting two distinct and well-marked, though closely-allied, genera; others have gone the length of breaking up mastodon into two genera: while M. de Blainville has reverted to the opinion of some of the earlier observers, that the so-called mastodons and elephants are but modifications of one common type, differing so little from each other that all the species may, with propriety, be included within the limits of a single genus. A still greater and vastly more important difference of opinion has prevailed, regarding the number and characters of the species; for, while the conflicting views respecting the generic distinctions concern little more than the principles of systematic classification, the accurate determination of the fossil species affects the value of facts, which implicate the accuracy of some of the most weighty arguments in the geology of the later tertiary strata, more especially such as relate to the changes of climate which are supposed to have accompanied their deposition, and the extension of the species through a wide range of time and space." (p. 3.)

Thus Cuvier regarded all the fossil-elephant remains of the Old and New World as belonging to one and the same species, the *elephas primigenius* or mammoth, and Professor Owen has been of the same opinion. By M. de Blainville it has even been urged that the mammoth cannot be shown to be specifically distinct from the existing Indian elephant. Other palæontologists, on the contrary, have constructed no fewer than ten species out of the single species of Cuvier; founding the distinctive characters upon the differences presented by the molar teeth. So in regard to the mastodons, some would restrict the number of species (known previously to those now described) to four or five, whilst others would raise the number to twenty. This great diversity of opinion, almost unequalled in any other section of mammalian palæontology, has, in a great measure, arisen from the isolated and defective nature of the materials relating to this tribe, as they ordinarily come before the palæontologist, and especially from the peculiarities which characterize the dentition of the proboscideans, and which cause the teeth of the same species to present very different aspects at different periods. It is only, therefore, as our authors justly observe, from the comparison of an extensive series of specimens, embracing every period of life, and the range of individual sexual varieties through which the species runs, that any safe conclusions can be drawn regarding the distinctive characters of any one form. In this respect they have been peculiarly fortunate, owing to the surprising number of

forms belonging to this family embraced in the fossil fauna of India, and the immense abundance in which their remains have been met with. The results of their researches lead them to differ on certain points from all palæontologists who have treated of this family. As the subject is only commenced, however, in the present Part, we shall not now enter upon it, but shall present our readers with a general summary of our author's views, and of the grounds of them, when they shall have been completely developed.

ART. XIV.

MR. CHURCHILL'S MANUALS.

1. *A Manual of Physiology, including Physiological Anatomy; for the use of the Medical Student.* By WILLIAM B. CARPENTER, M.D., F.R.S., &c.
2. *A System of Practical Surgery.* By WILLIAM FERGUSSON, Esq., F.R.S.E., Professor of Surgery in King's College, London, &c. Second Edition.—*London*, 1846. pp. 668.
3. *A Manual of Medical Jurisprudence.* By ALFRED S. TAYLOR, F.R.S. Second Edition.—*London*, 1846. 8vo, pp. 704.

IT has been our misfortune to have to turn the light of our critical countenance from so many books bearing the well-known sign and superscription of JOHN CHURCHILL, that we can afford to praise our worthy publisher, when he deserves it, without any danger of being accused of partiality. And, assuredly, when we look at the three splendid volumes now before us, and the others previously published on precisely the same plan,* which, under the humble name of MANUAL, comprise the most elaborate and complete treatises on their respective subjects—when we inspect their beautiful typography and incomparable woodcuts—and compare their extremely moderate price with what we know must have been the very large outlay requisite to bring them to such a state of perfection,—we must honestly admit that the obligations are mutual between the bibliopolist of Princes street and the members of the medical profession. If by their patronage he has waxed rich and portly, they, in their turn, are benefited by, and thus become sharers in, his prosperity. Nothing but a large capital could bring into the market works of this very expensive sort, and so rapidly one after another; and nothing but a most extensive sale could make the outlay of that capital remunerative, under the low price at which the volumes are sold. No doubt our good friend knows what he is about. We do not accuse him of any superhuman regard for the poverty of the medical exchequer. If he is liberal to his writers and liberal to his customers, it is a very possible case that he has found out that such liberality has no deteriorating effect on the treasury of Princes street. But be that as it may, we have so much reason to be satisfied with the result, as far as it concerns us, that we here give him public thanks for

* A Manual of Chemistry, by George Fownes, F.R.S.; The Anatomist's Vade Mecum, by Erasmus Wilson, F.R.S.; Elements of Natural Philosophy, by Golding Bird, M.D., F.R.S.

the positive benefit conferred on the medical profession by the series of beautiful and cheap Manuals which bear his imprint, and of the last published of which we now proceed to give an account.

I. DR. CARPENTER'S PHYSIOLOGY.

The multiplication of general treatises on physiology, especially of those intended for the medical student, is a cheering indication of the increased attention which is being paid to this department of medical science—a department whose importance is now coming to be acknowledged as universally as it was formerly overlooked. This little volume, we are informed by the author, owes its origin to a desire on the part of Mr. Churchill to add an elementary treatise on physiology to the series of student's manuals which he had previously issued; and the success of Dr. Carpenter's larger works naturally led him to press the execution of the task upon that gentleman, who would otherwise, he assures us in his preface, have been better pleased to devote his leisure time to the prosecution of original researches in his favorite department of study.

We are led by the preface to expect some degree of novelty in the plan of his treatise, that should render it something else than a mere abridgment of the author's larger works, which it must have otherwise almost necessarily become; and a glance over its pages will show in what this novelty consists. Ever anxious to impress his readers with the *principles* of the science he is expounding, and desirous to elucidate these by all the light to be derived from recent discoveries, Dr. Carpenter has devoted a large proportion of the early part of the volume to an account of the elementary parts of the human body—in other words, the primary tissues—including their structure, chemical composition, mode of growth and development, and their actions in the general economy. The following extract will show the conformity of his plan to that which would be adopted in any parallel case, and will also indicate that he has prosecuted it in accordance with the most advanced views of the present remarkable era in physiology—that in which the independent life of cells and their functional instrumentality have come to be recognised as the great fundamental facts in the vital actions of animals, as they have long been known to be in that of plants.

"In the investigation of the operations of a complex piece of mechanism, and in the study of the forces which combine to produce the general result, experience shows the advantage of first examining the component parts of the machine—its springs, wheels, levers, cords, pulleys, &c.—determining the properties of their materials, and ascertaining their individual actions. When these have been completely mastered, then attention may be directed to their combined actions; and the bearing of these combinations upon each other, so as to produce the general result, would be the last object of study. This seems to be the plan which the student of physiology may most advantageously pursue in the difficult task of making himself acquainted with the operations of the living fabric, and with the mode in which they concur in the maintenance of life. We should first examine the properties of the component materials of the structure in their simplest form; these he will find in its nutrient fluids. He may next proceed to the simplest forms of organized tissue, which result from the mere solidification of these materials, and whose properties are chiefly of a mechanical nature. From these he will pass to the consideration of the structure and vital actions of those tissues that consist

chiefly of cells, and will investigate the share they take in the various operations of the economy. Next his attention will be engaged by the tissues produced by the transformation of cells; of which some are destined chiefly for affording mechanical support to the fabric, and others for peculiar vital operations. And he will then be prepared to understand the part which these elementary tissues severally perform in the more complex organs. A due knowledge of these elementary parts, and of their physical, chemical, and vital operations, is essential to every one who aims at a *scientific* knowledge of physiology. True it is that we may study the *results* of their operations without acquaintance with them; but we should know nothing more of the *working* of the machine than we should know of a cotton mill, into which we saw cotton wool entering, and from which we saw woven fabrics issuing forth; or of a paper-making machine, which we saw fed at one end with rags, and discharging hot-pressed paper, cut into sheets, at the other. The study of these results affords, of course, a very important part of the knowledge we have to acquire respecting the operations of the machine; but we could learn from them very little of the nature of the separate processes effected by it; still less should be prepared, by any disorder or irregularity in the general results, to seek for and rectify the cause of the disturbance in the working of the machine, by which the abnormal result was occasioned." (p. 94.)

It is a great advantage of the general view of the elementary operations of the animal economy, which is thus presented, that the author is thereby enabled to carry forward his description of the several functions (which occupies the second part of the book) in what we cannot help regarding as their natural order; namely, commencing with the ingestion of food, tracing its gradual conversion into the fabric of the body, and its final disintegration; then proceeding with the reproductive operations; and closing with the functions of animal life, the maintenance of which is so completely the final cause or purpose of the preceding. The objection felt by the author to the adoption of this course in his human physiology—namely, the intervention of the nervous system in almost every one of the organic functions—is obviated by the previous explanation of the general operations of the nervous apparatus, which is contained in the first part.

Among the novelties in this volume, which indicate the progress of the author's general views, we may advert to his description of the ultimate composition of the muscular fibrilla, in which he seems to us to have advanced considerably both upon Mr. Bowman and upon Mr. Erasmus Wilson. He describes and figures the fibrilla as composed of a series of short cylindrical cells, laid end to end, the cavities of which, being dark, (probably in consequence of containing some highly-refractive material,) give rise to the series of equidistant dark spots in the fibril; whilst the intervening light spaces are made up by the contiguous walls of each pair of cells, which are separated by a distinct transverse line, equidistant between the dark spots. The white portion of the fibril is stated by Dr. Carpenter to present itself, not merely *between*, but also *around* the dark spots; which seems almost conclusive as to its being the cell wall. The figure given by Dr. Carpenter closely resembles one given by Dr. Goodfellow in the short-lived 'Physiological Journal;' but his explanation is altogether different, and to our minds much simpler. According to Dr. Goodfellow, the dark points are the "sarcous elements" of Mr. Bowman, imbedded in a tube of sarcolemma, which possesses transverse septa at intervals; whilst by Mr. Erasmus Wilson, in his 'Vade Mecum of Anatomy,' the

strange mistake (for such we must regard it) is made of describing the dark spots and the intervening light spaces as *each* consisting of cells, of which he is obliged, therefore, to suppose two distinct kinds to exist—a pair of light cells (separated by the transverse line above mentioned) being interposed between each pair of dark ones. To those who are acquainted with vegetable structures it may be sufficient to state that the fibril, in Dr. Carpenter's eyes, has precisely the characters of a minute conferva; and that the phenomenon of contraction consists in just such a change of form of the individual cells as that which occurs in the contraction of the irritable tissues of plants. He thus extends his previous generalization. Having shown that all the essential parts of the organic nutritive functions of animals are carried on, like the corresponding actions of plants, by the agency of cells, and having pointed out the strong reason there is for the belief that, wherever changes in the nervous system originate, their immediate instruments are of this very same character, he is enabled to show that *all* the active vital changes in the animal body may be reduced to this one great principle of *cell-life.*

We observe that in treating of the process of sanguification, the spleen, the supra-renal capsules, and the thymus and thyroid glands are included with the glandulæ of the absorbent system in the general assimilating apparatus; the cells of their parenchyma being supposed to withdraw certain materials from the blood, and then to restore them, after having exercised upon them an elaborating action. This view, for the grounds of which we must refer our readers to the book itself, is confirmed in a very striking manner by the interesting embryological researches of Mr. J. Goodsir, very recently communicated to the Royal Society; it having been shown by that gentleman that the three last of the above-named organs *are persistent portions of the original germinal membrane or blastoderma;* which is the sole organ by which the assimilation of the materials supplied by the yolk is effected during the earlier periods of development, and which thus leaves behind it (so to speak) some fragmentary portions to aid in carrying on the process during the earlier period of extra-oval life.

Various practical hints are interspersed through the pages of this beautiful volume, which may help, we trust, towards the erection of that new temple of Therapeia in which we call upon all our readers, young and old, to "bear a hand;" and we may point to the 'General Summary of the Excreting Processes,' (pp. 437-40,) as of peculiar value in this respect—not from any novelty in the ideas put forward, but as being invested with a definite and tangible shape, very different from those of the old humoral pathology, and much more likely to exert an advantageous influence on the treatment of disease.

The author has shown singular skill in preserving so marked a line of distinction between the present Manual and the 'Principles of Physiology' previously published by him. They are both on precisely the same subject; but the one is neither a copy, nor an abstract, nor an abridgment of the other. In one thing, however, they are exactly alike—in their general excellence, and in their perfect adaptation to their respective purposes. The reputation of Dr. Carpenter as a physiologist is too well established throughout the whole medical world to admit of increase from any commendation of ours; but we should be doing injustice to our own

feelings if we did not here express our admiration of his great intellectual powers, of his extensive learning, of the comprehensiveness of his views, of the quickness with which he seizes the important points and bearings of each subject, of the logical order in which he arranges his facts, and of the clearness and precision with which he explains and exposes his doctrines. Dr. Carpenter's various treatises are in fact models in their respective departments. It is their great and varied excellence which accounts for their unrivalled popularity. We can pay no higher compliment to the work before us than to say that it is equal in merit to the former productions of the author. This is equivalent to saying that it is, without question, the best manual or short treatise on physiology extant. Although designed for the student and framed expressly to meet his wants, it is a work, we will venture to say, that may be consulted with advantage by most physicians and surgeons, however learned.

II. Mr. Fergusson's Surgery.

The first edition of this work has been so recently under our notice that we deem it unnecessary, at the present date, to make any lengthened addition to our former observations upon it. It is but justice, however, to state our belief that much time and labour must have been expended upon the present volume; and the author very modestly and ingenuously admits, in his preface, that "advantage has been taken of the suggestions of the reviewers to alter or amend whatever seemed faulty or defective." A close inspection shows that, besides the new matter, there has been a very careful revision of the original text throughout. To show that some of our own hints have not been thrown out in vain, we may refer to the author's notice of the entrance of air into the veins (p. 444) during operations. We thought that he had treated this dangerous event somewhat heedlessly, but he has now made amends; and although still adhering to his former opinions, he gives reasons for his peculiar views and quotes the authorities of Nysten, Velpeau, and Cormack, to prove that Bichat was wrong in his estimate of the quantity of air which was likely to induce fatal effects, much more being requisite than was imagined by that great authority.

The object of our author seems to be to confine himself as much as possible within the limits of what may be called *pure* surgery. There is certainly much pertaining to the ordinary practice of the surgeon which is not even alluded to in this work, and the author apologises for such deficiency by stating his opinion "that a 'Complete System of Surgery' should embrace all branches of professional knowledge, but that as each department has its separate authors and teachers, he has limited his work accordingly." Certainly, in a portable volume like that before us, it would have been impossible to have grasped all such topics comprehensively; and perhaps a tolerably fair criterion of the practical value of the particular selection adopted by the author, is the popularity which his work has so rapidly attained.

The following seem to be the principal novelties in the present edition:

The alterations in the first part of the volume, 'The Elements of Practical Surgery,' are more extensive than elsewhere. Some chapters of the former edition have been altered and transferred to other parts of the

volume, and we observe the addition of a new one on the subjects of "boils, carbuncles, ulcers, scalds and burns, chilblains, and frostbite." We perceive, also, in this part of the work, that the author has not altogether neglected our former complaints as to the necessity of defining diseases as well as naming them; and although even yet we think him defective in this respect, we are sure that the additions which he has made will make his work more acceptable to all parties.

In the chapter on amputation (p. 149), our author gives reasons for entertaining a preference to "secondary" amputation rather than "primary" among civilians, and in this respect his doctrines are somewhat novel among systematic authors of the present day, the custom having so long prevailed of taking their data from military authorities. The views founded on the statistics of military practice have been somewhat modified, however, by Mr. Rutherford Alcock, and Sir George Ballingall; and the tables referred to by our author, as made up by Dr. Henry Reid from the books of the Royal Infirmary of Edinburgh, are exceedingly interesting, as showing the great success attending secondary operations of this description. The recent additions to operative surgery performed by Mr. Syme of Edinburgh, respecting amputations at the ankle and knee, are fully noticed by Mr. Fergusson; and we are pleased with the impartial manner in which he has considered these subjects. The bold operation of resection of the head and neck of the femur is given in detail; and the subject of staphyloraphy, for improvements in which the profession acknowledges itself indebted to our author, is here placed in a light at once novel and scientific. On the operation of harelip we perceive that Mr. Fergusson leans to the views expressed by Dubois, Malgaigne, Warren, and others, as to the advantages of early interference.

In conclusion, we will only add, that our former very favorable estimate of Mr. Fergusson's system is considerably enhanced by the examination of the present edition; and we can conscientiously recommend it to our readers as a work deserving a place in the library of every surgeon. To junior practitioners it may be said to be indispensable.

III. MR. TAYLOR'S MEDICAL JURISPRUDENCE.

In our Journal for January, 1844, we noticed with high but well-deserved eulogy the first edition of this work. The rapidity with which a large impression has been disposed of is a satisfactory proof that the character we gave of it was just. As we expected, it has become truly *the manual* of both the medical and legal professions, and is regarded by all as the standard authority on its subject. The author, also, as we find from the public prints, is the person consulted, almost as a matter of course, in the more difficult medico-legal cases. In the present edition the work is greatly enlarged by the addition of new materials; and very important alterations and improvements are made in the former text. The following extract from the preface notices only a few of these changes:

"In the section on POISONING, I have thought it advisable to make a few alterations, in order that I might be enabled to introduce the principal discoveries made in Toxicology during the last two years. Among the changes may be enumerated,—the omission of some chemical processes for detecting poisons, and the

substitution of others which appeared more simple,—a description of the methods employed for detecting poisons when absorbed into the tissues,—additions to the sections on arsenic, lead, copper and opium,—new facts connected with the smallest fatal doses of poisons, and the shortest periods for their operation,—cases illustrating the fatal effects which ensue from their external application;—and lastly, the strength of all medicinal preparations which contain poison. The chapter on prussic acid has been almost entirely rewritten; and in this edition will be found sections on poisoning by the Tartaric and Acetic acids, Lime, Phosphorus, Phosphoric acid, Chromate of Lead, Elder and Laburnum. . . . . .

"Numerous additions and alterations have been made in the chapters on Wounds and Infanticide. The sections on Rape, Pregnancy, Delivery and Abortion have also been enlarged; and some additions have been made to that of Insanity, including the changes respecting medical certificates, and the discharge of lunatics, under the new Lunacy Act."

To the various chapters on poisons, the author has appended a list of the medicinal preparations into the composition of which they enter, with the doses and the proportion of each poison in a given dose of the preparation. We perceive also that the rate at which each poison is retailed to the public, and the weights of some well-known popular measures, are mentioned, so that a medical practitioner may be able to give some information on these matters, when he is called upon to express an opinion regarding quantity or dose from the evidence of non-professional witnesses.

Among the additions to the section on infanticide, we may refer to the remarks made at p. 281, on the power of the lower class of females to exert themselves, and perform numerous acts requiring great strength and presence of mind, soon after delivery,—a question which frequently arises in cases of child murder.

In treating of *corpora lutea*, in the chapter on pregnancy, the author refers to the great changes in medical opinions, respecting the value of the evidence which the presence of these bodies is capable of furnishing. The views of Dr. Knox, Mr. Wharton Jones, and Professor Bischoff are briefly examined, and their bearing on practice pointed out.

In the chapter on Drowning some important remarks are made on the occurrence of accidental fractures of the vertebræ, part of which we here extract:

"*Fractures* are not often met with in the drowned as the result of accident during or after the act of submersion. Certain fractures likely to be followed by immediate death, may forbid the supposition of their having occurred after the act of submersion, and a careful examination of the body may show that they were not likely to have arisen from accident at or about the time of submersion. This point was raised in the case of *Reg.* v. *Kettleband*, (Nottingham Winter Ass. 1843,) where the prisoner was charged with the murder of his son, a boy aged ten years. The deceased was found dead in a pond soon after he had been seen healthy and well. An inquest was held, and, as usual, no inspection of the body was required by the coroner, and the jury were directed to return a verdict of 'found drowned.' An inspection was, however, subsequently made. The neck was observed to be very loose, and on further examination the processus dentatus was found to be separated from the atlas, and the ligaments were ruptured! The three medical witnesses who gave evidence at the trial, deposed that this displacement had caused death by compressing the spinal marrow,—that the injury had occurred during life,—that it was not likely to have been caused by accident from a fall into the water, as there was no mark of a bruise about the head, and the pond was

proved to be small, with a soft muddy bottom. All agreed that such an injury was not likely to have arisen from a blow or a fall under any circumstances, because it required for its production, that the body should be fixed and the head forcibly rotated on the trunk. It was in itself sufficient to account for immediate death, and it could not occur by accident after death from any other cause. Hence it was inferred, 1, that death could not have been caused by drowning; 2, that it had resulted from the compression of the spinal marrow, by displacement of the second vertebra; and, 3dly, that this injury must have been intentionally produced by some person. Circumstances fixed the crime on the prisoner, and the jury returned a verdict of manslaughter; although the nature of the injury, admitting that it was not the result of accident, proved that the prisoner must have acted with a most cool and deliberate intention to destroy life! This case furnishes a serious commentary on the practice of certain coroners in denying the necessity for an inspection, and in directing what is called an open verdict of '*found drowned*,' where a body is taken out of water! It is an important medico-legal question, whether fractures of the *cervical vertebræ* can occur from accident alone, about the time of drowning. In the above case, the medical witnesses had probably good reasons for denying that the injury was accidental, although such an opinion cannot be always expressed merely from the absence of marks of violence on the head. Mr. South quotes the case of a man who threw himself into a river to bathe from a height of seven or eight feet, the water being only three feet deep. He rose to the surface, but fell back senseless. When he recovered his consciousness, the account he gave of the accident was, that he felt his hands touch the bottom of the river, but to save his head drew it violently back, upon which he lost all consciousness. He died in about ten hours, and on examination the back of the neck was much ecchymosed—the interspaces of the muscles were gorged with blood, and the vertebral canal filled with it. The body of the fifth cervical vertebra was broken across above the middle of its depth, and the two pieces were completely separated from the lateral parts. As there was no mark of contusion or dirt on the head, Reveillon, who reports the case, believes that the fracture arose from muscular action, and not from a blow received by striking the bottom; but this is doubtful. In another instance related by Mr. South, a sailor jumped headlong into the sea to bathe, a sail being spread three feet below the surface. He immediately became motionless, and died in forty-eight hours. The fourth and fifth cervical vertebræ were found extensively fractured, and the spinal marrow crushed and lacerated. Chelius's Surgery, Part vi., Fractures. In this case, the fracture must have resulted from contact with the water or the sail; but as the latter was freely floating, this would be a yielding medium, hence this serious injury may occur accidentally in cases in which we might not be prepared to look for it. (For an important medico-legal case, involving many questions connected with marks of violence on the drowned, see Ann. d'Hyg. 1839, ii. 195.)"

The other chapters on the various forms of Asphyxia have undergone considerable revision.

In the section on Insanity, which is much enlarged, the rules regarding certificates under the new act are fully stated for the guidance of practitioners, and a better arrangement has been adopted in the treatment of the subject of homicidal monomania. The causes, symptoms, and legal and medical tests are successively examined; then follow cases in illustration, including some of very recent occurrence. We here subjoin a portion of Mr. Taylor's general summary, and with it must conclude our very imperfect notice of this admirable treatise.

"A strange and unaccountable notion prevails in the public mind, that a homicidal lunatic is to be distinguished from a sane criminal by some certain and

invariable symptoms or characters which it is the business of a medical witness to display, in evidence, and of a medico-legal writer to describe. But a perusal of the evidence given at a few trials will surely satisfy those who entertain this notion, that each case must stand by itself. It is easy to classify homicidal lunatics, and say that in one instance the murderous act was committed from a motive—i. e. revenge or jealousy; in a second, from no motive but from irresistible impulse; in a third, from illusion or a delusive motive—i. e. mental delusion; in a fourth, from perverted moral feeling. This classification probably comprises all the varieties of homicidal insanity; but it does not help us to ascertain in a doubtful case, whether the act was or was not committed under any of these psychological conditions. It will enable us to classify those who are acquitted on the ground of insanity, but it entirely fails in giving us the power to distinguish the sane from the insane criminal.

"According to M. Esquirol, whose views, more or less modified, are adopted by all other writers on the medical jurisprudence of insanity, the facts hitherto observed indicate *three degrees* of homicidal monomania.

"1. In the first, the propensity to kill is connected with absurd motives or actual delusion. The individual would be at once pronounced insane by everybody. Cases of this description are not uncommon, and they create no difficulty whatever. The accused are rarely allowed even to plead to the charge.

"2. In the second class, the desire to kill is connected with no known motive. It is difficult to suppose that the individual had any real or imaginary motive for the deed. He appears to be led on by a blind impulse which he resists and ultimately overcomes. (Case by Mr. Daniell, antè, p. 670.)

"3. In the third class, the impulse to kill is sudden, instantaneous, unreflecting, and uncontrollable (*plus forte que la volonté*). The act of homicide is perpetrated without interest, without motive, and often on individuals who are most fondly loved by the perpetrator. (Maladies Mentales, ii. 834.)

"These three forms differ from each other only in degree;—the two first being strongly analogous to, but lighter modifications of the third. All the cases which came before M. Esquirol had three characters in common. An irritable constitution, great excitability, singularity, or eccentricity of character: and previously to the manifestation of the propensity, there was a gentle, kind, and affectionate disposition. As in other forms of insanity, there was some well-marked *change of character* in the mode of life. The period at which the disorder commenced and terminated could be easily defined, and the malady could be almost always referred to some moral or physical cause. In two cases it was traced to the result of puberty, and in four to the power of imitation. Attempts at suicide preceded or followed the attack: all wished to die, and some desired to be put to death like criminals. In none of the cases was there any motive for the act of homicide.

"M. Esquirol believes that there are well-marked distinctions between this state and that of the sane criminal. Among these he enumerates, 1st, the want of accomplices in homicidal monomania. 2d. The criminal has *always* a motive—the act of murder is only a means for gratifying some other more or less criminal passion; and it is almost always accompanied by some other wrongful act. The contrary exists in homicidal monomania. 3d. The victims of the criminal are those who oppose his desires or his wishes:—the victims of the monomaniac are among those who are either indifferent, or are the most dear to him. 4th. The criminal endeavours to conceal, and if taken, denies, the crime; if he confesses it, it is only with some reservation and when circumstances are too strong against him; but he commonly denies it to the last moment. It is the reverse with the monomaniac." (pp. 673-4.)

This section has also, we observe, an addition to it "on the Civil and Criminal Responsibility of the Deaf and Dumb."

ART. XV.

The Practice of Surgery. By JAMES MILLER, F.R.S.E., Professor of Surgery in the University of Edinburgh, Surgeon to the Royal Infirmary, &c. &c.—*Edinburgh,* 1846. 8vo. pp. 680.

WE had occasion in a former Number of this Journal (April, 1845) to speak in deservedly high terms of Professor Miller's work on the Principles of Surgery, and we are happy to be able to pronounce an equally favorable judgment on the manner in which the present volume is executed.

The same reason which prevented our undertaking to give anything like a complete review of the preceding work, namely the extent of the field embraced by it, must be our plea for contenting ourselves with selecting portions here and there, as specimens of the manner in which the author has performed his task, rather than attempting a condensed survey of the whole.

The author has chosen the division of the body into regions as the groundwork of his arrangement, commencing with the injuries and diseases to which the head is liable, and passing successively to those of other parts of the body. This order is perhaps the most convenient. The objections that may fairly be made to it, viz. that analogous affections, such, for instance, as fractures and dislocations of the upper and lower extremities, are unduly separated from each other, will have less weight when we remember that the general principles of treatment have formed the subject of a preceding volume, and that the practical application of those general principles is alone the object of the present.

The chapters devoted to injuries of the head and brain, and to diseases of the eye, furnish a good epitome of the present state of practice in these important parts of surgery; that on diseases of the scalp is derived chiefly from Mr. Erichsen's excellent treatise. The chapter on affections of the neck is carefully and judiciously written. The following are the author's conclusions respecting the diseases in which bronchotomy or tracheotomy should be employed:

"*Bronchotomy*, then, is available in the following cases: 1. In the case of *foreign bodies* lodged in the air passages. Extrusion, independently of this operation, is and ought to be the exception to the general rule. 2. In *suspended animation;* when we cannot otherwise effect with certainty artificial inflation of the lungs. 3. In *spasm of the glottis.* Threatened asphyxia from external injury may depend on this cause—perhaps on a precisely opposite condition; in either case the operation is imperatively demanded to save life. There is the same necessity in the spasmodic occlusion of the glottis which attends poisoning by carbonic acid. In *laryngismus stridulus* we withhold the operation if possible, and trust to general treatment; yet we are aware that urgent circumstances may arise to demand the tracheal wound, at least with the hope of palliation, and perhaps with the hope of affording time for the effectual working of other remedies. 4. In *œdema glottidis,* chronic or acute, there is no safety but by operation, as soon as the symptoms have become at all urgent. And in the acute cases there is the best hope of speedy discontinuance of the tube, closure of the aperture, and complete restoration of normal respiration. 5. In *laryngitis fibrinosa*, the operation is as warrantable as in urgent œdema, when the disease is limited to the larynx. But in the great

majority of cases of true croup, in which the whole windpipe with its ramifications is involved, operation is surely to be regarded as a rare exception to the general rule of non-interference; in the early stage it is inexpedient, while mechanical obstruction to respiration is not yet threatened; in the more advanced period it is ineffectual—failing to fulfil the objects of its performance. 6. In *purulent laryngitis* there is the same necessity for operation and the same good prospect as to its result as in acute œdema. 7. In *chronic laryngitis*, with thickening, supervention of œdema, through inflammatory accession, may render the operation indispensable to the preservation of life. 8. In *simple ulceration* the same event may occur as that just mentioned in connexion with mere thickening of the membrane. Or, independently of the occurrence of such an accidental crisis, the operation may be deemed expedient, to assist the action of other remedial means and by effecting early cicatrization to save structure and function. 9. In *ulceration and disease of the cartilage* operation is likely to be required to save life from threatened asphyxia, but with little or no prospect of discontinuance of the tube's use. 10. In *phthisis laryngea*, it may be similarly demanded for a temporary object, scarcely with a hope of contributing to a cure, but rather as a means of protraction and palliation. 11. In *pressure on the windpipe*, caused by the formation of a tumour or abscess, or by impaction of food or a foreign body in the œsophagus or pharynx, operation may be necessary if the obstruction to respiration cannot be otherwise relieved—namely, by removal of the cause, by evacuation of the matter, extirpation or diminution of the tumour, or extension of the impacted substance. 12. In *cut throat*, tracheotomy is not unfrequently demanded, imperiously, to save life from impending asphyxia, and it may be expedient, at an early period of the case, to avert all such hazard, and to favour as well as permit entire closure of the wound. 13. In *glossitis*, in *tonsillitis*, and in extreme cases of *pharyngitis*, it is required when swelling is so great, rapid, and uncontrollable as otherwise to render fatal asphyxia all but inevitable. 14. In *carotid aneurism* of large size, when by circumstances we are precluded from speedy recourse to deligation of the artery, life may be suddenly brought into peril by supervention of the diffuse form on the circumscribed, and consequent compression of the windpipe. Bronchotomy then is essential; and the tube will require to be worn until by deligation of the artery we have effected such diminution of the bulk of the tumour as altogether to free the respiratory canal. *Thoracic aneurisms*, be it remembered, by compressing the air passages, may stimulate the results of inflammatory disease in the larynx; and in such cases no good can be expected to result from bronchotomy.

"In the great majority of instances tracheotomy is preferable to laryngotomy for obvious reasons." (p. 270.)

From the respiratory we must make a rapid descent to the urino-genital organs, passing by the intermediate parts and their diseases. On the subject of calculous affections we have a chapter occupying about sixty pages, the first nine of which only are allotted to the description and treatment of urinary deposits. This we consider a somewhat meagre allowance for so important a part of pathology, in which the surgeon is equally interested with the physician; and we think a chapter on the principles might with advantage have been dedicated to the subject. The mode of operating recommended by the author in lithotrity is that of Mr. Liston, the greatest authority in the present day on this subject. The following are the directions given for avoiding urinary infiltration, the most serious and frequent of the evil attendants on lithotomy; these, though not new, cannot be too carefully borne in mind:

"To obviate it the following points are of essential importance: Maintain the reflexion of the ileo-vesical fascia entire, at the base of the prostate, that gland being

not divided throughout its whole extent by the knife, but rather first notched, and then dilated by the finger and forceps. Make the general wound conical in form, the base at the integument of the perineum the truncated apex at the prostate. Make the general wound also sloping in form, its fall being from the prostate obliquely downwards—cutting obliquely up to the bladder, not directly into it; also arranging the patient's trunk in bed so as to favour this sloping form, obviously so well calculated for the ready draining away of the urine. In using the finger in dilatation avoid all laceration, torn parts being but ill disposed for rapid plastic exudation. Retain the tube for the necessary number of hours, and keep it clear from coagulum or other source of obstruction. I have latterly thought it advisable to use a somewhat larger tube than that in general use, in the belief that it is better adapted for preventing urinary infiltration; as, while it affords a more ready exit for that fluid, it compresses the track of the deep wound, and may be supposed to afford a very effectual barrier to entrance of urine into the cellular tissue. Further experience of its use is, however, necessary, to determine whether or not it may do harm by delaying the closure of the wound and exciting inflammation at the neck of the bladder. Further, the risk by infiltration is certainly diminished by not operating unless the urinary organs and general system are free from excitement, the kidney acting healthily, and the urine in a satisfactory condition; and also by maintaining after the operation a supply of urine which is bland as well as copious, mainly aqueous, and containing but a sparing amount of saline matter. For if infiltration do occur to some extent, it will be less hazardous to part and system under such circumstances than if the infiltrated urine were the scanty and acrid urine of fever or of renal disease." (p. 460.)

On the subject of the treatment of aneurism by pressure on the artery above the tumour, the author has the following observations:

"Popliteal aneurism is probably the most common of all external aneurisms; and hitherto the Hunterian application of ligature to the superficial femoral has been the only approved mode of treatment. Latterly, however, as formerly explained, (Principles, p. 457,) the application of pressure instead of the ligature has been proposed. And experience is almost daily giving direct and undoubted testimony to the efficacy of the practice. There are some patients, doubtless, who may prove intolerant of pressure, and there may be others who prefer the apparent certainty of the knife and ligature to the apparent uncertainty and delay of the compressor; but the vast majority of cases are assuredly capable of cure by pressure properly applied, without risk, with but little pain or inconvenience, and without any wearisome amount of privation or confinement. The skin which is to bear the pressure is protected by a layer of thick soap plaister, and that again may be covered by leather. And more than one compressor is used, or at least pressure is made at different parts at different times; so that the burthen of it may not all be thrown on one point, but by being subdivided among several points may be rendered much more tolerable. Using several instruments along the course of the vessel in the thigh, they may be slackened and tightened alternately; or the same instrument may be shifted in its site with a like effect. It is never to be forgotten that all severity of pressure is unnecessary — that it is not essential that it should be such as to arrest the arterial flow at the compressed point—that, on the contrary, consolidation of the artery is more likely to take place when a slow, and gentle, and feeding circulation remains. And it is also important to remember that should this mode of treatment fail, it by no means interferes with the subsequent performance of the ordinary operation, but, on the contrary, renders its success all the more probable." (p. 609.)

We had occasion to remark on certain little peculiarities of style in the former volume, which we thought might with advantage be altered. These, if they have not wholly disappeared in the present, appear, we are

bound to say, in a very mitigated form; and we feel no hesitation in recommending Professor Miller's two volumes as affording to the student what the author intended, namely, a complete Text Book of Surgery. Taken altogether, indeed, the work is one of a highly creditable kind, and full of such proofs both of talent and knowledge, as justify us in expecting something of yet higher mark and likelihood from its accomplished author.

ART. XVI.

Leçons sur les Maladies de la Peau, professées à l'Ecole de Médecine de Paris en 1841, 1842, 1843, 1844. Par ALPH. CAZENAVE, Médecin de l'Hôpital Saint Louis, &c. Livraisons I, II, III.—*Paris*, 1845-6.

Lectures on Diseases of the Skin delivered in the College of Medicine at Paris in 1841, 1842, 1843, 1844. *Illustrated by coloured plates.* By ALPH. CAZENAVE, Physician to the Hospital of Saint Louis, &c. Parts I, II, III.—*Paris*, 1845-6. Folio.

WE hail with real pleasure the appearance of the work whose title heads this notice, and which is now in course of publication at Paris. The specimens before us are in every way worthy of the distinguished name which M. Cazenave has acquired as a dermatologist, and do honour to the celebrated institution—the Hospital of Saint Louis—to which the author has been attached, in the capacities of student and physician, for more than twenty years. The illustrations are extremely beautiful as works of art, and are faithful transcripts of the diseases which they are intended to represent. We have had an opportunity of comparing some of these plates with the actual cases, in the Hospital, from which they were drawn, and were gratified with the fidelity of the copies.

M. Cazenave proposes to complete his work in twelve Parts, one to appear every other month, and each Part to contain five coloured plates, folio, with about twenty pages of text, at the moderate price of 10s. It is sufficient for the present merely to direct our readers' attention to the work, reserving for a future period, when it shall be complete, a more extended examination of its contents. Before dismissing it, however, we may state briefly the plan the author proposes to follow. M. Cazenave has taught for several years, both at the Ecole de Médecine and at the Hospital of St. Louis, the necessity of viewing the subject of cutaneous pathology in a more philosophical light than has hitherto been done by those who have studied and practised that branch of medicine. It is not sufficient to know the *character* of an eruption,—to be able to tell whether it belongs to the vesicular, papular, or pustular groups. Although accuracy of diagnosis and a precise nomenclature are elements of the first importance in the study of this class of diseases, still they are not (although the contrary has been too generally believed) the only requisites necessary for forming correct principles of treatment. We must endeavour to disclose the intimate nature of the diseases themselves—as far as they can be ascertained—and thereby arrive at a knowledge of those general laws which regulate their progress and duration. Microscopical anatomy, by giving us more pre-

cise views of the structure of the skin, has materially assisted in dispelling the absurd and empirical notions which have so long degraded this branch of medicine. It has taught us to take a more enlarged view of the subject,—to regard it as an important and integral part of general pathology; and it is through the same medium that we must hope to establish lasting principles on which to found an enlightened and rational method of cure.

As a means of diagnosis, M. Cazenave considers the classification of Willan as modified by Biett, and which the reader will remember is founded on the external character of the disease, to answer all purposes; but here its utility ceases. M. Cazenave himself adopts the following classification, as being more comprehensive and more in accordance with the present state of medical science; but, although he has taught it for several years, he does not put it forth as definitive and complete, for it is impossible to form a lasting classification of cutaneous diseases in the present progressive state of anatomical science.

First Group. *Inflammatory diseases.*

Order I. Non-specific eruptions which may assume an acute or chronic character.

Erythema, Erysipelas, Urticaria, Strophulus, Herpes, Eczema, Pemphigus, Impetigo, Ecthyma, Sycosis.

Order II. Non-specific eruptions which always assume a chronic character.

Rupia, Lepra, Psoriasis, Pytiriasis, Pellagra.

Order III. Acute specific eruptions.

Roseola, Rubeola, Scarlatina, Variola, Vaccinia, Varicella, Miliaria.

Order IV. Chronic specific eruptions.

Syphilides.

Second Group. *Diseases of the secretory apparatus.*

Order I. Lesions of the follicular secretion.

Acne, Porrigo favosa.

Order II. Lesions of the epidermis.

Icthyosis, Horny productions.

Order III. Lesions of the colouring matter.

Loss of colour: Albinismus, Vitiligo. *Changes of colour:* Slate-coloured skin, Ephilides, Nævi.

Third Group. *Hypertrophic diseases.*

Abnormal development of the diseased parts.

Elephantiasis Arabum, Molluscum, Frambæsia, Vernea, Nævi vasculares.

Fourth Group. *Destructive diseases.*

Destructive tendency in the parts affected.

Elephantiasis Grecorum, Aleppo Evil, Cheloidea, Lupus, Cancer.

Fifth Group. *Hemorrhagic diseases.*

Tendency of the blood to exude from the vessels of the skin: hemorrhagic diseases properly so called.

Purpura, Melanosis. (?)

Sixth Group. *Exalted sensibility of the skin.*

General or local Hyperthesia.

Lichen, Prurigo, Anesthesia.

Seventh Group. *Foreign bodies.*

Acarus, Pediculus, Pulex.

Eighth Group.

Diseases of the hair: Alopecia, Canitia, Plica.

Diseases of the nails: Onyxia.

PART SECOND.

Bibliographical Notices.

ART. I.—*First Steps to Anatomy*. By JAMES L. DRUMMOND, M.D., Professor of Anatomy and Physiology in the Royal Belfast Institution.—*London*, 1845. 12mo, pp. 201. With Twelve Plates.

THE *idea* of this little volume is excellent; its aim being "to prepare the young medical student, by some initiatory broad views of the general component parts of the animal frame, that he may thereby be better enabled to enter on the innumerable and minute details of physiology and descriptive anatomy." In many respects the execution is correspondingly good. The style is very simple, as befits the object; and the general arrangement of the subjects well fitted to lead on the student by progressive steps towards the higher and more difficult departments of anatomical and physiological science. The chief fault we have to find in it lies in this: that it is, in many respects, decidedly behind the present state of science. We see no more reason why this fault should be excused in an elementary work than in one intended for the more advanced student; since it appears to us to be above all things essential that those who are entering upon any department of study should be made to *learn* as little as possible what they may afterwards be called upon to unlearn. We must justify our statement by a few extracts:

> "While the principle of life thus resists the decomposing operations of chemistry, we must recollect that it is only the injurious part of chemical action which is resisted, and so far is life inimical to this action, that it is only when life itself would be endangered that it is opposed and forbidden; for many chemical processes are carried on in the living body. Among these I may mention respiration and digestion; but these are all for good, and those chemical affinities only, which would tend to injure or destroy the organization, are forbidden to exert their power." (p. 20.)

We have on various former occasions endeavoured to expose the fallacy of this doctrine, (a very *prevalent* one we admit it to be, but not the more true or philosophical,) that the vital power in any way controls chemical change; and the evidence is continually becoming more definite and irresistible, that all these peculiar powers termed vital (and we may instance those of the muscular and nervous systems as most unequivocally falling under this category) are dependent for their manifestation upon chemical changes, which are destructive to the tissues exhibiting them; just as the manifestation of electric power in a voltaic battery is dependent upon the chemical changes taking place between the metals and fluids in its cells. We shall not now dwell further on this topic, since we could scarcely do so without entering upon the question *in extenso*, which we have already discussed as fully as we deem suitable to the nature of the subject.

In the second chapter we have our old friend the cellular substance; which we are informed, as of old, constitutes "the basis of every solid part of an animal." Surely Dr. Drummond must be aware that anatomists, in this country at least, have now universally agreed to change the old designation of the tissue for one which more satisfactorily expresses its character—namely, *areolar* tissue; and that the real cellular tissues of the body, whose extent and actions have been ascertained by the aid of the microscope, perform functions of far greater importance. The functions of the true areolar tissue we conceive to be purely mechanical; it ties together the different elements of other tissues, in such a manner as to permit them to have a certain degree of movement upon one another; but of its being "the matrix or mould in and by which every structure is imbedded, pervaded, or enclosed," we believe that there is an utter deficiency of proof. In cartilage, for instance, not a trace of it is to be found. In bone it only lines the medullary canals and cancelli, serving to fix the vessels in their places; in teeth it does not extend beyond the pulp cavity, where it answers the same purpose; and there are many other textures of which the same may be said. Nor is it correct to say that it forms the chief bulk of the tendons and ligaments, since, as Messrs. Todd and Bowman have shown, the areolar tissue contains an admixture of two elements—the *white* and the *yellow* fibrous structures; of which the former alone is found in the tendons and non-elastic ligaments, whilst the latter is found only in the elastic ligaments. Again, in the succeeding chapter on fat, not a word is said of the important office to which this tissue is subservient in the living body—that of serving as a reservoir of combustible material, by which our heat is kept up, when from any cause there is a suspension of the supply afforded by the food. The experiments of Chossat prove that, if this reservoir be entirely exhausted, the animal is dependent, from hour to hour, upon the combustible matter ingested and absorbed; and thus, if it were not for such a provision, the loss of a single dinner, to say nothing of a longer abstinence, would be fatal. Some little notice of the more precise views, on these and other subjects, which have of late contributed to raise physiology to a much higher rank among the sciences, would greatly improve the value of Dr. Drummond's treatise, in this and many other departments; and such we hope to see introduced in another edition.

In a concluding chapter, entitled 'Thoughts on Anatomical Education,' Dr. Drummond urges what has often been suggested by ourselves as likely to prove advantageous—that the entire first year's course should be of a general preparatory nature, embracing no one subject in detail, but of a nature to lay a broad and solid foundation for the more special inquiries to be subsequently pursued.

ART. II.—*Insect Life.* By DAVID BADHAM, M.D., late Radcliffe Travelling Fellow of the University of Oxford, &c.—*London*, 1845. 12mo, pp. 171.

THIS little work is nothing else than a new edition of a pamphlet which was published some years since under the title of 'The Question concerning the Sensibility, Intelligence, and Instinctive Actions of Insects,' and which we noticed in our Fifth Volume, (p. 543.) Its object is to prove the startling proposition that *insects have no sensation;* but we do not

perceive that the author has advanced one whit further in his argument than he did before. The following are the assumptions on which it is founded: 1. Sensation is inconceivable as existing apart from intelligence and spontaneous movement. 2. The actions of insects and the organization of their nervous system, indicate that they do not possess intelligence. 3. Therefore, as sensation cannot exist without intelligence, insects have no sensation.

Now we may admit with the author that the greater part of the actions of insects, if not all, are executed without the exercise of any intelligence, or designed adaptation of means to ends, on their own parts, and yet contest the position that sensation cannot exist, and give rise to muscular movements, without intelligence and voluntary power. What, for example, is more involuntary than the act of vomiting? yet this results from sensations with which the intelligence has nothing to do,—as when provoked by an offensive odour, the sight of a disgusting object, the tickling of the fauces with a feather, &c. This is only one of a large group of *consensual* acts, which we regard as having a character distinct from the reflex on the one hand, in that they require sensation as a link in the chain of their performance; and as equally distinct from those prompted by intelligence, in being performed independently of the will, and even in opposition to it.

The fundamental positions taken up by the author being so palpably erroneous, (in our estimation, at least,) we do not think it necessary to enter into a detailed examination of his arguments, or of his supposed facts. We think that we could point out a considerable number of inconsistencies and non-sequiturs in the former; and as to the latter we could almost undertake to show a blunder in every page. We think that the Radcliffe Travelling Fellow cannot have derived much advantage from his appointment, if he can produce no better fruits of it than his "Insect Life."

ART. III.—*The Modern Treatment of Syphilitic Diseases, both Primary and Secondary. Comprising numerous formulæ for the preparation and mode of administration of the new remedies; and an account of a safe and successful mode of treating chronic, protracted, and constitutional syphilis, by the mercurial vapour-bath.* By LANGSTON PARKER, Surgeon to the Queen's Hospital, Birmingham, &c., &c. *Second Edition.—London,* 1845. 8vo. pp. 228.

WE noticed the first edition of this book on two former occasions (Vol. IX., p. 239, Vol. X., p. 381), and gave a very favorable report of its contents. The present edition is considerably enlarged and much improved. It is, indeed, entirely rewritten. The following extract from the preface indicates the character of the principal changes made in the work.

"Six years' additional experience, both in hospital and private practice, has enabled me to confirm the efficacy of most of the plans of treatment recommended in the first edition of this work. Much new matter has been added in the present edition, chiefly, if not altogether, original. That which I regard of the first importance, is the account of the treatment of various forms of syphilis by the mercurial vapour-bath. Diseases rebellious or tedious under ordinary treatments,

generally yield with ease to this combination, more particularly affections of the skin and bones; the duration of treatment is by it much shortened, the quantity of medicine required to be given by the mouth greatly diminished, and the cures rendered more permanent and certain. Relapses after this mode of treatment I have found extremely rare; whereas under the ordinary plan they are exceedingly frequent, even after a perfect cure had been supposed to have been effected."

We strongly recommend this volume to the attention of our practical readers. It is the only recent complete manual we possess, of a moderate size, on the subjects of which it treats, and ought to be in the possession of every young surgeon who is called on to treat venereal affections.

ART. IV.—*A Natural History of the Mammalia.* By G. R. WATERHOUSE, Esq., of the British Museum.—*London,* 1845-6. 8vo, Parts I—VI.

THE brief title, 'A Natural History of the Mammalia,' led us at first to regard this work as not fairly coming within our province; but on turning to the contents of each number as it has appeared in regular monthly sequence, we have been agreeably surprised to find that we had formed too low an estimate of its value, as the plan includes not only the natural history, but also much connected with the osteology and other structures of the mammalia. The parts already published enter fully into the natural history of the marsupialia, and also into the structure of portions of the skeleton, more especially into the zoological character of the skull and teeth, and the general conformation of the brain. The work thus combines details of high interest to the comparative anatomist as well as to the zoologist, details which ought always to be given together; because, by blending these two kinds of information, the value of each is doubled, and each serves to explain and illustrate the peculiarities of the other.

We are glad to observe that Mr. Waterhouse is enriching his work with facts recently brought to light by Professor Owen respecting the fossil mammalia; and also with others derived from the magnificent accumulation of fossil mammalia under his own care in the British Museum. With these means of knowledge, and with his already well-known extensive acquaintance with the recent species, we are satisfied that his work will prove to be one of considerable value to the anatomist as well as to the zoologist. But we must take leave to criticise his style, which is far too diffuse, and requires condensation—more especially in that part of the work which details the habits and natural history. This is an error into which most zoologists are apt to fall, and to which they are almost necessarily conducted by the very nature of many of the objects they have to describe—objects which are known to be quite distinct from each other, yet which frequently afford so few marked peculiarities that it is only by the minutest details that they can be characterized and described in words. We have no doubt that this hint will suffice. We strongly recommend Mr. Waterhouse to adhere closely to his plan of giving full anatomical details of the skeleton, the teeth, skull, and more especially the conformation of the brain; so that these may be commentaries on the instincts and natural history of the class. In a word, let him give us all *the facts* that can be obtained, but condense his style and mode of communication. We shall then have an exceedingly useful and desirable production.

ART. V.—*On Scarlatina and its Successful Treatment by the Acidum Aceticum Dilutum of its Pharmacopœia.* By ISAAC B. BROWN.—*London,* 1846. 8vo, pp. 66.

THIS pretty little book is like the Dead Sea apples—fair without, but naught within. It seems as if it came forth on purpose to illustrate some of the statements in the article "Homœopathy, Allopathy, &c.," in our last number. The good man who writes it, no doubt, conscientiously believes that the cases of scarlatina treated by him, would have all gone wrong but for his "sheet-anchor, acetic acid," and his "other remedies as auxiliaries;" and equally conscientiously, no doubt, did the famous fly in the fable glorify itself for the successful revolution of the wheel whereon it sat. Poor Nature, what wouldst thou have done, without the help of our good doctor? Thy wheel would assuredly have stood still under the pressure of the scarlet plague, but for the oiling of the axle with his acid. And should we marvel thereat, knowing, as we know from the lore of Isaac B. Brown, that, in this wondrous acid, this nepenthe rare, "we have all we want, for destroying the poison of the blood, for allaying the vascular state of the mucous membrane, for preventing hemorrhage from the blood-vessels, astringing the tonsils and fauces, and for checking the febrile action and supplying fresh chyle to the blood." (p. 10.) And yet we marvel at one thing, O Isaac B. Brown, and this is—that with such an all-sufficient, and more than all-sufficient, panacea in thy hand, thou shouldst have needed any auxiliaries, and least of all such auxiliaries as thou soughtest and usedst. Most firmly do we believe that if thou hadst satisfied thyself with the pleasant potions of acidulated syrup and water, thy patients would have all got well quite as fast, and blessed thee for saving them from thy naughty and nasty auxiliaries.

But the leaven of the old man of drugs was too strong in good Isaac Brown, to allow him to be content with the really excellent treatment of the acidulated ptisans,* recommended to him by the "assistant to Mr. George Yates Hunter, of Margate," and thus, therefore, does he torment his poor patients *secundum artem*:—They are carefully purged by calomel, castor oil, rhubarb, and magnesia, their tonsils sponged with a solution of nitrate of silver, their throats rubbed with soap and camphor liniments and laudanum, or surrounded "from ear to ear," with linseed poultices kept constantly applied, their bodies affused with vinegar, &c., the room sprinkled with a solution of chloride of lime, and last, but not least, the patient's strength supported by bark, ether, mutton-broth, brandy, port wine, &c.

How does the author reconcile the two following statements, in pages viii and ix.? In the first he tells us, his treatise is intended for "parents and heads of families," as well as for the "members of the medical profession;" in the next, that there is the utmost danger in any one not "qualified to practise the medical science," "to carry out this or any other plan, *without first obtaining the best advice the locality in which they reside affords.*" Is this, like the rest of his treatise, a bit of *post-hoc-ergo-propter-hoc* reasoning, which he wishes to diffuse among the philosophic fathers and mothers of his "locality?"

* "Diluted distilled vinegar, two drachms; syrup, four drachms; distilled water, two ounces. Mix and take a fourth part every three hours." We would suggest as a great improvement in this formula to quadruple the proportion of water, and then double or yet further increase the amount of the dose!

ART. VI.—*On the present State of Therapeutical Enquiry.* By JAMES ARNOTT, M.D.—*Brighton,* 1845. 12mo. pp. 57; with an Appendix.

DR. JAMES ARNOTT first takes occasion to lament the great difficulty the therapeutical discoverer experiences in obtaining a notice. This he attributes principally to the frequent disappointments practitioners experience in the use of new and much praised remedies; the remedies being, in fact, inadequate, because recommended from false analogies or imperfect observation. Dr. Arnott then proceeds to consider, firstly, the principal sources of therapeutical knowledge; secondly, to what degree medical theory has in this respect proved useful; and thirdly, the present defective mode of reporting the results of medical experience, and how it may be amended.

Under the first head, Dr. Arnott indicates the principal sources of therapeutical knowledge to be,—the accidental observation of the effects of remedies; the direct calls of instinct for some remedial means, as in wounds, &c., from mechanical injuries; the artistic or technical perfection of remedies thus discovered; and lastly, the analogy between the observed action of certain medicines by which the one has been substituted for the other. Dr. Arnott gives various interesting examples of remedies from these various sources.

With regard to the influence which medical theories have exercised on the progress of therapeutics, Dr. Arnott expresses an unfavorable opinion. If the explanation of the morbid condition, he observes, rest solely on mechanical or chemical principles, the appropriate remedy may originate from such explanation; but what is called the theory of disease usually implies a consideration of the laws of vitality, and, in this sense of the term, it would be easier to show the mischief which theory has occasioned, than to collect the instances of its direct advantage. It has been the source of many dangerous practices, and by the prejudices it has excited, and the conventionalism and routinism it has developed, the practitioner has been prevented acquiring and adopting better plans of treatment. Dr. Arnott thinks that medical prophylaxis has been more advanced of late years than therapeutics. In this opinion we fully concur, and we think that it may be taken as a strong, and, indeed, irresistible proof, of the great advance made in medical science and art in our own times.

Theory has, however, its value; indeed all our greatest practitioners, whether physicians or surgeons, have been theoretical. The faculty to discover necessarily implies a speculative mind. Now a speculative mind is in fact one which collects and meditates on facts, if so be that it be rightly trained. It is only absurdly speculative when it leaves facts altogether to build theories on hypotheses—a series of unsubstantial erections which vanish into nothingness so soon as the speculator is beckoned from his airy height to the regions of plain common sense and utility. A truly practical man is he who continually endeavours to *adapt* his speculative ideas to the cases of disease that come daily under his notice.

Now we really must give Dr. Arnott the credit of being a speculative man of this class. In his little book the practitioner will find a closely-packed repository of useful inventions. Many of these are eminently

ingenious, and based on sound principles. It is, however, in medico-hydraulical mechanics that Dr. James Arnott most distinguishes himself, and we have various ingenious modes for using fluids in the treatment of disease, whether for the dilatation, compression, or regulation of the temperature of parts.

We cannot undertake even to enumerate the different ingenious uses to which fluids, and especially water, are applied by Dr. James Arnott, but assure the practitioner, that the outlay of the trifle which the little book may cost, will be a most excellent expenditure of capital. In addition to the description of various new therapeutical methods, the appendix is more especially devoted to improvements in the obstetric art, many of which are worthy the accoucheur's consideration. Invention seems characteristic of clan Arnott. The present author treads the same path—though at some distance behind his more famous brother Neill.

We would suggest to Dr. James Arnott the expediency of enlarging and somewhat altering the plan of his work in another edition, by noticing at greater length, and on a wider scale, the strictly medical part of his subject. We hope the free-spoken article in our last number may encourage him to tread less gingerly on the toes of ancient prejudices.

ART. VII.—*Elements of Anatomy.* By JONES QUAIN, M.D. Fifth Edition. Edited by RICHARD QUAIN and WILLIAM SHARPEY, M.D., Professors of Anatomy and Physiology, University College, London. Illustrated by numerous engravings on wood. Parts I, II.—*London*, 1843, 1846. 8vo, pp. 1063.

IT would seem as if the very fact of the division of a work into parts, and the separate publication of these, exerted an injurious influence on the minds of the parties concerned, inducing procrastination and forgetfulness of promises, and consequently disappointing all the hopes which credulous buyers had founded on the promises liberally put forth in the first announcement. It has certainly become the *exception*, rather than the *rule*, for a work published in parts to be completed in anything like the time originally intended; and the work before us cannot, we regret to say, claim exemption from the censure we have heretofore administered in similar cases. In fact, we are constrained to remark, that we think there is less excuse when a new edition of a work previously complete is concerned than where the entire work is new. Surely the editors might form some tolerable estimate, in the first instance, of the amount of alteration that would be required, and might withhold the publication of any part until the remainder were in an advanced condition. The first part of this new edition of Dr. Quain's Anatomy was issued in the autumn of 1843, with the promise that the remainder should be published "as soon as possible." The second part has only made its appearance within these few weeks, consequently, nearly two years and a half after the first, and it does not complete the work. By way of apology, we are told that "the changes made are much more extensive than had been originally contemplated. In fact the principal part of this volume has been written anew." Our question is,—why were not these changes *originally* con-

templated,—or, in other words, why did not the editors decide upon the amount of change they would introduce *before* they proceeded with the first part? Anatomy is not a science of such rapid progress as to require the complete remodelling of a previously good treatise every three or four years; and we cannot but think, that such highly-qualified editors might have more clearly divined their path at the outset of their course, and that, even if they found it requisite to deviate from it here and there, the delay need not have been quite so long. The evil is much greater in regard to a work especially destined for the student, than with respect to one which is chiefly intended for the established practitioner. The youth who purchased the first part, as the guide of his anatomical studies,—presuming, as he might fairly do in such a case, that the remainder would appear within the session, or at furthest at the commencement of the next session,—after spending three or four years in attendance on lectures, finds himself still the possessor of an incomplete work, and the last part makes its appearance just when it is becoming comparatively useless to him.

It pains us greatly to be continually obliged to recur to this subject, by the misdoings of medical authors; more particularly when we have to direct our censure upon those who in every other respect deserve the highest praise at our hands. We have no hesitation in saying that the work before us, completed (as we feel confident it will be) in the spirit in which it has been thus far carried on, will be alike worthy of the fame of its distinguished editors, and valuable to the anatomical student. The former editions of Dr. Quain's 'Elements' had attained a high reputation, as having successfully avoided the useless minuteness of some treatises, and the superficial curtness of others; laying deserved stress upon those points which enter into the consideration of practical questions, guiding the physician in his diagnosis and treatment, and directing the judgment and hand of the surgeon, and, at the same time, giving all needful information upon other matters of less obvious utility. The chief advances made, since the publication of the fourth edition, have been in the department of general anatomy. The whole of this has been rewritten by Dr. Sharpey, and forms an introduction, separately paged, illustrated by a large number of admirably executed wood engravings. It is now a most admirable and concise exposition of that subject; and we should much like to see it issued separately, for the sake of those of our brethren more advanced in life, who desire to make themselves acquainted with what has been recently effected by the aid of the microscope and of chemical analysis, in regard to the elementary structure of the tissues, and who do not require to possess themselves of a general treatise on anatomy. The purely anatomical portion of the work has been chiefly revised by Mr. Quain; but we think we discern, in the alterations which have been made in the account of the nervous centres, the indications of Dr. Sharpey's physiological mode of treating this difficult subject; at any rate, this portion of the work is remarkably well done.

We shall give a more detailed account of the work when it shall be completed, by which time we hope that Messrs. Todd and Bowman's Physiological Anatomy may be in the same condition, so that we may review them together, and point out their respective excellencies.

PART THIRD.

CONTRIBUTIONS TOWARDS THE ADVANCEMENT OF THE Natural History and Treatment of Diseases.

I. PREFATORY NOTICE. BY THE EDITOR.

THE article in the last Number of this Journal, entitled "Homœopathy, Allopathy, &c.," as was, perhaps, to be expected from its subject and object, has attracted a more than ordinary degree of attention from the profession. The Editor has, in consequence, been favoured with numerous communications relating to it from his friends in all parts of this country, as well as from the continent of Europe, and from America. The views of the writers are, on the whole, in remarkable accordance with those given in the article; and the opinion is generally and strongly expressed, that some such changes in the manner of studying and treating diseases as are there propounded, are absolutely necessary for the successful progress of rational medicine. So convinced is the Editor (the author of the article in question) of the truth and importance of this subject generally, that he now offers to open the pages of his Journal for its special consideration, if it shall be found that the warm interest at present excited by it, proves to be deep and strong enough to supply materials for its continuous elucidation and advancement. This is doubted. Still it has been thought advisable to evince practically and without delay, the earnestness with which the subject has been taken up and is sought to be promoted by the Editor. From among the numerous contributions with which he has been already favoured, two have, with the sanction of their authors, been selected for publication in the present Number. Whether these shall be followed by others, in subsequent numbers, will, as just stated, depend on the inclination of the profession. It will afford the Editor much satisfaction to be able to present to his readers any authentic communications of the same high stamp and admirable tendency as those now submitted to their notice.

Before concluding this prefatory notice, the author of the article on Homœopathy and Allopathy desires to make a few brief observations in reference to one or two points in it, on which he finds he has been somewhat misunderstood. The misconception is admitted by him to be natural on the part of his readers, in consequence of the necessity under which he found himself, for want both of room and time, of leaving all the latter part of the article a mere sketch or outline. Had he completed the whole subject on the same scale as was allowed to the exposition of the character and fallacies of homœopathy, he believes that he would have left no grounds for the friendly criticisms and gentle remonstrances which some of his very estimable and enlightened correspondents have addressed to him. Most assuredly, in this case, he would have run no risk of being set down—as he finds he has been

set down by a few of his more careless readers—as favouring homœopathy; as denouncing rational therapeutics on the old system; as sceptical of the powers of all medicines; and as desirous of having diseases generally abandoned to the influences of Nature.

With regard to the charge of favouring homœopathy, it is submitted that it is the first duty of the historian to give an impartial narrative of events and a fair exposition of doctrines, without prejudice or affection. This it is believed was done. And if full credit was given by the author of the article to the statements in the documents submitted to his consideration, and full justice rendered to the characters of the individuals concerned, this was done—simply because it was felt to be just and right so to do. The writer might be mistaken in the estimate he formed of the value of the premises, but having conscientiously formed that estimate, its expression was a matter of course. The obligation of candour, imposed on all inquirers, was the more imperative in the present case, inasmuch as the writer felt that he stood in the position of a declared antagonist. And he knew, moreover, that anything approaching to unfairness could, in the end, only tend to injure its author and the cause he advocated. The writer was, indeed, too profoundly impressed with the reality and value of the ordinary system of medicine (miscalled in the Article *allopathy*, to avoid circumlocution) to entertain any fears from conceding to homœopathy everything which it could possibly claim of right. And he cannot help indulging the belief that the arguments adduced against the validity of its doctrines will be found to be more efficacious than many previously urged, simply because more candour has been shown in examining these doctrines, and more justice done to the characters of the men who have originated and professed them.

Probably the statement in an early part of the article, to the effect that, "homœopathy might be the remote if not the immediate cause of more important fundamental changes in the practice of the healing art, than have resulted from any promulgated since the days of Galen himself," made an impression on some readers who did not attend to the qualifications afterwards given. The "fundamental changes" alluded to had exclusive reference to the indirect or negative operation of homœopathic practice, in furnishing us with "the means of instituting a grand natural experiment in therapeutics," by leaving diseases really to nature, though nominally to medicinal treatment. And it is a profound conviction of the great importance of the knowledge to be thus obtained, which makes the writer now reiterate the statement, as well as another in a subsequent part of the article to the same effect, viz., "that the doctrine of Hahnemann will have [*thus*] conferred an inestimable benefit on the healing art." It never was intended to be conveyed as the opinion of the author, that the homœopathic treatment was valuable in itself, or that the system of rational eclectic medicine advocated by him, could derive any direct benefit from homœopathy by the adoption of any part of its so-called medicinal therapeutics.

But, the writer thinks, moreover, that the declaration of his opinions, in more than one part of his paper, was sufficiently positive, clear, and forcible, to secure him from the charge of favoring homœopathy in any way, or of over-estimating it at the expense of rational medicine. For instance, having in the course of the investigation come to the conclusion that homœopathy is to be utterly rejected and the old faith still adhered to, he added: "In doing so, we consider that though we are embracing a system extremely imperfect, we are, at least, embracing one which, with all its faults, contains a considerable amount of truth, and a yet greater amount of good; and which, above all, is, or may be made, in its exercise, consonant with the principles of science, and is capable of indefinite improvement; while, *in rejecting homœopathy, we consider that we are discarding what is, at once, false and bad—useless to the sufferer and degrading to the physician.*" (p. 250.)

And again: "It may possibly be inferred that we are entirely sceptical of the truth of medicine as a science, and think most meanly of it as a practical art. And yet this is not so. On the contrary, we look upon medicine, regarded in all its parts and all its bearings, as a noble and glorious profession, even in its present most imperfect state; and we believe it destined to become as truly grand and glorious in actual performance, as it now is in its essence, its aims, and its aspirations." (p. 259.)

A more specific charge has been made against the author for admitting, without sufficient corroborative evidence from other quarters, the validity of the results given in Dr. Fleischmann's Report. He anticipated this charge, and endeavoured to guard himself, in some degree, against it, by stating that while he allowed Dr. Fleischmann to be a man of honour and respectability, and incapable of attesting a falsehood, he could only admit his statements "with that liberal subtraction from the favorable side of the equation, which is required in the case of all statements made by the disciples and advocates of new doctrines." (p. 242.) The "subtraction" necessary in every individual case, assuming the narrators to be honest, will depend on many circumstances, both personal and relative. The author was and is disposed to make it very ample in the present case—so ample, he believes, as would deprive Dr. Fleischmann and homœopathy of all the glory they have derived from this flattering record. Nevertheless, he here repeats the assertion that, "even after this rectification, enough remains to justify the inference that these tables substantiate the momentous fact, that all our ordinary curable diseases are cured [*i. e.* get well] in a fair proportion, under the homœopathic method of treatment." (p. 242.) The expression "fair proportion" is very indefinite, and therefore improper; it was meant to have reference to the results obtained under the *more general or common* modes of treatment pursued by practitioners and which it was one main object of the article to denounce. It never was stated, nor was it in any degree believed, that the results obtainable and obtained under homœopathic treatment were equally favorable with those obtainable and obtained under an *improved system* of eclectic treatment, which, it was admitted, was now practised by many, and the more general extension of which among practitioners it was the great aim of the paper to promote.

The writer of the article, however, may now state that, since the publication of his paper, he has received from one of our most distinguished clinical physicians a corroboration of a part of Dr. Fleischmann's report which has been regarded with much scepticism. This gentleman reports that he has over and over again proved the complete curability of *pneumonia* when left to the powers of nature, in cases under his own immediate observation. These cases occurred in persons who had come into the hospital in so advanced a stage of the disease, and with symptoms so favorable, as not only to justify but to render imperative the propriety of not subjecting them to any formal treatment. And the result, as stated, was perfect recovery.

In dwelling so long, and expressing himself so strongly, on the curative powers of Nature, the design of the author of the article was to remove all rational doubt of the capacity of the *vis medicatrix* to effect all that was effected under the imaginary influence of homœopathy. And he had, also, the great collateral object in view, of doing his best to stay the plagues of polypharmacy, medical heroism, and many other kinds of *hurtful* interference with the progress of disease, which he believed and believes to be widely diffused in practice, and which loudly demand the attention of all who wish well to the best interests of the medical profession and the public. And he still thinks that, with so legitimate a purpose, he made no statements that are not only justifiable but imperiously called for. Nothing, however, could be

further from his intention than to advocate the leaving of diseases to Nature, as a general rule, or as a rule at all; or to deny the great value aud efficacy of remedies, whether drugs or other means, in the treatment of many of them. The whole of his argument and invective was against *the abuse*, not the rational and legitimate use of medicines and medical treatment. So far from wishing all diseases left entirely to Nature, the author of the article has the strongest conviction that none ought to be so left; and that there is not a single case, whatever be its nature, and whether curable or not, that will not be benefited, more or less, by the careful superintendence of the scientific physician. For a man thoroughly acquainted with diseases in all their relations there will always be enough to do in regulating, as far as is practicable, the manifold circumstances and conditions amid which the sick are placed, even if the individual case should require little or no medicine in the form of drugs. It is a most mean and circumscribed view of therapeutics, to regard it as almost identical with the use of drugs. So far is this from being the case, that it may almost be said to be the perfection of medical treatment to cure diseases without drugs, or with the smallest possible amount of drugs; as it is now allowed to be the perfection of surgery, not to perform operations but to do away with their necessity. And yet what an almost boundless variety of things, exclusively of drugs, call for the notice, and attention, and consideration, and discrimination, and appreciation of the man who is held responsible for the proper management of a severe disease, whether acute or chronic? It would, indeed, be strange if a task like this were either facile or of slight importance; that it could be safely intrusted to any one not deeply versed in all the better parts of medical science; or that its well or ill performance were a matter of indifference to the patient.

But it never was imagined, much less contended for by the author of the article, that the treatment of diseases should or could be exclusively left, in all cases, or even in any considerable proportion of cases, to this hygienic or non-medicinal system of management; or that we were not possessed of many drugs of the noblest powers, and of known and unquestioned efficacy in the mitigation and cure of many diseases. To do this would be, in the first place, wilfully to reject evidence of the most positive kind; and, in the second place, wilfully to encounter disease with only half our stock of arms and ammunition. And it would seem hardly less irrational for a physician of any experience to deny the efficacy of such general means as venesection, emetics, purgatives, or the individual power of such drugs as opium, mercury, iodine, quinine, iron, colchicum, &c., in controlling diseases, than for the soldier to denounce as useless the very weapons which had often enabled him to vanquish and overcome. Here, as in the former case, the writer's anathemas were directed against the abuse or improper use of the medicines, not against their proper use. In making this explanation, however, he must still avow his belief that of a very large proportion of the drugs in common use we are entirely ignorant of the action, or whether they have any action or not; that many are certainly useless and others injurious; that of the number which possess undoubted powers in modifying the condition of the animal body, in health and disease, we have yet to learn the exact mode of action, and the amount of remedial efficacy; and that in the administration of our very best medicaments we are absurdly profuse, and thus, not seldom, convert into an evil what Nature intended for, and Science was capable of applying as a blessing.

II. ON THE OBSERVATION OF NATURE IN THE TREATMENT OF DISEASE.

By Andrew Combe, M.D.,
Fellow of the Royal College of Physicians of Edinburgh, one of the Physicians in Ordinary, in Scotland, to the Queen, &c. &c.

IN A LETTER TO THE EDITOR.

Edinburgh, 25th January, 1846.

My dear Sir,—I have now carefully reperused your article in the recent Number of the 'British and Foreign Medical Review,' and rejoice that you have spoken out openly and honestly what you believe to be truth, regarding both homœopathy and what is antithetically but incorrectly called allopathy; and as I consider a consciousness of our faults to be the first step to improvement, in medicine as well as in morals, I feel no regret that you have made the confession so complete. In all probability you will be attacked for having spoken too disparagingly of "allopathy," and too favorably of homœopathy; but the result of discussion will be to extend the consciousness of such defects as are real, and to prompt to their removal, so that truth and the interests of humanity will gain by the course you have pursued. If I were to judge merely from the general tone of your remarks, I should say that you *have* exaggerated the defects of ordinary medicine, and that this has arisen from your not having been sufficiently careful to distinguish between its essence, and what may be more justly termed its errors and imperfections. Writing, as you did, for the express purpose of directing attention to the defects of ordinary medical practice, with a view to future improvement, you were in a manner forced to give them a prominence which was calculated to throw the real solidity of its foundation and the best part of its superstructure into the shade, in the minds of those readers at least who had not previously thought much on the subject, and hence were unlikely to discriminate for themselves. But, disregarding minor inaccuracies of expression and of opinion, and looking to essentials only, I should say that although, from the above causes, it might be easy to quote detached sentences from your review, which betoken an utter want of faith in medical art, yet it is evident that such was not your meaning, and that you have perhaps a stronger sense of its truth, beneficence, and importance to mankind, than many of those who will blame you for your broad avowal of its faults.

If you had been careful to make the necessary distinction between medicine as a practical science, based on the laws of nature, and only requiring the steadier application of sound principle to its cultivation, to lead to more certain and beneficial results, and that *soi-disant* medicine practised by so many of its votaries without regard to principles of any kind, and consequently often involving in its train no small amount of mischievous as well as merely negative consequences—between medicine, in short, and the abuse of medicine—and restricted your condemnation to the latter alone, you would have given less room for misapprehension and difference of opinion, and at the same time rendered your article both more acceptable and more instructive. Even as it is, however, you have not left yourself altogether defenceless. In two or three passages you admit, cursorily indeed but distinctly enough, that the defects you complain of are not inherent in, and inseparable from, medicine itself, but are only attributable to a large portion of the prevailing practice; and while you denounce unsparingly the faults and omissions of its disciples,

you are so far from being sceptical of medicine itself that you pronounce it to be, even in its present state, "a noble and glorious profession," and destined to become as truly "grand and glorious in actual performance, *as it is now in its essence, its aims, and its aspirations.*"

Believing your censure, then, to be directed against the faults of medical science as at present practised, and not against its principles and truths, I am so far from thinking it undeserved that I have been for years impressed with similar convictions to your own regarding them, and felt an earnest desire to contribute any little aid I could towards their correction and removal. From an early period of my professional life, I was struck with the exclusiveness with which relief was generally sought from drugs or active treatment, and the indifference with which the *lædentia* were often allowed to remain in undisturbed operation, and the *juvantia* left entirely to chance or the whim of the moment. And yet experience demonstrates that, in the great majority of cases, the drug is only one influence among many, and that it is by the intelligent regulation of these external conditions, far more than by active medication, that the physician can effectually contribute to the comfort and recovery of the patient. Disease is a perverted state of a natural organic action, and not a something thrown into the system by accident, and which obeys no fixed laws. In the cure of disease, therefore, the business of the physician is not to supersede Nature, but carefully to observe what is wrong, and to aid the efforts made by her to re-establish regularity and order. Accordingly, experience shows that the physician and the remedy are useful only when they act in accordance with the laws of the constitution and the intentions of Nature; and hence, in chronic and even in acute diseases, the most effective part of the treatment is generally the hygienic, or that which consists in placing all the organs under the most favorable circumstances for the adequate exercise of their respective functions. If this be done systematically, every effort of Nature will be towards the restoration of health; and all that she demands from us in addition, is to remove impediments and facilitate her acts.

So far, however, is this from being the prevailing view of the proper sphere and duties of the physician, that even many medical men habitually act and speak as if they considered their only business to be the prescription of drugs, or some active external remedy, such as a blister or a bleeding; and in ordinary medical education, no attempt whatever is made to direct the attention of the student to the value of preservative or preventive treatment, or to those important auxiliaries to recovery from illness which it is the province of hygiène to unfold. The consequence of thus considering drugs as our only or chief resource is, that, when called to the bedside, we are apt to fix our attention exclusively upon the prominent symptoms, and allow obstacles to recovery to continue in operation or start up unsuspected, which often go far to counteract the best devised and most active treatment. This is the more to be regretted because the practitioner himself is, or ought to be, the source of one of the most powerful and beneficial hygienic influences to which a patient can be subjected. Taking a high and just view of his position, his aim ought to be, on all occasions, to procure for the family of which he is the confidential adviser the highest health of which it is capable; and had the public, on their side, a just sense of his duties, they would resort to him for advice not only during actual illness, but regarding the management of their own health, and the education and management of their children. As a general rule, however, the practitioner attends only to the individual *sick* member to whom he is called in; and, so far from taking cognizance of the causes of disease amidst which the family may be living, he, unless specially called upon, rarely thinks of laying down precautionary rules for the *future* guidance even of the one who is ill. He prescribes for him, and the present

attack once over he leaves him to the mercy of accident, to sink or swim as chance may direct. Nay, if we look into the families of medical men themselves, it is rather an exception to see any rational precautionary treatment in systematic use, or any one advantage secured, which an acquaintance with hygiène would suggest as worthy of our attention. Breathing vitiated air, for example, is universally known to be one of the most active causes of scrofula, and yet I have more than once seen the scrofulous offspring of otherwise sensible and well-informed medical men sleeping three or four in a small room, with closed curtains around their beds, while an unoccupied well-aired room was close at hand, and reserved perhaps for a stranger. Again, it is by no means uncommon to see the children of medical men suffer in health from habitual and indiscriminate indulgence of the appetite, neglect of air and exercise, and over-working of the brain, without even an attempt being made to prevent the evil by the adoption of a better regimen; and if they thus, from indolent indifference or practical blindness, neglect the protection of their own flesh and blood from evils which may be guarded against, how can we expect them to feel any interest, or use any foresight, in protecting the children of others? It is not indifference, however, that causes this inattention. It is simply that *they have never been taught* that such concern is a duty incumbent on professional men; and they have never been so taught because hygiène has ignorantly been considered to be a subject which concerned nobody but old women and hypochondriacs, instead of being, as it is in reality, both in its preservative and in its therapeutical applications, one of the most important and most beneficial elements of our professional knowledge.

It was a deep sense of the evils resulting from this state of things which led me, about sixteen years ago, to begin the preparation of the first of the three works I have since published for the purpose of inculcating the importance of physiological and hygienic information both to the public and to the profession. It was the same deep conviction which induced me in 1838, in a letter on medical education to our friend Sir James Clark which was printed but not published, to comment strongly upon some of these evils and their origin in the defective state of professional education. And it was under the same feeling that, in January 1842, when disabled from practice by the illness under which I still labour, I became so desirous to rouse attention to the subject that I embodied an outline of my views in a letter to my brother, Mr. George Combe, then resident in Germany, to be made use of by him, if I did not survive to bring them out in a more satisfactory form. This, I have never yet been able to do, although I have never lost sight of the subject; and, in this state of matters, perhaps the best answer I can now make to your appeal to your brethren, for suggestions in aid of your own efforts, is to lay that letter before you. To yourself, its perusal will be gratifying, as exhibiting the pre-existence of views in many respects in harmony with your own, as to the present state and future prospects of medicine; while to your readers, also, it may not be without interest as affording in so far a presumption of the truth of the principles which have led us both, by almost parallel although different roads, to nearly the same conclusions concerning the sources from which the improvement of medical practice is to be obtained. But to bring out this coincidence more clearly, it will be useful, before subjoining the letter, to notice briefly the analogy which subsists between its objects and the results at which you have yourself arrived.

The aim I had chiefly in view in writing that letter was to show that, before undertaking the treatment of disease, we ought to make ourselves acquainted, 1st, with the laws which regulate the action of the different bodily organs during health; and 2dly, with the *natural history or course of diseases*, that we may be able to read aright the indications of Nature in their treatment, and

take special care neither to counteract her efforts nor to substitute another method of cure for hers, unless where we have positive evidence that the *vis medicatrix*, judiciously aided by us, will prove unavailing. I then proceeded to show that it is from losing sight of the order and indications of Nature, and neglecting the aid to be obtained from their observance, that no small portion of ordinary medical practice is fallacious, and some of it even hurtful, from actually interposing obstacles to the operation of the restorative power inherent in the living organism. In other words, I wished to direct attention in a special degree to the propositions, that *Nature is the active agent in the cure of disease*, as well as in carrying on the ordinary operations of life; that the physician can never be so well employed as when he acts intelligently and consistently as the *Naturæ minister et interpres;* and that there is small wisdom and less glory to be obtained from attempting either to substitute other devices for her arrangements, or to put her to the rout by main force, and by the use of means at variance with her laws.

Your propositions, on the other hand, are:—" 1st. That in a large proportion of cases, as at present treated, the disease is cured by nature and not by the physician. 2d. That in a lesser, but still not a small, proportion the disease is cured by nature in spite of him; in other words, his interference opposing instead of assisting the cure. And, lastly, that, consequently, in a considerable proportion of diseases it would fare as well, or better, with patients in the actual condition of the medical art, as more generally practised, if all remedies, at least all active remedies and more especially drugs, were abandoned."

Such are the inferences which you have deduced from a general survey of medical practice, and I need hardly stop to point out how completely they harmonise with the conclusions at which I had arrived. This will be apparent to every reflecting reader, and will become still more so after perusing the letter in which my views are more fully developed. Here, however, it will be useful to remark that an omission occurs in your article which will tend to the further diffusion of a misapprehension already too prevalent on a point of vital importance. When you speak of "leaving the patient to the efforts of nature," as the alternative of abandoning what is usually called "active medical treatment," you, in common with most other writers, unguardedly use the phrase as if "trusting to nature" were equivalent to consigning the patient to chance or caprice for his guidance, to do anything or nothing exactly as the feeling or whim of the moment may suggest. This, is at once an erroneous and a pernicious interpretation, because it is calculated to lead to indifference and carelessness, under the very circumstances in which vigilance and discriminating attention may prove most useful. So far from sanctioning inactivity on our part, an intelligent reliance on nature implies that we shall exercise, throughout the whole course of the disease, the most watchful observation over its phenomena and progress, and not only timeously remove obstacles which may interfere with its proper course, but rigidly fulfil all the conditions which a sound physiology shows to be most conducive to the well-being of the various bodily organs, and to their restoration when disordered. In this way the physician may often exercise the most salutary influence, nay, even be the means of saving the patient's life, and yet not give one particle of medicine. In No. 14 of your *excogitanda*, and in a few other places, all this is virtually implied; but you nowhere bring out the principle with that clear and prominent distinctness which it so eminently deserves, and which is so essential to a right understanding of the question in dispute between homœopathy and ordinary medicine. I regret this omission, because the more I see, the more I am convinced that it is only by the intelligent observation and study of nature that a sound system of medical doctrine can be obtained, or medical practice

ever be made to confer those benefits on mankind which it seems to me fully capable of rendering. In proportion as we proceed upon this solid basis, every step taken will be in advance, and every new discovery in physiology, anatomy, chemistry, &c., will tend to enlarge our power over disease, by enabling us more and more to give Nature fair play. Instead, therefore, of medicine being superseded, as many suppose, by taking Nature for our guide, it will, on the contrary, only begin to take just rank as a science, when our allegiance to Nature shall become practical, enlightened, and complete. One word more of preface. In reading the subjoined letter you ought to keep in mind that it was written merely as a private record of my opinions, and consequently bears the marks of haste both in its arrangement and language. If it had been intended for the public eye, I should perhaps have modified some parts of it, and been more careful in others to guard against any misapprehension which might arise from the imperfect development of my views. But as these are blemishes which do not affect the accuracy of its general statements or reasoning, and which I may safely leave to the indulgence of the reader, and as it is desirable to show the pre-existence of my opinions, such as they are, I shall not venture to make any alterations by way of removing the defects.

"*To George Combe, Esq., Mannheim.*

"My dear George, Edinburgh, 8th January, 1842.

"The great defect in the study of medicine, and in all investigations connected with it, at present, seems to me to consist in the nearly total absence of guiding principles, and in the neglect of the great rules of Bacon, and more especially of *the observation of Nature*, as the only solid foundation on which medicine or any other science can rest and advance towards perfection. This last will seem to many of my brethren a very singular charge, because if there is one circumstance on which the profession prides itself more than another at this moment, it is on the ardour with which observation is pursued and facts are sought for. Nevertheless, I believe the charge to be supported by incontrovertible evidence, and I attribute the small progress really made to this very truth. I admit that, everywhere, observations are made and facts stored up with an industry, accuracy, and zeal, which, under better guidance, would soon accomplish great things. But these observations and facts are incomplete, and therefore partial, and, if relied upon, apt to mislead. They are *phenomena* or *occurrences* rather than ultimate facts, and, their conditions and relations remaining unknown and unconsidered, they lead to no useful results. Hence, the multitude of observations daily recorded in the writings of medical men serve more to oppress the memory and puzzle the inquiring mind than to advance science and improve practice. Hence, too, the thousand and one facts of the one year disappear under the shade of the thousand and one newer facts of the succeeding year. *All*, indeed, are not of this description. Some few out of the many are *complete* facts, and have a meaning which becomes daily brighter, and bears a direct relation to practice. The sum of these constitutes the real amount of the progress made by medical science, and the proverbial uncertainty of medicine affords a pretty accurate indication of the relative amount of incomplete or false facts gathered into the granary.

"The grand object of medicine is to preserve and restore the healthy action of all the different organs and functions of the human body, so as to ensure their efficiency, and fit the individual for the successful discharge of the duties devolving upon him as a created being and a member of society. Here, then, the first step to be taken is obviously to become acquainted with the mechanism of the body, the structure of its constituent organs, the conditions or laws under which these act, the purposes which they respectively serve in the animal economy, and the relations in which they stand to each other, and to

the external agents by which man is surrounded and acted upon from the moment of conception down to his latest breath. In other words, the first step towards rational principles of cure must consist in *a knowledge of the laws of the healthy functions.* The second ought to be the observation of the manner in which the various disturbing causes act upon the different functions, and the *kind, course,* duration, and termination, of the morbid action which they produce. Having investigated these points we become qualified to inquire, in the next place, what circumstances will best favour the intentions of Nature, and remove the obstacles which may have arisen to impede or thwart her efforts. To succeed in these aims, or even to make a rational attempt at succeeding, we must be profoundly impressed, or I may say *saturated,* with the great principle or truth, that all the operations and actions of the living body, whether healthy or morbid, take place according to *fixed and discoverable laws,* and that God has left nothing to chance. With this grand fact before us, it becomes palpably evident that we can do nothing rational, in the way of either prevention or cure, except in so far as we act in accordance with these laws. Many medical men have, however, a very different impression from this. A good physician will always seek to be, and never aim at being more than, Bacon's '*servant and interpreter of Nature.*' A greater than he created man and ordained the laws of his being, and no surer road can be found than that traced by the hand of his Creator. Overlooking this truth, and viewing disease as an entity ungoverned by any definite laws, and not destined to run through any definite course, many medical men talk as familiarly of *their* 'curing' and 'arresting disease' as if they had an absolute control over the whole animal functions, and could alter their laws of action at pleasure. To my mind, no clearer proof of presumption and philosophic ignorance can be found than this usurpation of the prerogatives of Deity; and its results are often very unsatisfactory.

"That there are forms of disease in which a determinate nature and course cannot be easily traced is quite true; but there *are* many more in which the natural course is as obvious as that of the sun. Take the familiar example of cowpox, smallpox, fever, or ague. The disease is regulated by fixed laws in such a palpable manner, that every medical book describes with perfect accuracy the appearances which each will present on given days of its progress in an average constitution. The same holds with measles, scarlatina, and many other acute affections; and less clearly, but still perceptibly enough, with gout, rheumatism, and inflammation. All of these go through a regular course, in a shorter or longer time; and when everything goes according to rule, we feel assured that the constitution is safer than where some unusual accident has interrupted the natural progress of events. This, be it observed, is the course towards health which the Creator in constituting man considered best for him; and the wisest thing we can do is to act in accordance with it, and seek only to remove impediments. It is not we to whom the cure is intrusted, or by whom it is effected. The Creator has perfected all the arrangements for that purpose, and our sole business ought to be to give these arrangements full play. Man, however, is too full of his own importance to view things in this light. He wishes to be master, and to control disease by his own act, and accordingly he has in all ages been seeking for the means of 'arresting' disease at its onset. Not many years ago the cold affusion was in this way in high vogue for *cutting short* fever, and its praises were loudly sounded. Gregory applied it even in scarlet fever, and I rather think in measles. In many cases Nature was so far vanquished, by repeated cold drenching, that the disease was apparently cut short during the commotion, and, (probably from relief being obtained through another channel,) without visible bad effects. But in many more, Nature stood firm, and additional mischief was added to the original evil, in the shape of affections of internal organs, which

ended fatally. Now affusion is laid aside, and its legitimate substitute, tepid or cool sponging of the surface, is usefully employed, because in harmony with the natural course of events.

"Here, then, is a type or standard to guide us to the correct investigation of nature in other less determinate affections. Is it not presumable that they also have a certain nature, and course, and termination, which it would be well for us to observe and promote? Take even a severe cold, with which all are acquainted more or less. Everybody knows that when once set in, treat it how you like, it will run through a determinate course of increase, maturity, and decline, and that all we can do is to shorten a little the duration of its stages by diminishing its intensity; or lengthen it by increasing its severity. Occasionally, it is true, an incipient cold may be stopped by a 'heroic' remedy, such as a tumbler of warm punch at bed-time; but much more frequently the heroics leave the patient worse than they found him, and the common experience of mankind shrinks from their use. Even a common boil on the fingers runs through its regular stages of inflammation and decline, or of suppuration and ulceration, each stage being hastened or retarded by external or constitutional causes, but never inverted. But if we apply to the one stage the means which are adapted only to the succeeding one, the result will be injurious; or if we lower the system so much that it becomes inadequate to carry on the regular succession of actions required for recovery, mischief must once more be produced. Let us take a case of pleurisy in an individual of average strength as an example. We know that, in ordinary circumstances, the excitement goes on increasing during a period varying from two to five or six days; that effusion of fluid into the cavity of the pleura ensues, that the inflammation then begins to abate, and after a few days more passes into an inactive state; that the natural action of the part then begins to be restored, and the fluid to be absorbed, till by and by recovery is completed. Or, if the inflammation endangers life, it either goes on longer than usual, or gives rise to effusion of a quality and quantity incompatible with recovery, and death at last ensues. In a case of average severity in a healthy constitution, left simply to the quiet and abstinence which Nature almost compels, we know, from observation, that such are the stages by which recovery is brought about; and all that the physician need attempt or care for is to use every precaution to prevent excitement from running too high or going on too long, and to meet any contingencies which may interfere and impede recovery.

"Very different, however is the general course of proceeding. Relying on the testimony of an incomplete fact (viz., that bloodletting produces excellent effects in inflammation, without attending sufficiently to the influence of the *adjuvantia*), the moment the practitioner ascertains the existence of 'inflammation,' he pulls out his lancet and bleeds the patient copiously. The oppressed vessels being thus partially emptied, much relief is experienced, and both patient and physician are pleased with the hope that the disease will be 'cut short.' This we shall suppose to have happened at the end of twenty-four or forty-eight hours or first third of the *ascending* stage of the inflammation. In a few hours, however, the vessels have contracted, and they and the heart adapted themselves to their diminished contents, and Nature thereupon resumes her attempt to carry the disease through its proper stages. The pain returns, the pulse rises, and the oppression augments. Bleeding is again resorted to with immediate relief, and the same phenomena recur. At the third bleeding we arrive at the period of the *natural* decline of the disease, and consequently no more excitement appears. 'Now then we have cut it short at last,' says the doctor, smiling complacently. 'Yes,' says the gratified patient, 'that last bleeding did the business; but what a pity you did not take more at first, and stop it at once.' With care and good management all goes on well, and by degrees the patient returns to his former diet and habits. If, however, he

happens to be a person not of a robust constitution, matters go on more doubtfully, and after partial recovery he finds his strength permanently shaken, or perhaps falls into chronic disease, and ultimately dies, or after a long struggle he may regain his former health.

"If we attend to the observation of Nature with a view *to co-operate with her*, and not simply to take her by storm, we shall be guided in our practice by the indications which she presents. In smallpox, for example, or measles, the excitement often runs very high in the first or eruptive stage, and means are required to moderate it. But if we bleed too freely, it is well known that the eruption (which we shall suppose to have come out) will generally disappear and increased danger to life ensue, because the order of Nature being forcibly interrupted, some internal disease is brought on, or the system sinks exhausted. Whereas if, instead of bleeding excessively, we keep the patient very quiet, in a cool well-aired room, and administer cooling drinks, mild laxatives or antimonials, and reserve bleeding for cases of necessity, the probabilitiy will be much in favour of recovery. To apply this to the pleurisy. Instead of being intent *on cutting it short*, the moment we ascertain its existence, we would have respect to its natural course and duration, and reserve our means to carry it safely through its regular stages. So far as my observation goes, cures would be more numerous and complete were this principle followed. If a severe bleeding disturbs fatally the progress of smallpox eruption, may it not also, when unseasonably used, injuriously influence the course of internal inflammation, and lead, for instance, to fatal oppression or effusion? We know that *de facto* it is a very rare thing to cut short a smart inflammation by a severe bleeding; but is that a reason for bleeding again whenever the pain returns? I think not. If the inflammation threatens to run very high and endanger life, then depletion becomes a reasonable alternative, but not so if it resumes merely its normal or regular course. If regard were had to the nature of the disease, I am convinced that many cases in which blood is very freely drawn would do well and better by much milder means; and that in many where bloodletting is really needed, a great deal might be spared in point of quantity, with much future advantage to the constitution. Take, again, the beginning of a severe cold or influenza. We know that *de facto* it will increase for several days, after which the feverish state will decline. If, to force it away, we begin with hot drinks and sudorifics, we invariably increase the fever by adding to the excitement; but if we keep quiet in bed, moderately cool, eat little, and drink mild tepid fluids when inclined for them, abstain from mental excitement and annoyance, the feverish state will be kept down,—and if we *then* give a good sedative antimonial, we shall rarely fail to elicit free and relieving perspiration, and to break the force of the disease. If we go out during the *rising* stage, we make the cold more severe, but in the second stage we do good. In the former, meat is injurious, in the latter useful. The principle holds with a slighter cold, but is of course modified in its operation.

"Even the causes of disease, and remedies, themselves produce their effects in a regular order, according to fixed laws which we ought to study and respect; but in practice we disregard this. It is no answer to say, 'Oh, they have acted quite differently in A. B. from what they did in B. C., and therefore they can have no determinate effects.' But find out the disturbing cause which interrupted the regular course in B. C., and the uniformity will become apparent enough. Such a cause must exist whether we know it or not. Having practically no regard to the laws of the constitution, we are apt to overlook their influence on the operation of causes of disease, and to consider nothing in the light of a cause which has happened more than a few hours before. We forget that there is always an interval of hidden action in the system itself before the effect becomes palpable to sense. Take hydrophobia

as a very striking example in proof. A person is slightly bitten on the finger, wipes it with his handkerchief and forgets all about it, till at the distance of two or three months symptoms of hydrophobia suddenly show themselves, and he is speedily carried off. In the interval he was unconscious of any change going on, and none was remarked by his friends. Yet nobody can deny that during all that time the poison was in the system, and producing changes of which the fatal paroxysm was the result. Even in the ordinary contagious diseases of smallpox, scarlatina, fever, &c., *several days of silent internal working* always intervene before the poison takes visible effect. This indicates the operation of fixed and determinate laws presiding over the production and course of diseased as well as of healthy actions. We never see a disease start up instantly after the application of its cause, nor full health suddenly take the place of active disease. We see the inhalation of fixed air induce immediate death; but it acts not by giving rise to disease, properly speaking, but by withdrawing one of the essential conditions of life. It is no proof of the absence of fixed laws and a determinate course to say that the same cause does not always produce the same results. The constitutional state upon which the cause acts is often different, and hence an impossiblity of the result being identical under every variety of circumstances.

"By thus insisting on the necessity of a more complete and faithful observation of the course of Nature, and of acting more systematically according to her guidance, I am far from meaning that we are to sit with our hands across, and allow things to take their own way. So far from it, it is certain that the principle I inculcate would demand more watchfulness, and give room for a nicer exercise of judgment, and a more consistent and, I believe, successful treatment. Disease arises either from the habits of the individual, from accidental causes, or from peculiarities of constitution acted upon by these. Hence, on being called to a patient, the first step in the *natural* investigation is to examine the constitutional qualities, to make ourselves acquainted with the mode of life, feelings, &c., and to trace the manner in which the cause has acted or continues to act. All these influence very greatly both the nature of the disease and its probable course. They also bear directly upon the kind of treatment and its probable success. If, however, we are content to regard disease as an entity, arising by chance and observing no laws, we shall have no inducement to trouble ourselves or the patient with any of these inquiries. Such is in fact the practical faith of the great majority of professional men. They discover the existence of an entity, which in medical works has a certain name, and, knowing that in the same books certain remedies are said to be good for that entity, they prescribe them accordingly, without giving themselves much concern about their mode of action or fitness for the individual constitution, age, or stage of the disease, and without inquiring whether there is anything in the mode of life tending to reproduce the malady or not. In many chronic ailments, removable causes are thus often left in full operation, while the effect is partially mitigated, but not cured, by the use of active medicines, and in a short time the whole evil returns in its full force.

"Whereas, if, proceeding according to the order of nature, we can trace the disease to any error in the mode of life, to any external source of danger, or internal peculiarity of constitution, aggravated by either of these two conditions, we can convince the patient of the fact, and give him a rational and confiding interest in the changes which we may recommend, and thus not only promote his recovery, but render him proof against all the seductions of quackery. According to the prevailing kind of intercourse between patient and physician, viz., unhesitating dictation on the one hand, and ignorant obedience on the other—blind faith is the pivot on which their mutual connexion turns, a faith which is thus necessarily at the mercy of the chapter of accidents, and is often supplanted by reliance on the first bold and confident quack

who comes in the way. People wonder that quackery abounds, and medical men ask for power from the legislature to put it down. They themselves, however, are in no small degree its abettors, and they have the remedy already to a great extent, although not wholly, in their own hands. *If they, who are educated, and should know better, accustom their patients to the principles of quackery, by themselves treating them empirically*, can they wonder that patients who are not professionally educated, and are trained and treated on purely empirical principles, should be as ready to listen to the assurances of the quack as to those of the regular practitioner, whose manner of proceeding is often so nearly allied in kind, as to present no very obvious marks of distinction from that of the quack? In fact, medicine, as often practised by men of undoubted respectability, is made so much of a mystery, and is so nearly allied to, if not identified with, quackery, that it would puzzle many a rational on-looker to tell which is the one and which the other. And this being the case, it requires no ghost from another world to explain why the profession has decidedly sunk in public estimation, and does not exercise that wholesome influence on public opinion which it ought to do. If the mass acts empirically, it can, in the very nature of things, expect only the amount of respect due to empiricism. The public mind has advanced immensely within the last fifty years, in elevation of view as well as in extent of knowledge. Medicine, however, has advanced only in knowledge; and, on looking back to the writers of 80 or 100 years ago, I incline to think that it has actually *lost* in elevation and comprehensiveness, and even in the perception of its own *nobleness* of sphere. If this be so, we must look within for the sources of regeneration, and for the means of regaining a dignified and honorable place in society. The public mind has advanced, while, in scope and general principles, the professional mind has stood still. To regain respect and relative position, the latter must shoot a-head again, and, on doing so, will regain its influence also.

"Let us, however, return to the case of pleurisy as an illustration. It is in general a well-marked disease; its nature is supposed to be well known, and the indications of treatment as clearly understood as those of any malady to which the human frame is liable. It is, therefore, rather a favorable example of the state of professional knowledge and principles of treatment. And yet, what do we find? Are medical men agreed how it should be treated? They *ought* to be, as it is frequent enough in its occurrence to give ample opportunities for experience; but they are not. In this country many place their chief reliance on free and repeated bloodletting and mercury. In France the plan of bleeding *coup sur coup*, in small quantities frequently repeated, is somewhat in vogue. In Italy bleeding was given up by many, and large doses of tartar emetic were resorted to. In Germany the cure was often intrusted to homœopathic doses of 'medicaments.' The strange thing is, that pleurisy is cured by, or at least pleuritic patients recover under, each of these plans, while also many recover under the 'médecine expectante' plan of lying in bed, drinking ptisan, and waiting upon Providence. Even in this country, however, a change has come o'er the spirit of my brethren within my own brief day. When I first opened my professional eyes, the lancet was in great vigour, and a well-employed medical man almost lived in a stream of blood. '*Vigorous practice*' was the order of the day. In typhus as well as in inflammation the lancet was the sheet-anchor of many, and quantities of strong purgatives were administered, sufficient to put disease of every shape and hue to the rout. Take the same men of vigour now, at the distance of twenty-four years, and they will tell a different tale. It is no longer, 'Be bold and decided and prompt in what you do;' but, 'Be watchful and trust *something* to nature.' This diversity of opinion and practice seems to me to have arisen partly from different constitutional states, arising from changes of atmospheric or other influences affecting the prevailing character of diseases,

but much more from all parties disregarding Nature's indications and efforts, and acting heterogeneously and without *any* rational principle. In this way, I believe that under each plan of treatment individual cases recovered which would have terminated fatally under a different mode; but also, that under all of them many died who might have been saved by a more rational and close adherence to sound physiological principles. To these we may be partially led even by a reference to the symptoms usually present. The sense of cold and shivering, which commonly precede, would lead to the avoidance of exposure. The pain, increased by breathing, inculcates absolute rest and refraining from speaking. The oppressed respiration requires, of course, *purity* of air more than ever. The heat and thirst which soon arise demand cooling simple drinks, and occasionally tepid sponging of the arms and face. The local stitch asks for mild emollients, such as bran poultices, and the impaired appetite requests the stomach to be let alone. If the bowels are oppressed, a mild laxative or lavement are indicated.

"Suppose the exciting cause not to have been violent or long-continued, and the attack, consequently, one of moderate intensity in an average constitution; the probability is, that by the adoption of the above means alone the disease will run through its course safely, and leave the constitution unimpaired. In this case effusion of fluid will as usual take place, be moderate in quantity, and be gradually absorbed. This effusion of serum, be it observed, is a natural provision—1st, for the relief of the vessels of the inflamed part; and, 2dly, for preventing adhesion between the pleura of the ribs and that of the lungs, and, consequently, for the preservation of their free play upon each other. When two surfaces, one of them inflamed, are placed in juxtaposition, and no fluid intervenes, adhesion takes place and hampers their mutual action. Within certain limits, then, there is no advantage to be gained by *preventing* the effusion, supposing we could always do so. *If we watch attentively*, and find all going on regularly and smoothly, according to the method by which Nature removes the inflammation, why should we step in to prevent, say, any effusion? I believe that many of the extensive adhesions actually met with after death, in persons who had at one time suffered from active pleurisy, are the results of over-active treatment interfering disadvantageously with natural processes. Adhesions occur also, however, but generally of small extent, from inflammation so slight in degree as to have suppressed the usual secretion, without leading to any effusion at all.

"Again, suppose the exciting cause to be very energetic, the habit inflammatory, and the attack violent. We, of course, ought to be doubly rigid in the use of the means above mentioned, and in addition we must watch closely, and come in time to Nature's assistance to prevent mischief. The reaction may run so high as to induce disorganization, or the effusion of an albuminous or semipurulent fluid, or to over-excite the brain or heart, and produce distinct disease in them. Or the individual may have some infirmity of body, which renders it more dangerous to wait the usual course of events than to attempt to shorten or arrest it. The thinking physician will take all these contingencies into account, and decide to act; but when he does so, he will still time his measures according to the law of Nature.

"*18th January.*—So far I had got, bit by bit, in my exposition; but as writing fatigues me I must now break it short, and send this away. Keep it merely as a rough and unfinished outline, and not as a proper embodiment of my views. To bring out these would carry me into writing not a letter but a book. I shall, therefore, not resume the subject, nor for a time must you expect any more letters from me.

"Yours, &c. A. C."

Assuming the general accuracy of the views contained in the preceding

letter, we shall be warranted in inferring from them that the radical fault of a large portion of the prevailing medical practice consists—1st, in prescribing for the abstract disease, irrespective of the constitutional condition of the patient; 2dly, in placing the chief reliance, even in cases where the active interference of the physician is not at all required, on the use of drugs and active means, the operation of which, from their administration being guided by no principle or well understood rule derived from experience, tends not unfrequently to impede and disturb the restorative efforts of Nature; and, lastly, in the general and often entire neglect of those physiological or hygienic arrangements which, from their co-operation with the intentions of Nature, contribute so powerfully to the well-being of the individual when he is in health, and to his recovery when attacked by disease. But much as these defects impede the present efficiency of medicine as a practical art, they do not in the least disprove its firm foundation in the laws of nature, or detract from the value of its principles, when properly applied (as they also often are) under the guidance of reason and experience. On the contrary, by laying bare the causes of the blemishes by which the medical superstructure has been disfigured, we not only show how they may be removed, but hold out the prospect of medicine being ultimately brought to as high a degree of certainty and perfection as any *estimative* science can attain. For even were all its constituent elements as fixed and certain as those of mathematics, their application must still be made by men of varying powers and knowledge to patients of different ages, sexes, habits, and constitutions; and it is plain that, under circumstances so inherently variable, there must always be such a wide field for the exercise of individual judgment and skill, as to render absolute similarity or equality of practice a moral impossibility. Approximate results are all that can be looked for.

Keeping to essentials then, and believing as I do, from the force of evidence, that ordinary medical science rests on a solid and natural foundation, I see great reason for endeavouring to improve and purify its doctrines and practice, but none whatever for abandoning the principles on which it is based. So far from this, I am more than ever persuaded that the observation of the order of nature is at once the surest guide we can have in the study and treatment of disease, and the only means by which we can arrive at a correct estimate of the special action and relative value of every therapeutic agent, whether homœopathic or eclectic. The same remedy prescribed at different stages of the same disease may be followed by consequences so widely different as wholly to mislead us if we do not use the necessary discrimination. Thus an infinitesimal dose or an ordinary drug administered just at the crisis or turning point of a disease will be *followed* by an alleviation of all the symptoms; but surely it would be unreasonable to infer, merely from that sequence, that the dose or the drug was therefore *the cause* of the amendment. On the other hand, when a medicine in itself useful or harmless, given eclectically or homœopathically during the increase of the disease, fails to prevent that increase from taking place, are we on that account warranted in inferring that it was the *cause* of the subsequent aggravation of the symptoms? Certainly not; and yet such inferences are so frequently drawn on equally slender grounds, that I, in common with most of my brethren, have had the mortification to receive unmerited praise and gratitude on the one hand, and blame on the other, for good and bad consequences which I was conscious were wholly independent of and unconnected with the drugs which were supposed to have caused them. In both instances all that could be *justly* said was, that the phenomena which ensued were *subsequent in point of time* to the administration of the medicine; and hence, unless we discriminate carefully between mere sequence and actual causation, we may easily be led by supposed experience into the most grievous errors. A case illustrative of this principle occurred a

few days ago. A young lady complained of troublesome palpitation of the heart, brought on by unusual agitation of mind. At the very time when the exciting cause was removed, a homœopathist was consulted and prescribed a globule. In a day or two the symptoms began to subside, and in a few days more they ceased. The homœopathist knew nothing about either the existence or cessation of the exciting cause, and therefore would naturally consider the result as produced by his medicine. In reality, however, the supposed cure was so palpably the mere return of quiescent and regular action in a healthy subject, after the exciting and disturbing cause had ceased to operate, that the homœopathist himself would never have thought of ascribing it to anything else, if he had been fully aware of all the facts of the case.

It is from not taking sufficiently into account the many modifying causes which may influence the progress and results of disease, altogether irrespective of any drugs which may have been administered, that the most opposite conclusions may be honestly arrived at regarding the efficacy and merits of the treatment pursued. In the preceding letter, taking smallpox as an example of a disease known to pursue a regular course, I contrast the safety and advantage of a treatment conducted according to the indications of Nature with the mischief which may be done by interfering violently with her order of proceeding; and show how differently the same indications may be interpreted when we have no sound principle to guide us. A few weeks ago two cases of this disease occurred, so strongly illustrative of both propositions, and so entirely in harmony with the views we are both advocating, that I cannot refrain from shortly alluding to them. The first patient was attended by a general practitioner, who, it was said, had improperly resorted to copious and repeated bloodletting at the beginning of the attack. The eruption did not come out freely, and the patient sank. The second case occurred in the same family, but was attended by a judicious and watchful homœopathist, who adopted what I have described as the natural treatment, but with the addition of certain infinitesimal doses. The result was that the disease went through its regular course, and terminated in complete recovery. By some of those interested in the two patients, this result was proclaimed to be a triumph of homœopathy over "allopathy;" but, considered impartially, and assuming the accuracy of the statements made to me, the triumph, if there be any (for it does not follow that because the first patient died under the treatment pursued she would *necessarily* have recovered under the homœopathic), may more justly be regarded as that of rational over irrational practice. Not having seen either of the cases, and not being accurately acquainted with their details, I shall not pretend to pronounce a positive opinion concerning the influence of either the bleeding or the globules; but as the results in both instances coincided exactly with those specified in my letter, written four years before they occurred, as likely to attend *good* as contrasted with *bad* general treatment, I can scarcely be accused of uncharitableness in still doubting whether the globules had any real share in bringing about the recovery which report ascribed wholly to their agency.

Connected with and arising out of the same neglect of the natural history of disease is another difficulty, which greatly impairs the value of many of the cases published as illustrative of the efficacy of either system of treatment. In the majority of instances the vagueness and meagreness of detail are such as to render it impossible for the reader to form, *from the data before him*, even a rational conjecture how the disease has arisen, or what constitutional or external causes are in operation to influence its intensity and affect its duration. It is precisely the obscurity inseparable from these omissions which often renders it difficult for the practitioner to appreciate the real agency of a new remedy or mode of treatment, where the unprofessional friends, from unconsciousness of their own ignorance, see only the clearest evidence of its

efficacy, and are apt to express amazement and indignation at what they consider the wilful blindness or prejudice of the practitioner who, still feeling a doubt, asks for further evidence before sharing in their enthusiasm. Not aware of the many sources of fallacy which lie in the way, the bystander attaches unhesitating faith to the narratives of cure, the very marvellousness of which is sufficient to call for the exercise of a prudent caution before perilling the lives of others on the assumption that the narratives contain a pure embodiment of truth.

It is for this reason, also, that I consider the numerous cases published in some recent homœopathic works to be individually valueless as *proofs* either of the action of homœopathic remedies, or of the superiority of the new system to the old. Taken in the aggregate, their results may constitute a fair ground for further inquiry; but, individually, little weight can be attached to them as proving the specific virtues of any particular medicine, or the reality of its influence on the cure. They contain a mere statement that certain symptoms were complained of, and certain remedies prescribed, and that certain changes followed; but they afford no concomitant information regarding the mode of life and causes which led to the disease, or the hygienic observances by which the exhibition of the remedies was accompanied; and, consequently, the reader is not enabled to form an opinion of his own as to the agency by which the cure was effected. To the homœopathist himself, who watched the cases, they may be pregnant with meaning, because he may have satisfied himself on these points; but to his readers they are nearly barren as items of evidence. This, indeed, is a defect inherent in a large proportion of ordinary as well as homœopathic cases, although in a smaller degree; and as a consequence cures are often ascribed to the use of drugs, which have in reality had nothing to do with them. A case in point occurred to myself shortly before I relinquished practice. A delicately-constituted boy fell into a state of health which excited the alarm of his friends, who consulted several practitioners about him without benefit. The treatment prescribed seemed to have been in many respects appropriate, but little advantage was derived from it. This led me to minuter inquiry, and I found that the guardians of the boy, in their anxiety for his mental progress, kept him at school from an early morning hour till late in the afternoon, and thus prevented him from obtaining a due supply of nourishment to support his strength, till, by the mere lapse of time, he became exhausted and irritable. They conceived a biscuit or piece of bread in the forenoon sufficient for him, because it seemed to be so for other boys. I prescribed a mild tonic for immediate relief, but insisted that he should have a longer interval in the forenoon for exercise in the open air, and an early dinner of plain nourishing food. The decided amendment which speedily ensued was attributed by some of the friends *to the medicine;* but, in reality, it was due almost exclusively to the regimen being brought into harmony with the laws of nature. Similar medicines prescribed by others had been of no use; but, conjoined with the required change of regimen, their operation, so far as it went, was beneficial. Every medical man must have met with similar cases, and also with many in which it remained difficult for him, even after the most careful consideration, to determine what was the real agent in the recovery which ensued. If, in the case of the boy, I had prescribed a homœopathic globule instead of the simple tonic, recovery would, I believe, have equally followed: but would the infinitesimal dose have been on that account entitled to the credit? I refer to this because I am acquainted with several cases of homœopathic cures equally equivocal as this would have been, but to which much importance has nevertheless been attached; but I need not occupy your space with their details. To my mind they do not indeed *disprove* homœopathy, but they do show that Nature, duly seconded by the arrangements of the practitioner, is adequate to the cure of many diseases, without his resorting to drugs of any kind.

Speaking, then, from a general view of medical practice, I should say that it is open to the charge of being carried on without due regard to the period and natural course of the disease, or to any other recognized principle which can yield us safe guidance, and that this is the cause of much of the uncertainty and contradiction for which our art is proverbial. We too often, I repeat, attack the disease as if we had to deal with an entity, and not with a state *of a living being* of a determinate constitution, who is suffering under it, and whose qualities, tendencies, and powers of endurance, consequently require to be taken into account as well as the disease itself. These defects, however, are not inherent in and inseparable from medicine. They are simply the defects of its cultivators; and if we were to begin by making ourselves acquainted with the laws of action of the different bodily organs, with the natural history of the diseases to which they are liable, and with the physiological conditions most favorable for their restoration, and then endeavour to deduce our curative indications from a general consideration of all those circumstances, there is every probability that we should make a nearer approach to unanimity of opinion, because then every one would set out from the same starting point, and proceed in the same direction towards the goal. It is true that as yet we know so little of the course and tendencies of many forms of disease, that we might often be at a loss what treatment to adopt; but the clear recognition of our ignorance is the first step towards the acquisition of knowledge, and more enlightened observation would gradually remove the obscurity which at present prevails.

If any of these remarks should be considered by some to imply a want of faith in professional aid, I have only to reply that no conclusion can be more unfounded. With all its imperfections on its head, medicine, in the hands of discriminating and experienced men, seems to me already to be the source of the greatest benefits to suffering humanity. When cultivated with more constant reference to sound principles it will become still more beneficial in its applications. It is a deep conviction of this truth which makes me so desirous to assist in the good work of medical improvement to which you are now devoting yourself

It would be easy for me, were it needful, to point out numerous instances in practice, in which medicines were prescribed without reference to any guiding principle, or natural tendency in the system at the time, and in which, consequently, results ensued which were wholly unexpected. In this respect I cannot exempt myself from the censure I have bestowed upon other practitioners; and I must further admit that, even after I became fully alive to the importance of endeavouring on all occasions to act as the assistant and interpreter of Nature in the treatment of disease, I continued to meet with many cases in which I could not discover what the real order of Nature was, and in which, consequently, I was obliged to resort to purely empirical treatment, and with necessarily varying success. But even then I had this advantage on my side, that the abiding consciousness under which I lived of Nature's presence and power inspired me with watchfulness for the observation of her earliest indications, and induced me in the meantime to borrow all the aid I could from her, by placing the various bodily functions, as far as possible, under the conditions most favorable for their healthy operation. When doing so, I have sometimes been rewarded by the gradual disappearance of difficulties which seemed at first irremediable, and by an amount of improvement which served to increase my faith in the restorative powers of Nature, even under unfavorable circumstances.

The one great principle, then, to which a comprehensive review of homœopathy, "allopathy," hydropathy, and all other systems of medicine seems irresistibly to lead is, that in all cases and on all occasions, *Nature is truly the agent in the cure of disease; and that, as she acts in accordance with fixed and*

invariable laws, the aim of the physician ought always to be to facilitate her efforts, by acting in harmony with, and not in opposition to, those laws. Disease, as already remarked, is a mode of action of a living organism, and not an entity apart from it. In accordance with this view, experience shows that when we favour the return to a normal action by simply natural means, recovery will ensue in most cases, without the use of drugs at all. So far from being always necessary to a cure, drugs are required only where the power of Nature to resume her normal action proves inadequate or is impeded by a removable obstruction. Even then it is still Nature acting in accordance with her own laws that brings about the cure. She may be *aided*, but *she ought never to be thwarted;* and medicine will advance towards the certainty of other sciences only in proportion as we become saturated with this guiding principle.

A few words now on homœopathy in particular. I am very glad that you have brought the question of its truth and merits seriously before your readers; for of all methods of advancing the interests of science, that which consists in the supercilious neglect of alleged new discoveries, merely on the ground that they differ from what is already known, is assuredly the worst. We know far too little of the constitution of nature, and more especially of animated nature, to be able to decide *a priori* what can or cannot be true regarding the mode in which vital operations are conducted, or in which they may be modified by external influences. Medicine itself is in its very essence an estimative science, and the truth of the principles on which it rests can be ascertained or verified only by careful and extensive observation. Theoretically these principles may be rendered more or less probable in the eye of reason, but they never can be demonstrated except by an appeal to experience. Medicine, moreover, considered as a system or body of doctrine, is still at the best in a very defective state. Every page of your Review admits and laments this unfortunate truth. We ought, therefore, to extend the hand of welcome to every man who is able either to correct an established error, or add a new truth to the existing store; and much more so, if the offered contribution should be that of a new and important principle capable, if true, of modifying and improving the whole field of medical practice. Not that we are by any means called upon to run after and examine every new theory or alleged discovery in medicine, merely because it is announced to be such. If we did, we should impose upon ourselves a never-ending and most useless task. But surely we are bound not to be too rash in rejecting without examination facts and principles which come before us, attested by men of experience, skill, and integrity, and who can have no motive for deceiving us. Judged of by the standard of our own opinions, these facts and principles may seem at first sight to be altogether absurd; but if so, the question then comes to be, is our standard itself undoubtedly a correct one? Or may it not be that ignorance has misled us to adopt it as infallible, and that it would be wiser in us to compare both it and the alleged discoveries with nature before assuming either to be demonstrably true? Had this reasonable course been followed with the discoveries of Harvey, Jenner, and Gall, how much idle and acrimonious disputation and professional obloquy might have been avoided, and how many benefits might have been obtained which were lost for years to suffering humanity, by the opposite course of first rejecting and ridiculing, and then examining evidence only when compelled to do so by a humiliating, because tardy and ungracious, necessity.

Let not this wholesome lesson, then, be lost to us who are the living successors of those who acted so unwisely. To use your own words, homœopathy, whether true or false, comes before us for examination with "claims on our attention which cannot be gainsayed." It is, you say, an ingenious system, "professing to be based on a most formidable array of facts and expe-

ments," and "woven into a complete code of doctrine with singular dexterity and much apparent fairness." Its discoverer and chief cultivators are, as you believe, "sincere, honest, and learned men." Dr. Fleischmann, perhaps the most eminent among them, is considered by you as "a regular, well-educated physician, as capable of forming a true diagnosis as other practitioners," and as "a man of honour and respectability," whose testimony as to matters of fact you "cannot therefore refuse to admit." Even in acute diseases the results of his treatment are such as "would have been considered as satisfactory by any candid physician;" and, according to you, even his own narrative affords ample evidence that many of his cases *were* severe; and you candidly add that this was confirmed to you by the private testimony of a competent physician who followed his practice for three months, and himself traced the progress of the pneumonic cases by careful auscultation through all their stages up to perfect recovery, which took place in as short a time as under the most energetic treatment. In some eruptive and febrile diseases the mortality is stated by you to have been below the ordinary rate. Let us scout quacks and pretenders as we may, here is surely too strong a *prima facie* case to warrant our dismissing it with mere ridicule and contempt, and one which amply justifies you in the course you have adopted of seriously investigating its claims. I am aware that you have been blamed by many for occupying your pages with even a refutation of "such trash;" but so far from participating in this feeling, my chief objection to your review is that it does not go far enough to be *conclusive*, either for or against homœopathy. You have admitted too much, and denied too much, to warrant your either pronouncing a definitive sentence, or reposing in *mere opinion* against its truth. Had you shown that the general results of its practice were *less* favorable than those of ordinary medicine, you might legitimately have held yourself absolved from going further; but in your present position you can no longer stand still. If, as you admit, the truth of homœopathy is a *question of fact and experience*, which no mere argument can set aside, you are bound in reason and in logic *to test its facts* for yourself before pronouncing authoritatively that it is not true, and more especially before stigmatizing it as "useless to the sufferer and degrading to the physician." However improbable its doctrines and practice may be in an *a priori* point of view, it is not by argument or ridicule that its alleged stronghold of facts can be successfully assailed. *As a matter of theory*, supported only by argument, homœopathy produces no conviction whatever on my mind of its truth, or even of its probability; but as *a question of fact*, claiming to rest "on the irresistible ground of its superior power of curing diseases and preserving human life," and on the alleged experience of able and honest men, as competent to judge as most of those who oppose them, I cannot venture to denounce it as untrue, because I have no experience bearing especially upon it to bring forward, and we are still too ignorant to be able to predicate *a priori* what may or may not be true in the great field of nature. But after the presumptive evidence which you yourself have produced, if I were now in practice I should hold myself bound, without further delay, to test its truth by careful and extensive experiments; because where truth is really our aim, the shortest and least encumbered approach to it is always the best; and even a few well-defined and carefully observed facts would carry far more weight, as *items* of evidence, than volumes of general or controversial reasoning. In instituting such an inquiry, however, we ought to be prepared to lay aside prejudice, and to scrutinize facts with the fairness and liberality characteristic of a love of truth, and not regard them with dislike and distrust, as if they were as many live embers purposely laid down to burn our fingers the moment we touched them. View the question as we may, *one of three things must be:* either homœopathy is true, or it is false, or it is a mixture of truth and error. Let us suppose the worst, and hold it to be false in its foundation, and

false in its superstructure, what harm can result from putting it to the test, and ascertaining the fact demonstrably? None whatever, but, on the contrary, much good. We shall at least *have gained the power of giving a direct and authoritative negative to its allegations*, which we shall then prove to be fallacious, and which have been suffered to reign and diffuse themselves for thirty years from the absence of *direct* counter-evidence by which to rebut them. We shall thus be able also to put the profession and the public on their guard with some chance of being listened to, and shall have obtained the inestimable advantage of keeping our own minds open to the admission of new truths, and of showing that in our estimate of evidence, and in our conclusions, we are actuated not by any mean jealousy or dogmatic assumption of authority, but by the single and simple desire of advancing the interests of science and humanity to the best of our ability. The *very worst* that can happen in the event of its being wholly untrue is, that we shall have bestowed some time and pains in obtaining the means of more effectually putting down a great error; while, as a compensating advantage of no small value, we shall have at once increased our knowledge and cultivated and strengthened our intellectual and moral faculties, by the very nature of the mental exercise which such a scrutiny requires; and surely these will be rewards well worth all the time and trouble which they may cost us.

If we adopt the supposition that homœopathy embodies an *admixture of truth and error*, the inducement to institute a rigid and careful inquiry into its claims becomes still more imperative, that we may obtain possession of the one and carefully avoid the other. The degree of success, be it more or less, which all admit to attend homœopathic practice, as conducted by such men as Fleischmann, is sufficient to show that either the system or its advocates possess *some* advantages in the treatment of disease, which it would be useful for ordinary practitioners also to examine and adopt. Whether the means which afford these advantages be derived from the domain of hygiène, of materia medica, or even of the imagination, is of comparatively little practical consequence, provided *their utility to the patient and the best mode of reproducing and applying them to the treatment of disease* can be clearly established. This, however, can be done only by careful investigation; and that such investigation would be amply rewarded may fairly be presumed, from the good already effected by homœopathy in demonstrating the evils attendant on that over-active medication, which characterizes so much especially of English practice. Ordinary medicine is now not nearly so heroic and undiscriminating in the use of strong measures as it was some years ago, and this improvement is unquestionably due in part to the progress of homœopathy, as well as to the natural increase of our knowledge.

The remaining, although unlikely, supposition, viz., that homœopathy shall prove to be *essentially true* in its fundamental principle, and consequently fraught with benefits to science and humanity, as its advocates affirm it to be, need not detain us more than a moment *If true*, how much more shall we then have reason to rejoice that we did not look upon its claims with prejudiced eyes, or reject and condemn it unheard and unexamined! Had Harvey's detractors examined his facts first, and then given their verdict, how different would the results have been to themselves, to him, and to mankind! And yet in our own day the profession acted towards Jenner, and also towards Gall, as if Harvey's name and memory had been blotted from the page of history.

I press all these considerations upon you, not from any particular leaning towards homœopathy, or any other new and disputed branch of knowledge, but because of the transcendent importance of cultivating science in a right spirit, and offering truth a ready and unprejudiced welcome from whatever quarter it may come. Ridicule and declamation may be rightfully employed to explode errors *after they shall have been proved to be so;* but they are most

unfit instruments for the primary investigation of truth, and as such ought to be banished for ever from scientific discussion, and a candid spirit of philosophical inquiry be instituted in their room. I have had no personal experience of homœopathy, and am, consequently, as little inclined to admit as to reject its claims, but I should wish to steer clear of prejudices regarding it. There are perhaps a few analogies in its favour, but its doctrinal expositions embody much that is crude and contradictory, and most of its practical evidence, in the shape of published cases, is rendered nugatory by the same sources of doubt which render so much of professional experience and testimony inconclusive, if not worthless. Sufficient discrimination is not used, or if used not recorded, to warrant much reliance on the alleged connexion between the remedy and the recovery in individual cases. As in ordinary medicine the *post hoc* is too universally assumed to imply the *propter hoc*. If I am not mistaken, the more intelligent homœopathists themselves admit this, and in consequence do not claim *belief* on the ground of the recorded cases, but affirm that on the contrary rational belief can be produced only by personal and extensive experience. But while I refuse belief I can see no reason for that deadly hostility which many feel towards *the principle* of homœopathy. If it be true, such hostility is misplaced and injurious. If false, it is needless and supererogatory; for the hostility will vanish with the non-existence or destruction of its object. And, after all, why should either party delight in representing homœopathy and ordinary medicine as *in every respect* opposed to each other? In a large proportion of cases the more rational and enlightened men of both parties employ the very same hygienic and general means which we have already seen to act so large a part in effecting recovery; and the chief difference between them relates to the principle on which the requisite medicine is to be selected. The homœopathist prescribes according to the principle of *similia similibus*, because experience, he says, proves this to be the safest and most efficacious plan. The ordinary practitioner, on the other hand, prescribes that which rational, or it may be routine, experience has led him to believe the best adapted for cases of the kind before him; and without stopping to inquire whether its action is homœopathic, allopathic, or antipathic. Surely there is no necessary cause of quarrel in all this, but merely results to be tested by careful experiment. "True," you may say, "but then the infinitesimal doses are so absurd." They certainly look very absurd; and I at once admit that nothing short of demonstration and personal evidence will ever inspire me with a conviction of their power to do either good or harm. But then all homœopathists say that it is the principle of *similia similibus*, and not the dose, which constitutes the essential element in their system, and that the infinitesimals may be discarded and yet the great principle of homœopathy remain unshaken. This latter, then, is the great fact to be proved or disproved, to settle the question for ever; and why should it not be put to the test? Let experiments be made on a sufficient number of healthy persons with quinine, or any other drug, to ascertain whether it really has the property ascribed to it of exciting certain groups of symptoms in a sound constitution, and after carefully varying and repeating the experiments, faithfully record and publish the results. Surely there is nothing unphilosophical or undignified in instituting such an inquiry, and nothing so difficult as not to be easily overcome by judgment and patience. Having tried their action in health, try the same remedies *in the usual doses* in the treatment of disease with as much care and discrimination as possible, and again record the results. If the principle holds good, let us adopt it and be thankful we have now a surer guide than before. If it fails, our exposure of its fallacy will tell with tenfold effect, from being founded on direct experience. In the same way with the infinitesimal doses, let us go at once to facts, and leave mere disputation to the idle speculator. All truth is harmonious, and what is true in the one system must harmonize with and throw light upon

what is true in the other, and, consequently, it would be better for science were both parties to endeavour to find out the points of contact rather than those of repulsion. In the very nature of things, certainty or absolute identity of opinion is, and ever must remain, an impossibility; and it ought never to be forgotten that in this respect there is a radical difference between medical and physical science. Physical science is *fixed* and *positive* in its principles and in its details, because its facts are always accessible for examination under the same conditions, or under such variations as can easily be traced and allowed for. Medicine, on the contrary, is and ever must be an *estimative* science, because its facts and phenomena are subject to continual variations from varying states of the body and mind of the patient, which we can neither control nor appreciate with entire accuracy. Its cultivators, too, are men differing in intellectual power, knowledge, skill, and experience; and even if they were all equal, their judgment is constantly liable to be impaired or disturbed by any slight disturbance of health or excitement of feeling, or even by a little extra fatigue, and hence, although its principles are fixed and determinate, because also founded on the laws of nature, the soundness of the conclusions deduced from them for the guidance of treatment must ever depend on the soundness of the estimate formed by the physician of their operation and influence in the individual case. Very rarely indeed can they be absolute, and hence the wide field for the exercise of sound judgment, skill, and discrimination on the part of the practitioner, and the mischief which may attend a practice founded on mere routine. Hence the forbearance and charitable construction which, as members of a liberal and useful, but most difficult profession, we are bound to exercise towards each other; and for the exercise of which there is, I fear, ample occasion in this very letter. But restrained as I have been by impaired health, as well as by the impossibility of doing full justice within your limited space to a subject at once so extensive and so important, I could not always express my opinions with the precision which I wished, and therefore I must trust to your good sense and right feeling not to give undue importance to any isolated or dubious expression which you may meet with, but to adopt that meaning which is in accordance with the general spirit of my remarks. My only anxiety has been to help you in the good work to which you have dedicated yourself with so much zeal, energy, and talent, and for which you will, I have no doubt, one day have your reward in a rich harvest of useful results.

I remain, my dear sir, very sincerely yours,

Andrew Combe.

P.S. To prevent the recurrence of a very common mistake, may I be allowed once more to call the attention of your readers to the broad distinction which subsists between the principle of *similia similibus*—which alone constitutes the basis of homœopathy—and the doctrine of the infinitesimal doses, which has been engrafted on, but does not constitute a necessary part of it? This caution is the more required, because the two propositions are more frequently confounded than distinguished, and we are surely bound to take the word in its correct meaning, as used by Hahnemann and his followers.

In a practical point of view, also, it is important to note the distinction, because, while it would be comparatively easy to verify the specific powers or mode of action of any drug given in ordinary or appreciable doses; and thus to *test the real principle* of homœopathy, it would be far more difficult and require a much longer and more varied inquiry to obtain precise and conclusive proofs, were the same drugs to be administered in doses altogether inappreciable to sense, as in the decillionth of a grain. We ought therefore to begin with the most important part of the inquiry first, and to leave the doctrine of the infinitesimal doses to be tested in its turn, if need be, after the *viability* of its parent shall be decided. A. C.

III. ON THE METHODS FOR OBTAINING A NATURAL HISTORY OF DISEASES.

By Thomas Laycock, M.D.

IN A LETTER TO THE EDITOR.

York, Feb. 13th, 1846.

My dear Sir,—When I received the reprint you were so kind as to send to me of your admirable article entitled "Homœopathy, Allopathy," &c., I had already read it *in situ* with unmingled satisfaction. In compliance with the request you make on the fly-leaf for the opinions of your friends on the matters discussed in it, I send these to you with a very sincere and humble *quantum valeat*, and with a wish that they may aid in the reformation you seek.

I would here, as a general preliminary, and once for all, make a thousand apologies for any dogmatism or too confident expression of opinion that I may appear to be guilty of. Indeed I would rather make ten thousand here (if you think ten thousand necessary) than occupy your time and patience with a premonitory palaver and kotoo at every notion I may express. So you will really and truly understand that I dissent, assent, and assert with some diffidence and with humble deference to you especially, and to everybody else in general.

In the first place, then, I fully agree with you in all your propositions; I differ with you in two or three minor details only. With regard to proposition 4,* I advance as the result of my experience in vital statistics, that the numerical method is one not as yet *generally* applicable to medical observations; or, if generally applicable, only in points of detail so simple in character that they scarcely require it for their elucidation, except to those who wish for a numerical expression of facts. In the first place you want skilled observers; you want a regiment of *trained* men—trained, too, in the same school, at the same drill—so that, whether their observations be right or wrong, they will be alike right and wrong. You know well the difficulty of minute diagnosis; how much skill is required to tabulate effectually according to the inductive method (for this is the true principle of the numerical method) those cases of which you have no doubt; and under the most favorable circumstances for observation how many links in the chain of phenomena to be observed and classed cannot be made out at all. Pray observe I am not giving an opinion adverse to the numerical method, because I am sure it is a truly scientific method. What I advance is, that as yet it cannot be made *generally* applicable.

Another minor detail I object to is in proposition 19. You would abolish the practice of keeping and preparing medicines in the houses of practitioners. You never could do it. I have lived and practised in both rural and civic districts, and I give this opinion also as the result of experience. Thousands of practitioners will give you the same opinion. A much better arrangement would be to require every practitioner, whether physician or surgeon, living half a mile from a druggist's shop, to keep and prepare medicines in his house. Compel him to do it by law, that the health and comfort of his neighbours may be consulted—things they, at least, estimate more highly than his own sham notions of dignity. It is the selling of drugs and the drugging system that lowers the practitioner in public estimation. He might keep a warehouse full of drugs, and welcome, if he gave them all away, as *he ought to do*.

Nevertheless there is the germ of a right good reform in proposition

* "The general adoption by practitioners in recording their experience of the system known by the name of the *Numerical Method* is essential to the attainment of the ends proposed in the preceding paragraph, as well as in many that are to follow."

19. If generally adopted, it will give a heavy blow and great discouragement to the sectarian bigotry of medical men. The "pures" and the impures will be well mixed, and a glorious *tertium quid* will be the result of the operation. In Thomas Carlyle phrase, strong symmetrical forest trees will grow up, instead of clipped Dutch-garden dragons. Here an oak (a sound-headed, omniscient pure), there an ash, here an elm, there a wide-spreading beech (some Nestor of the general practitioners), or a chesnut; but all graceful, symmetrical things, each supporting each other when the storms come. Of course there will be stubble and brushwood—quacks and humbugs—but even from these may be extracted some good. If there be no good in them, then they are unavoidable evils under *all* circumstances, and must therefore be endured.

Proposition 17 is, I think, rather too strongly expressed.*

Having specified the objectionable points, I ought now in justice to specify the more valuable. This, however, is not an easy business. All the propositions contain pith and marrow. Perhaps the first is the most pithy; at least I shall stop at the first.† We are to "attempt to establish a true natural history of human diseases." As this is what I have been trying to do for some time, I will give you my views as to the method most likely to be successful. I premise, however, a special and very low "kotoo."

It is essential to the natural history required that there should be—1, a system of observation; 2, a system of classification. You cannot take up a jumble of observed things (such a jumble, for example, as the homœopathic cases), and by the mere force of labour in inductive analysis and synthesis deduce useful results. There must be a guiding thread of theory—a centre round which the facts may crystallize—an idea which shall determine the sides and angles. Well, then, as disease is an aberration from normal structure and function, a history of normal structure and function, or in one word the science of physiology must be the basis of your natural history of disease. You cannot adopt any other usefully or safely. Any other will lead you into the empiricism from which we want to escape.

The classification of diseases should, I think, be founded on *both* structure and function, but mainly on structure. Function is often dependent on structure. Structure, too, really varies less than function, and is therefore better adapted as a basis for that "more comprehensive and philosophical system of nosology" which you recommend in proposition 15. The mucous and serous tissues are those which present the strongest points of difference, and have been often adopted by taxonomists. My own method has been to take the embryological development of the tissues as the guide to observation and classification.

Twelve or fourteen years ago the blastoderma, or germinal membrane, was described as forming two layers of granules. From the outer of these (termed the serous layer) the nervous, osseous, muscular, and tegumentary systems of the body were said to be developed. The other or inner layer, situated next to the yolk, was termed the mucous layer. Between these, or from these, it was reported that the vascular layer was formed. From the combined changes which these two undergo, it was said the intestinal, respiratory, and glandular systems owe their origin. *All* these views may not be true, but the facts so described had the merit to me of being both useful and simple. I thought they formed a capital basis of classification. I was thus enabled, in treatment, to consider

* "No systematic or theoretical classification of diseases or of therapeutic agents ever yet promulgated is true, or anything like the truth, and that none can be adopted as a safe guide to practice." Some kind of guide is safer than no guide whatever; a classification of diseases is simply *a methodical arrangement of past experience:* this is always a *comparatively* safe guide.

† "To endeavour to ascertain, much more precisely than has been done hitherto, the natural course and event of diseases, when uninterrupted by artificial interference; in other words, to attempt to establish a true natural history of human diseases."

all the products of the serous layer—the muscles, bones, serous membranes, and skin (always excepting the glands embedded in it) as but one extended serous layer in different stages of development. Here is not only a leading idea for a comprehensive nosology, but also for a comprehensive pathology and therapeutics. You see nothing marvellous in the conversion of muscles and tendons into bone, or in the development of cartilage and bone in the plastic matter effused from serous membranes. The metastasis of disease in serous structures is just what you would expect; the sympathy and relation to remedies of organs distant as to site, but related as springing from the same tissue, is a matter of course. The pathology of the vascular system is perhaps better elucidated by this idea than that of the serous system. Embryologists report that it is compounded of both mucous and serous layers; but the serous evidently predominates in it. Pray note especially, first, its actual structure, and then those approximations to cartilaginous deposit in the subserous cellular tissue of the heart and arteries—the atheromatous deposit—so often observed. Pray note, also, the participation in metastasis from serous membranes; the decided sympathy of the heart and arteries with serous inflammation (the pulse is but little disturbed in true mucous inflammation); and the coincidence of a well-marked muscular and osseous development with the sanguine temperament, and a predisposition to rheumatic and arthritic affections. I assure you that I have found such an idea to be a valuable guide in practice, especially in those mixed affections of organs in which the mucous membrane is implicated, in consequence of disease of the subjacent serous layer; as in affections of the bronchi, for example.

I could multiply from the pathology of the skin and mucous membranes illustrations of the utility of taking a start in your natural history at the beginning. As, for example, the determining causes of the difference in the phenomena of epidemics. Why do people vomit in smallpox, but not in measles? Why are there pustules in the latter, and a rash in the former? What, in fact, determines the elective powers of the tissues for each specific virus? I really think that a grouping according to structure would solve many mysteries of this kind, as well as the mystery of secretion itself. Pray understand this is only a sketeh of the *kind of classification* I think best: every practitioner has his own method of grouping facts, and there might easily be a better than my method.

In attempting a natural history of diseases, the anatomy of structures and natural order or sequence of vital actions must require a large consideration. Nature cures, you say. How? What is her machinery? What her order and method? Does she restore to health in the same way as she begins and continues life? If so, we must inquire into her method, and the order of her acts, if we would lay the foundations of therapeutical science deeply. What is really the limit of the *vis medicatrix naturæ?* It may, perhaps, render the inquiry more simple to take a special example—suppose inflammation of a serous membrane. Some cause of disease has been determined to the pleura; its blood vessels are congested; they cannot be distended, as in the mucous or cellular tissues; and the first effort of the vis medicatrix of nature is to exude the watery portion of the blood, through the parietes of the vessels, constituting the primary grade or stage of inflammatory exudation. If this effort be successful the congestion is relieved, the effused fluid is absorbed, and there is an end of the matter. If it be unsuccessful, the effusion continues, but not to be absorbed. The fibrin now poured out contains cytoblasts, which are the germs of an organization analogous to that of the parent tissue. False membranes or connecting bands are now formed, and these become tendinous, cartilaginous, and even osseous. Nay, I have seen the whole of the costal pleura ossified. But are these products of disease to be considered marks of a vis medicatrix naturæ? I know you will readily allow that there are limits to

the healing powers of nature. Science, assisted by close observation, will be able to fix those limits.

I think it may be assumed as a certain principle, that the *healing* effort differs in no degree from the *formative* effort. It is subject to the same laws, is effected by the same powers. The same may be said of the process of renewal, or moult, as Schultz terms it. These are as yet mysteries to us, although so much has been done by able microscopic observers. We are still uncertain as to the method by which regeneration or renewal of tissues takes place, and consequently we can scarcely attempt to determine the mode and limits by and within which the vis medicatrix operates. It is a great step gained to know how ignorant we are. The "cock-sure" practitioners, those heroes in physic, are the worst of all. And yet I never think of that vast amount of knowledge which the microscope and telescope have revealed to us without admiring delight. The sense of power which the intellect feels, as it sweeps from the infinitely grand in the universe to the infinitely minute in this microcosm of ours, is I think one of the highest and purest of pleasures. We are enabled to contemplate atomic beings here; there is the vast profound of mighty worlds. It is very clear, that in cell-life death is a necessary element; just the same thing occurs to the whole organism, in the structure of which these cellular beings play so important a part. The formative effort (the *nisus formativus* of the late venerable Blumenbach) contains within itself the elements of destruction. We have, therefore, to study a destroying as well as a healing power.

Function must not, however, be overlooked in the details of the natural system. In this field pathological chemistry will achieve triumphs as wondrous as those of microscopic research. The mucous layer of the embryo may here also be taken as the starting point, and its diverticula (the various glandular structures) be considered as parts of one great whole. The sympathies they manifest, the metastases they experience, the elective affinities they display, the vicarious action they occasionally assume, all present leading ideas for observation and classification. Groups of organs may also be established. I think of all organs the ovaria and testes exhibit the most extensive and general relations, and their influence in disease is perhaps the least understood, unless we except the muscular system. It is a remarkable circumstance that the physiology of the muscular system, and its pathological relations, should be so much neglected.

I have said that the *natural order* or *law of sequence* of vital phenomena is a point necessary to be known in constructing a natural history of diseases. We never can know the results of medication without a knowledge of the physiological antecedents; you are aware how much careful and earnest attention I have devoted to this point, and you know the result of my inquiries. It is simply this, that starting in the rudimentary germ, vital phenomena advance in waves of greater or less magnitude, the crest of the wave being the morbid period; that the periods of these oscillations vary in length from a few hours to several years; that they can be measured; and that they determine the periods, crises, and duration of diseases, and the order of their phenomena. As you have honoured me with a notice of my doctrines in No. XXXV. of your Review, I need only refer to it. In the history of the more common fevers, the utility of these doctrines is manifest enough. As they run through a known and determinate cycle, the action of remedies may be better estimated than in those in which the periods are not thrust before the eye of the practitioner. But even with regard to the more common forms, there are many "post hoc ergo propter hoc" absurdities committed. The natural and regular crisis or termination has often been triumphantly attributed either to some paltry medicine, a decillionth globule perhaps, or a few drops of hay tea; or to the effects of some heroic dose,—ten

or twenty grains of calomel to wit—or a frightful drastic, in which case it was really nature winning a double triumph, conquering both the doctor and the disease.

It is very manifest that any medicine whatever (the more inert the better for the purpose) may be so administered that the number of recoveries, stated numerically, shall be more recommendatory of its activity than can be produced in favour of any other medicine. Suppose I was able in all cases of pneumonia to ascertain the day when the first rigor began, and suppose that I treated them all on the expectant method, but on the eve of the 7th or the 14th day I invariably administered one grain of rhubarb. The amendment on the next day would, in the majority of the cases thus treated, be so manifest that, without a knowledge of the law of sequence, the evidence in favour of the impotent grain, as an active and effectual remedy for pneumonia, would be absolutely irresistible. If amendment followed its administration in 28 cases out of 30, the probable ratio, (vide Fleischmann's table), how could you invalidate the "ergo propter hoc?" You could only answer, "your grain of rhubarb is like a literary pirate, it claims merit not its own. When the disease began then began also a series of changes, the purport of which was a restoration to healthy action."

Now on this point at least I belong to the *old* physic school, the real old school. The ancient physicians and their successors, studied the natural history of disease very closely, and applied this law very systematically to the treatment of febrile affections. Although I specify febrile diseases, please to remember that the course of *all* diseases is regulated by it, because all vital phenomena are. You may not always be able to trace it; several points are requisite for this, as undisturbed action in the organism, the power and habit of acute observation, manifest phenomena, and the like. But be certain that the natural history of all diseases will be fallacious unless this principle be made a leading idea in the observation and classification of pathological phenomena.*

I might refer for another illustration of the important bearing physiology has upon a true pathology to the phenomena of the nervous system. But we are really on the verge only of the vast field these present to us for observation and analysis, and I fear I should only weary you with long explanations; I therefore forbear. I think, however, I have said enough (I hope not too much) in support of my main argument. Yet would it be believed that there are able, nay, scientific, physicians who virtually, at least, prevent the application of physiology to the elucidation of pathology? You occasionally hear half-educated practitioners speak slightingly of him who devotes his leisure to physiological as well as to empirical reading. They hint that he is a "mere reader," "a theorist," "not a practical man," &c., but this is usually in the way of business; it is an ignorant dealer and chapman trying to extinguish a dangerous rival in the true artist. No great harm can result, as the pretender must needs yield at last. Not so, however, with the able physician; such an example as I met with not long ago in a weekly journal. The Editor, in reviewing a book on practical medicine, had animadverted in a passage (I think in the introduction) in which the author, an accomplished physician and a physiologist, observed that scientific men are not and cannot be

* Dr. Graves has published an interesting essay on the law of relapse periods in agues. The foundation of the essay is a case of quartan ague, which continued with intervals of health for more than two years. Dr. Graves found the relapses took place, with one or two trifling exceptions, exactly on the days on which a fit *would* have happened, provided they had recurred regularly. Now this law, which Dr. Graves publishes as a discovery, and of which this case of Dr. Graves' is a good demonstration, has long been known to me, and is simply a corollary deducible from my general law. It is, however, an interesting example of the utility of the latter, for by it Dr. Graves might have *predicted* the facts he observed so accurately.

practical, because they have had no experience. The Editor was "pulled up" by the offended author, and positively the latter, in his letter of reply to the criticism, repeated his offence.

It is obvious enough that a physiologist *and nothing else* cannot be a good physician; but a practitioner may be a physiologist and *something more*. While he studies the experiments of nature, he may have an acute vision for the minute shades of disease, and a quick perception of the effects of medicinal agents. Nothing will escape his prying eyes. He will be as fertile in curative resources as in theories; as keen a lover of practical information as of scientific knowledge. Further, this practical study of physiology is in itself most interesting. In effecting it you have not the trouble of torturing, and dissecting, and analysing. If, for example, you want illustrations of reflex action, your first patient will probably present them, look closely only. If not seen in your first, they will appear in your second; at all events, wait patiently, and you need not go and catch frogs or snails to vivisect. Indeed you may study the physiology of the nervous system everywhere, in everybody; in the man that walks before you in the streets, that stands before you in the pulpit, that struts his hour upon the stage, in people in the ball-room or at a dinner table; nay, you can make your bachelor friend, who pleasantly chats tête-à-tête with you at your fireside, wagging his leg the while, a subject of physiological research. All these, and a thousand more, are good examples of subjects for scientific investigation, presented by persons in perfect health. But when the proteiform diseases of poor human nature come before you, in all their varied phenomena, what an inexhaustible store of matter for observation and meditation !*

You will see that I think a physiological practitioner is the man of the age, and that it is of essential importance to the reformation you advocate that the practitioner should be a thoroughly good physiologist. He should have the science as ready at hand as his snuff-box. He should be *trained* to its use; for every symptom he should at least seek out, if he cannot find, a *rationale*. He should always be curiously catechizing physiology for the why and the wherefore. Much might be done in the training of the student by our clinical lecturers, if they would set their shoulders to the wheel; but, unfortunately, the empirical method is the easiest to teach as well as to practise. The student might be led on step by step. He might, for example, first have simple pathological problems in muscular action given to him. A man with morbus coxæ steps across the ward before a clinical class. Problem as follows:—Required to describe the bones and muscles entering into the deformity, and the rationale, physiological and pathological, of its production. Or if the students be more advanced, five patients are brought under the notice of the class; one is in an apoplectic fit, a second has scoliosis, a third has valvular disease of the heart, a fourth has phthisis, a fifth, pneumonia. Required to describe the peculiar characteristics of the respiration

* I cannot help giving you a small and homely illustration of these views, which has just occurred to me in looking over Dr. Ranking's excellent Half-yearly Abstract. At p. 72 of vol. ii, you will find two modes of treating scabies, the one the consequence of physiological and microscopical research, the other of empiricism. Now, firstly, scabies gets into very respectable families, and is no jest when a guest with them; 2dly, sulphur, its best cure, stinks. Mr. Stiff, a "*practical* man," and evidently of the right sort, namely, a physiological practitioner, meditates on these two points, and then on the disease. Scabies depends on an insect, the acarus: so the microscope tells us; the acarus belongs to a class of insects that breathe by tracheæ: this entomology sets forth. Comparative physiology shows that (like men), if insects have their tracheæ stopped up, they die. Now, Mr. Stiff argued that if the acari were smeared over with an oleaginous substance, their tracheæ would be stopped up, and they would be suffocated; consequently that, after all, it is the lard, and not the stinking sulphur, which cures the itch. He tried the simpler method, and has cured more than forty cases with simple innunction alone. The other example is the *empirical* use of veratria alba, solemnly published without a reason in the Annales de Thérapeutique. Now this, as well as half a dozen other *poisons of the acarus*, have been known empirically to vagrants from time immemorial.

in each case, and to give a physiological and pathological rationale of them. Would not this method sharpen the diagnostic and therapeutic wits, and be at least one safe and sure step towards a Natural History of Disease? Alas, for the "practical men," those "dealers and chapmen," that sneer in the way of their *métier* at physiologists as "mere theorists," and "bookworms!" What a rout a few swarms of students so trained would create among them!

Well, then, such being the leading ideas that should regulate reformation in medicine, you inquire, can they be brought into action? No! most emphatically; in the present condition of the profession it is impossible. You want a complete organization of the profession, so that there may be unity of action with a systematic division of labour. Medicine is eminently a science of assiduous incessant observation—not observation by one man, but by many, on a definite plan. But how can you get this requisite unity of action from the scattered units of the profession, when the public hospitals with their immense advantages have hardly yet approximated to a scientific league? Again, *non omnia possumus omnes*. Let me ask how many of the leading physicians and surgeons of the metropolis are skilled in microscopic philosophy or pathological chemistry? I know your answer. No practitioner engaged in only moderately extensive practice *can* be.

Well, then, what is to be done? Shall we give up the attempt in despair? Surely not. That course would be equally cowardly and impolitic. Impolitic, because the intelligent public is treading fast on our heels. People already begin to say, "when we send for a doctor he can only tell us what we know already;" or they slily hint to you how *they* would attain to successful practice—"I would always let nature cure the disease." It is certain that we cannot stand still; and if we determine to march forward, our only alternative is AN ENTIRELY NEW ORGANIZATION OF THE PROFESSION.

I see on reviewing my letter that I have made a considerable détour. From transcendental physiology to wrangling, jangling, medical reform is rather a long step downwards. But the regular current of my ideas cast up this cross-grained subject; it is quite a legitimate consequence of my views. You cannot separate medical organization from medical doctrine and discipline. The present disjointed condition of the profession may certainly be termed in some sort an organization; but it is such an organization as leads to disunion instead of union—to isolated efforts instead of combined action—to strife instead of peace. It is necessary that the twenty thousand general practitioners of the United Kingdom should enter cordially into the proposed reformation of medical science and practice; I say *necessary;* for without the aid of that intelligent and numerous body all efforts at amendment must be fruitless. But what cordiality can exist between the scientific leaders of the profession and the men in the ranks, when all of the latter, (being at least nine-tenths of their number) who practise midwifery are set down in the rules and regulations of their college as an inferior class of workmen? Can it be possible that these medical legislators have ever held a council to consider how they could best advance medical science and art? I think not, or surely this principle would not have been adopted. On the other hand, I think the English practitioners have pursued a mere shadow. The fellowship of the College of Surgeons can do them no good; its practical advantages are only nominal; its professional value is nil, unless it can be used to humbug the public. But does the title really signify that its possessor is a wiser or more skilful man than his neighbour, the "member"? Put *that* to the vote amongst the generals, and an immense forest of hands speaks out the negative. Do let us try and extinguish lies, and cant, and quackery within the profession, before we sally forth to rout the sham friends of suffering humanity without. But enough on these hateful squabbles. Neither the present status nor the proposed reforms are worth contending for; they both must lead alike to nothing.

My own notion is, that the inductive method should be applied to our internal economy; and why should not a scientific body bring scientific principles of government into daily action? The profession living in each of the large towns, and in the surrounding districts, should be incorporated, and have all the privileges and responsibilities of our civic corporate bodies. The influence of numerous medical corporations dotted over the country, if their action were confined to their own locality only, would be considerable; but if they acted conjointly through a general council, what potent results might we not expect! You need only refer to the occupation abstract of the population returns to see that there would be no deficiency as regards numbers. Five years ago, the physicians and surgeons in five provincial towns stood thus:—Birmingham 166, Bristol 192, Leeds 130, Liverpool 334, Manchester (including Salford) 274. Now you cannot give me any solid reason why these large bodies of medical practitioners should not be incorporated. You cannot say that the discipline, dignity, or progress of the profession, would be retarded by such incorporation. You cannot say that they have not a right to the aid of the law in establishing a system of self-government, having in view the maintenance of professional honour, the advancement of medical science, and the public weal. Whatever argument you bring against my proposition applies equally to the medical corporations of the metropolis.

Pray note the advantages which would accrue from this scheme. Firstly, you would secure unity of action by a general council, constituted by representatives from the several corporations. You could secure a joint purse and division of labour in the sciential evolution of the profession. Every medical corporation would have its library, its reading-room, its museum for pathology and the kindred sciences, its curator, its physiological investigator, as well skilled in microscopic as in biochemical analysis. And who knows but that rich and enlightened laymen, seeing our efforts, would join us? Indeed, they certainly would, and the several local corporations would be enriched by legacies for sciential purposes. Public-spirited men would think more of the prevention of disease, by increase of knowledge, than of the cure of disease by increase of hospitals. With such a system I should indulge a hope that before long every practitioner would keep a case-book on a uniform plan, and each corporation would have its statist to work out the results by the numerical method.

I might as well stop here, but permit me to add that I augur good from the movement you have commenced. I trust confidently that it will usher in a true reformation of the profession. I hope we shall all soon be convinced that our political advancement, as an industrial body, depends upon our sciential advancement as a profession; upon the good we can effect; upon the knowledge we can gain; upon, in short, our moral and intellectual condition. I hope, too, that the miserable rivalry of grades will cease, or at least so much of it as will allow of unity of action upon these principles. This united action of the profession is indeed a worthy object of ambition; the more difficult of attainment, the more glorious when accomplished. It would surely be a noble sight to see our army of twenty thousand practitioners (now a disorganized multitude) marching forward in disciplined battalions, obedient to a gradation of officers, turning aside from no obstacles, winning triumph after triumph, as well over ignorance and error as over social prejudice and ingratitude, and setting an abiding example to mankind of the power of science to gain permanent worldly power and effect real objects. Is there any reason why we should not try? Of course the word impossible must be excluded from our vocabulary.

Believe me, my dear Sir, yours most faithfully,

T. LAYCOCK.

IV. ON THE PATRONAGE OF QUACKS AND IMPOSTORS BY THE UPPER CLASSES OF SOCIETY.

BY THE EDITOR.

The following communication appeared in the 'Athenæum' of Feb. 28, 1846. It is here reprinted in the hope that, through the medium of the members of the profession, it may not only reach an additional number of that class of persons to whom it was, in the first instance, specially addressed, but may reach them with the further advantage of new illustrations and analogous commentaries, which it must be in the power of every medical man to supply. The few following prefatory remarks seem to afford not only a further reason for republishing it, but also for giving it a place among the communications to which this department of the Journal is at present devoted.

The evil which the subjoined narrative is intended to expose seems so far from decreasing with the progress of knowledge, that it would appear almost to keep pace with it. And it is worth the serious consideration of the members of the profession, whether this belief in the most palpable of absurdities may not, in some degree at least, be fostered by part of their own conduct.

When we consider, for instance, the knowledge which the public have of the kind of evidence of the powers of remedies which medical men are often satisfied with—the mystery which is still, not unfrequently, sometimes purposely, thrown around the real or supposed mode of action of medicines—the belief engendered by much of the ordinary practice, that nature is helpless in the cure of diseases, and the active interference of art necessary in all cases—and, lastly, the overweening confidence so constantly displayed in the potency of many medicaments of obscure action or of no action at all—there would certainly appear to be some reason for having a like fear with Macbeth, "that we but teach instructions which, being taught, return to plague the inventor." If men ignorant of every principle of science, especially medical science, are led, on what they must consider good authority, to regard the relation of sequence of events as the accurate exponent and characteristic of medicinal cure, there exists no good reason for surprise if they fall into the trap laid for them by the impostor, when they can urge in excuse that the grounds on which they proceed are as sound and secure in the one case as the other. We know that this is not so; because we know that our own knowledge, whatever be its amount, is real, and our good faith unquestionable, while the utter ignorance and roguery of the professed charlatans are as real and unquestionable; but the public can only be made to know this by having their minds enlightened as to the true principles on which therapeutics is based, and as to the actual amount of our positive knowledge and positive power, and being thus made to see and comprehend the true and unmistakeable distinctions between rational practice and empiricism, and between the honorable professors of legitimate medicine and the vile race of quacks with which the world in general, and this country in particular, is infested.

MADEMOISELLE JULIE.

Omne animi vitium tanto conspectius in se
Crimen habet quanto major qui peccat habetur. (JUV. Sat. viii.)

Every now and then we read in the public prints of some wretched old woman brought before the police magistrate for practising, or pretending to practise, witchcraft, and therethrough swindling juvenile widows and love-sick maid-servants out of their shillings and sixpences.* Occasionally, also, we find parties of the same class and craft invading the province of the doctor, and doing "a snug little business" in the way of prescribing for, and of course curing, the diseases of all and sundry who may become their clients. The medium through which these wise women of the alleys and suburbs of this great city profess to become mistresses of the maladies of the unseen, is commonly a bit of rag from the clothing, a nail-paring, a lock of hair, or anything else connected with the person of the patient. The half-crown being paid, the nature of the malady is declared, and the means of cure specified. This is very various, according to the experience, the genius, or the fancy of the prescriber. Sometimes the disease is combated by what the learned would call *dynamic* means, such as words† or gestures, or the doing certain things at certain hours, or the handling of black or white cats, the plucking feathers from the tails of cocks, &c. At other times, the vulgar materials wherewith doctors work are put in requisition; especially those more obsolete sorts of drugs which, owing to the prime virtues of powerlessness and harmlessness, have come down to our times with undiminished fame from the days of the Asclepiades or before. Cures marvellous and manifold are thus wrought; cures, the result of which is never questioned; and which, to the philosophers of the alleys and attics, seem, and are, unquestionable. And no marvel. Have not these reasoners the very same grounds for their belief which satisfy their betters? The disease was declared, the remedy prescribed and administered, and the patient after a time got well. What can be more convincing? If, being ignorant of physic, they are ignorant of the fact that nature has the happy power of curing some diseases of her own mere motion; and if, having studied neither Bacon nor Locke, they confound sequence with consequence, the *post hoc* with the *propter hoc*,—can we blame severely, or at all, their loose logic or their halting reasoning? Should we not rather pity, and excuse, and forgive them, laying blame, if blame there be, on the lowliness of their lot and all its attendant circumscriptions, which make ignorance unavoidable, science impossible? Alas,

—— Knowledge to *their* eyes her ample page,
Rich with the spoils of time, did ne'er unroll.

Nor, looking to the influences of the same condition, the same circumstances, the same opportunities, the same causes, should we regard with too deep a disgust, or visit with too fierce an indignation, the poor wretches who thus practise on the ignorance and credulity of their humble neighbours. In one sense, knowledge may be said to be goodness as well as power; if it strengthens the intellectual faculties into wisdom, it strengthens the moral faculties into virtue. It has this tendency at least; and if it does not always do so, it often does so. Ought we, then, to feel surprise that among the children of penury and ignorance there are deceivers as well as dupes?

* The fact of the present paper having been written for a non-medical Journal will account for some peculiarities of manner and style, which will not fail to strike the professional reader.

† Sunt verba et voces, quibus hunc lenire dolorem
Possis, et magnam morbi deponere partem. (HOR. Ep. I, l.)

But what shall we say for those who, without having any of the same grounds for excuse, exhibit the same intellectual debility, the same debasing credulity, the same lamentable ignorance and error? Could it be credited, if it were not known as a positive and melancholy truth, that it is by the upper classes of society, by our aristocracy, that quacks, charlatans, pretenders, and impostors of all sorts, are most especially patronized? Proofs of this fact, and the most pertinent illustrations, present themselves on every side. Indeed the thing is undeniable—is notorious. What is its explanation? Can it be aught else than this—that among a portion of this class of the community, with all their refined and fashionable culture and accomplishments, science and logic, scientific truths, and the modes of investigating them and judging of their nature, their evidence and value—are as little known as among their social antipodes? If such is the fact, it is one as melancholy to contemplate as it is deeply to be deplored: it is more—it is discreditable, unjustifiable, fraught with much present evil, and ominous of more.

I give the following brief narrative, as explaining and illustrating, and (I hope) justifying the observations and animadversions which precede. I leave to the reader all comment on the case. To me it seems to speak for itself, "with most miraculous organ," disclosing secrets of the most humiliating and portentous kind, in quarters where, least of all, such disclosures should be possible.

During the last six months there has been allocated in the near neighbourhood of the most fashionable precinct of the West-end, a certain young Frenchwoman, known by the name of Mademoiselle Julie, who has obtained a great reputation among our aristocracy as a curer of diseases. She is about twenty years of age, obviously from her manners and conversation of the lower orders of society, ill-educated, and indeed illiterate. She is accompanied by her mother, a person in manners and bearing even inferior to her daughter, and by a gentleman who is said to be the brother-in-law of the mother. These people at present occupy good furnished lodgings in a street opening into one of the West-end squares. Their principal operations are performed at home; but Mademoiselle also condescends to visit patients at their own houses, more especially those of high rank and title.

The system adopted by Mademoiselle Julie is too ingenious and too well calculated to attract attention from the class by whom she is patronized, to allow us to doubt that it has been adopted after mature consideration and with malice aforethought.

It is well known that the two most striking and attractive delusions of recent times, HOMŒOPATHY and MESMERISM, have met with especial favour and patronage from the upper classes of society in this country, and have, through their means chiefly, become in consequence fashionable and famous. The system of the fair Julie has the singular merit not only of combining these two celebrities, but of selecting their most attractive and agreeable parts, and separating them from all that is offensive and troublesome. Thus armed, thus accomplished, is it surprising that her success has been great, or that, from the first day of her descent upon the realms of fashion, she has gone on conquering and to conquer?

This is the system of our Wise-Woman of the West-end: The sick person cuts off a lock of her (or his) own hair "close to the head," places it, unprofaned by other touch, upon a piece of white silk, folds this with his (or her) own hand, and finally deposits it in an envelope of clean paper. This facile and self-executed rape of the lock is all that is required of the patient in the first instance. No doctor intrudes with his troublesome and disagreeable questions—no pulse need be felt—no tongue need be shown—no horrid *percussor* or more horrid *stethoscope* need frighten the gentle breast from its pro-

priety. The lock is shorn, the deed is done; the dropped *Morning Post* is picked up, the new novel is resumed; the ripple of a moment vanishes, and the surface of life is tranquil as before. The next step is to convey the precious lock to the cell of the Wise-Woman, where the real business begins. This is transacted as follows: The uncle or mother of Julie magnetizes or mesmerizes her by some of the ordinary manipulations, and she falls asleep almost instantly (time is precious to those who are paid by the half-hour). The hair is then placed in her hand by the person who brings it; this person is put *en rapport* with her, by simply touching her hand once; she removes the covering from the mystical lock, takes it into her hand, and then commences a very active and elaborate process of rubbing, and squeezing, and picking it with the right hand, while it is held by the left; occasionally, also, she smells it. When this process has continued a few minutes, she begins to touch and press her own body with the fingers of the right hand, moving them from one place to another, sometimes rapidly, sometimes slowly, but finally dwelling preferably on one place, which she continues to press and manipulate more mystically and earnestly, and at last exclusively. It is then easily guessed that *here* is the site of the patient's principal malady, and the guess is soon verified by the words of the Pythoness. These words are waited for by the uncle, pen in hand, and are immediately committed to paper as they are uttered slowly, interruptedly, and in a subdued, sleepy tone. The record is made in the first person singular, as if the fair Julie were the patient. "I feel a pain,"—"I feel a sensation," &c., a mode of expression which is accounted for by the transcendent fact, of which both Julie and her *confrère* assure us, that through the mystic influence of the lock of hair by the intermingling of *its* (i. e. the patient's) *magnetic fluid* with her own, she, poor soul! is for the nonce made the recipient of all the aches, pains, sensations,—in short, of all the morbid symptoms of the unseen sufferer, who may, for anything she knows or cares, be hundreds of miles distant.

Good heaven, what a life of martyrdom must be that of poor Julie! To have one's poor carcase made the stage on which all the horrors that escaped from Pandora's box are to play their part—one after another, from morning to night; and, worse than all, a new one every hour! The very imagination of the thing is intolerable; what must be the reality? The conception of such an intrinsic monopoly by one poor body of all the ills that flesh is heir to, puts that of Dante to shame. The worst torments of the *Inferno* must yield to the Promethean sufferings of the unhappy Julie. And then, what inconceivable devotion to the cause of humanity, what unexampled fortitude, what heroic courage to dare and do all this, voluntarily, willingly, readily, cheerfully, yea eagerly! It is, of course, impossible to believe that into a mind capable of doing and suffering such things, the thought of fee or reward as compensation could enter; and, doubtless, the half-sovereign per *séance* and per lock, is accepted either in simple accordance with the practice of vulgar doctors, or for the purpose of being expended in relieving the sufferings of others, which assuredly none can know so truly and feel so surely as our poor Pythoness. But to return.

Having exposed the ills of one region, she passes to another, then to a third (as the case may be), and so on until she has given the full, true, and particular account of all the patient's diseased organs and their various symptoms. This is what the doctors call the *diagnosis* of the disease (viz. the settling its nature and name), which is followed by its *prognosis*, or exposition of its result; and, last of all, comes the *treatment*. This is set about as follows: A small box or tray containing upwards of two hundred tiny bottles is set before her. These bottles are those used by the Homœopathists, each containing its multitude of globules of medicated sugar of milk, with the name of

the contained remedy pasted on each. She passes her fingers rapidly over the corks of this multitude of bottles, and selects three or four, when the rest are put aside. She sniffs at the selected few, and at length fixes on one: this is the certain remedy for the disease, if it is remediable, or its emollient, if it is incurable. The half-sovereign is then paid, and the *séance* breaks up.

During the whole course of the proceedings, Julie remains with her eyes nearly or wholly closed, and speaks in a subdued tone; but exhibits no special indication to the observer of being in any peculiar condition but what might be expected from any person performing the part that is performed by her. She converses freely with the person originally placed *en rapport* with her, and answers any question he may put in relation to the patient or to herself.

"My personal knowledge of Julie and her proceedings is limited to two visits on two successive days, recently paid to her at her lodgings in ——— street. These visits were paid at the suggestion of a gentleman of rank, for one of whose relatives Julie had prescribed; and who, although a believer in her marvellous doings, was yet anxious that one who had had somewhat more experience with the mesmerists should observe her proceedings and test her powers. I willingly consented to accompany this gentleman to the cell of the Wise-Woman, not, of course, to settle any doubts I myself entertained of the true character of the whole affair—for of this I had no doubts—but in hopes that something might occur that would disabuse one honorable mind, at least, if it did even help to break the degrading and despicable spell which had snared and bound the judgment of hundreds of his own high class, reducing them, in this respect, to the level of the lowest. I was aware of the risk I was running of helping to confirm, instead of exposing, their absurd infatuation—which would be the consequence of Julie's guesses happening to be right in the particular cases I was to submit to her. On the other hand, I thought that a few very simple precautions in the selection of the cases, and in the mode of presenting them, would turn the chances on my side. I need hardly say that I knew the pretended knowledge to be an impossibility; but I knew, at the same time, that the symptoms of diseases are so various and vague, and many of such uniform occurrence in disease, that it would not be very difficult, by an enumeration of more or fewer of these common or universal symptoms, to give a colour of accuracy where nothing of the kind existed. And in the cases which had been already reported to me as successful instances of Julie's powers, I perceived that this was the usual course of her proceeding. I selected my cases accordingly—cases strongly marked, thoroughly definite, and with such bold and characteristic features that the failure to state *these* must be admitted as a total failure, however much mention might be made of many other symptoms of an inferior or immaterial kind. And in order to satisfy my friends that no special pleading would be possible either on my part or theirs, I placed a memorandum of the nature of each case in a sealed envelope, to be opened at the close of the sitting, and compared with the written revelations of the fair seer. In doing this, I confess that I felt my position somewhat humiliating, as if I were still open to the suspicion of entertaining some doubts as to the real state of things. However, for the reasons given above I went on.

"I had prepared three cases of disease; but I only *consulted* the fair Julie for two—one on each day. I regret that I cannot give here the full and exact particulars of each case, as they are now lying before me in my own memoranda, and those taken down from the dictation of Julie;* but these are

* These are appended to the present reprint.

only suited to the pages of a medical journal. The following general outline, however, will suffice for my present purpose:

"*Case First*—was that of a girl of twelve years of age, who has a most horrible and disfiguring disease of *the mouth*, but is in *the most perfect health* in other respects. So said my sealed memoranda. Julie's *diagnosis*, now before me, is—that there is disease of the heart and lungs, and stomach and kidneys, with general debility, fever, &c. &c., but not one word respecting the actual disease! Upon being questioned as to the sex of the patient, she said the individual was a woman ('*une femme*,' not *fille*).

"*Case Second*—was that of *a man*, with an incurable disease of a peculiar kind, having its seat in the *left lung*, and who laboured under no other disease, except debility and general derangement of functions necessarily dependent on so severe a malady. Mademoiselle Julie's memorandum says not one word of any disease of the lungs or other organs of the chest, but places all the mischief at the other extremity of the body, and allocates the main disease in an organ not possessed by that half of the species to which the individual belongs! The hair was declared to be *a woman's*, and the disease one peculiar to the sex!

This, I think, is what in vulgar language is called a "clincher," and with it I take my leave of the subject of Mademoiselle Julie. If, after receiving this taste of the quality of their oracle, her fashionable patrons and patronesses still continue to frequent her shrine, accept her inspirations, and obey her behests, it is to be at least hoped that the police magistrate will henceforth visit with pity and forgiveness, and not with reproach and punishment, the vulgar witches of the suburban alleys, or their poverty-stricken and unlettered victims.

After what is above truly reported of the sayings and doings of Mademoiselle Julie, the reader is left to form his own judgment as to her precise bodily and mental condition while making her revelations. I will only say, that not an iota of evidence exists in favour of the alleged fact of her being in that peculiar state termed by adepts, mesmeric or magnetic sleep, or somnambulism. To mine and to all common eyes, she seemed simply to be a very zealous but bungling fortune-teller, as wide awake as her nature permitted, but with her eyes shut. I think the very mesmerists will hardly contend for the opposite view of the case; although the extent of her blunders ought by no means to deprive her of their good word and patronage.

Two more observations I will make before concluding, which, considered in all their bearings, tend, I think, to account for a good deal of Julie's success with the class of persons who consult her.

None but those who have given a good deal of attention to the subject, and seen much of proceedings of the sort now under notice, can believe the utter incapacity of the majority even of educated persons to appreciate evidence as to matters of fact. And when the parties engaged in the observation of the phenomena are unreasoning partisans of the doctrine involved in them (as they generally are), it is literally true that trifles light as air are to such persons confirmation strong as proofs of Holy Writ. Although it is almost profanation to quote Bacon on an occasion like this, still, as his great words tell strongly on the matter in hand, I venture to give them in corroboration of the remark just made:—"The light of the understanding," he says, "is not a dry or pure light, but drenched in the will and affections, and the intellect forms its knowledge accordingly; for what men desire should be true, they are most inclined to believe. . . . . . When the mind is once pleased with

certain things, it draws all others to consent and go along with them; and though the power and number of instances that make for the contrary are greater, yet it either attends not to them or despises them, or else removes and rejects them by a distinction, with a strong and pernicious prejudice to maintain the authority of its first choice unviolated." (Nov. Org.)

The other observation is this:—Had I given Julie, on my *first* visit, the lock of hair which I gave her on my *second*, her description of the disease (though really false) would assuredly have been regarded by her votaries as an additional proof of her omniscience: inasmuch as on *that* occasion she really did hit on the organ which was affected *in the other case!* On such slight chances do the fame and fortunes of the great oracles of the world depend. The difference of *sex* would have been regarded as of no importance, being readily "rejected and removed by a distinction."

NOTES OF THE CASES.

Case I.—*Dr. Forbes's Memorandum.*

C. H., a girl æt. 12, who, from previous fever and affection of the mouth from mercury (some years since I think), suffered sloughing of the lips and cheeks, caries of the jaw-bone, loss of teeth, &c., leaving behind the present affection—viz., inability to open the mouth wide, total inability to close the mouth at the angles of the lips, consequent continuous flow of saliva from the mouth, imperfect utterance, &c. The little girl is in the hospital (St. George's) awaiting an operation for the cure of this defect. She is in perfect good health otherwise.

Case I.—*Mademoiselle Julie's Statement.*

C H. 21 Fevrier, 1846. Pulsations très vives au cœur. Les valvules du cœur sont pleines; c'est ce qui est la cause des pulsations. Il y a un douleur au coté gauche du poumon. L'épine dorsale est faible, douloureuse et fatiguée, principalment vers les reins. Les membranes de l'estomac sont très rouges: il y a comme un poids sur l'estomac: il y a aussi de l'irritation à l'estomac. Un peu de fièvre, causée par la faiblesse. Je sens le corps bien faible. Il y a des taches rouges au poumon gauche. Le dessous de la clavicule est douloureux. Je sens que le siège principale de la maladie est aux poumons et au cœur. Il n'y a aucune lésion au poumon. Je sens au milieu du dos autre chose que ce que j'ai dit; je ne puis pas bien le définir. Le sang au cœur n'est pas bon. Tous les organes de la poitrine sont malades, mais non pas d'une manière dangereuse. Le mauvais état du sang me fait voir la maladie comme étant grave. Plus je touche les cheveux, plus il me semble reconnaitre ceux d'une personne que j'ai déjà touchée. Je sens que les cheveux sont ceux d'une femme. La maladie est grave; mais non pas incurable.

Case II.—*Dr. Forbes's Memorandum.*

W. G., a man aged 52. Disease of rather more than three years and a half standing. During all this time he has had a severe cough, and pain on the left side of the breast, between the nipple and collar bone, and towards the middle of the breast. Has had repeated hemoptysis (six times), generally slight, but sometimes severe. Never any fever. Is now greatly emaciated.

Has also lately a pain in the throat about the larynx. His disease is rather peculiar, and is supposed to be encephaloid tumour of the lung (the left), which must prove fatal in no long time.

Case II.—*Mademoiselle Julie's Statement.*

W. G. 22 Fevrier, 1846. Irritation aux intestins; pésanteur dans le bas ventre. Je vois des taches rouges à l'intérieur du bas ventre. Cette partie a beaucoup d'inflammation: la vessie est très enflammée, et la peau de la partie supérieure est très épaissée. L'eau qui se forme aux reins est tres épaisse. Le foie est gonflé, et cause une sensation pénible. Il y a faiblesse dans les jambes; la personne ne peut pas marcher. Je sens des douleurs derrière la tête. La circulation du sang est mauvaise dans les intestins et dans les jambes; le sang est faible et pauvre dans tout le corps. La maladie principale est dans le bas ventre. Je sens que c'est une femme. Le sang est si décomposé que la guérison me serait peu probable. Il y a cancer à la matrice.

PART FOURTH.

Original Reports and Memoirs.

REPORT ON THE PROGRESS OF HUMAN ANATOMY AND PHYSIOLOGY IN THE YEARS 1844-5.

By James Paget,
Lecturer on General and Morbid Anatomy and Physiology, and Warden of the Collegiate Establishment, at St. Bartholomew's Hospital.

The following Report concerns the history of physiology during a longer period than either of the two former Reports, and contains notices of the works published between the 1st of October 1844, and the 31st of December 1845. Its general plan is similar to that of those already published, except in that, for brevity's sake and for convenience of reference, there are appended to each chief section the titles of such essays relating to the subject therein treated of as, for various reasons, could not or needed not to receive any further notice. For this, as for all the former Reports, the original essays have been read in nearly every instance; and whenever reference is made in the foot-notes to more than one publication as containing the facts to which any part of the Report relates, the first of the books so referred to is that from which the account given in the text was derived.

CHEMICAL COMPOSITION OF THE BODY.

Elementary constituents. Another fact in evidence that copper may occasionally exist, independent of poisoning, in the body, is furnished by Bertozzi,* who has detected it in fourteen biliary calculi of various kinds. The more yellow biliary colouring matter they contained the more abundant was the copper: white biliary calculi contained no trace of it. In bile he never could detect it. But, though these may be facts, yet the question, whether either copper or lead ever exists in the healthy body, must still be held open: in the last year MM. Devergie, Barse,† and others have again asserted that they do thus exist, and MM. Flandin and Danger‡ have as positively again denied it.

Proteine compounds:—Albumen, &c. M. Wurtz§ has rendered a further account of his mode of preparing albumen, in a state of purity, yet soluble without the addition of alkali or any other of the substances hitherto supposed necessary to its solution.

Dr. Ludwig‖ has extracted from the materials hitherto vaguely named *Extractive matters* of the blood a principle which is isomeric with the binoxyde

* Oesterr. Med. Wochenschr. Sept. 6, 1845.
† Comptes Rendus, 28 Oct. 1844.
‡ Comptes Rendus, 30 Sept. 1844.
§ Annales de Chimie et de Physique, Nov. 1844.
‖ Annalen der Chemie und Pharmacie, Oct. 1845.

of proteine of Mulder. The extractive is obtained by the pressing of defibrinated and coagulated blood, the neutralizing of the fluid pressed out, and again coagulating it, and filtering. To this filtered fluid, now free from albumen, alcohol is added, and the precipitate thus obtained, being washed with water, yields chloride of sodium, phosphate of soda, and a very small quantity of substance like binoxyde of proteine. Boiling alcohol removes fatty matters from it, and ether a crystallizing fat. The body thus purified, both in its general characters and in the results of its chemical analysis, resembles in all essential respects the binoxyde of proteine. It exists in the serum—not in the blood-corpuscles—and, next to the saline constituents, forms the chief part of the extractive matters of the blood of man, and of the dog, ox, sheep, and pig.

But what binoxyde of proteine may be is a question; for doubts are now cast by Liebig* on the accuracy of Mulder's researches from which he deduced the existence of proteine itself. Liebig finds that the supposed binoyxde [tritoxyde?] of proteine obtained by adding ammonia to the solution of fibrine in dilute hydrochloric acid (the albuminose of M. Bouchardat), contains really all the sulphur which was present in the fibrine. And he finds that, when, as in the process proposed by Mulder, fibrine, albumen, or caseine is dissolved in potass ley, and the solution is neutralized by acetic acid, this solution contains no sulphuret of potassium or other sulphur compound; but that, if the precipitate formed when the acetic acid is added (and which is the supposed proteine) be dissolved in potass, sulphur may be detected in it by adding sugar of lead to the solution. This precipitate, therefore, Liebig holds, is not proteine; neither does he find the corresponding precipitate obtained from peas to be free from sulphur; nor, finally, has he "yet succeeded in producing a non-sulphuretted substance possessing the composition and properties of the so-styled proteine."

Dr. Buchanan† has found the remarkable fact, that the liquid of hydrocele may be coagulated in five or more minutes into a transparent tremulous jelly-like substance, by adding to it a small quantity of the washed clot of blood. The same coagulating property is possessed in a less degree by several other animal substances; e. g. the buffy coat of blood in minute shreds, or even when dried and pulverized; the transparent coagulum on a blistered surface; and, in various much less degrees, by muscle, skin, cellular membrane, spinal marrow, mucus, and pus. He thinks that the coagulant power is chiefly seated in the colourless blood-corpuscles, which exist, together with the insoluble parts of the red corpuscles and some fibrine, in the washed clot; and with fibrine in the buffy coat; and in the coagulum from blisters. To these also he ascribes, I think, the usual coagulation of the blood; the colourless corpuscles inducing the coagulation of the fibrine dissolved in the liquor sanguinis, and thus leaving its other constituents as serum, which is only *defibrinated* liquor sanguinis.

Fibrine. Dr. Polli‡ has confirmed the fact, that when fibrine begins to putrefy in water, albumen is among the substances formed from it. He has also found that if serum be mixed with three or four times its volume of water, and then boiled, the milky fluid which remains, and which contains, according to Boudet, albumen combined with soda, is spontaneously coagulable. If left to itself for a few days it forms an opaque-white soft clot, from which a

* Lancet, Feb. 20, 1845.

† Medical Gazette, Aug. 8, 1845. Dr. Buchanan argues as if it were proved that the fluid of hydrocele contains fibrine, and that it is fibrinous coagulation by which it becomes jelly-like when the portions of clot or other substances mentioned are added. It may be so, but there is no proof of it; and he is not right, I think, in saying that hydrocele fluid is commonly regarded as analogous to liquor sanguinis. The same coagulability may be seen in other dropsical fluids, e.g. when blood is let into them after death.

‡ Annali Univ. di Medicina, Feb. 1845.

limpid watery fluid separates, in which the clot sinks. But the spontaneous coagulation is the only circumstance in which it imitates fibrine, for the clot is not filamentous.

The rest of this paper is occupied with the changes induced in the fibrine of blood by inflammation, especially its diminished tendency to coagulation, and its rarefaction. Several cases of late coagulation are given, similar to that mentioned in the last Report,* but less remarkable than it. They confirm his belief that no blood is really incoagulable or remains liquid out of the body till it decomposes: and that when blood has seemed not coagulable, as in typhus and scurvy, its coagulation has been overlooked, or it has not been kept long enough for an unusually slow coagulation to take place. He considers that the most essential condition of this slowness of coagulation is a peculiar modification of the vitality of the fibrine, other favorable conditions being the excess of carbonic acid and of salts in the blood. And for fibrine thus modified by the influence of an inflammation he proposes the name of *bradyfibrine*.

Fibrine modified in another manner, and peculiarly *rarified*, he calls *parafibrine*. He has found that in the most acute inflammations the specific gravity of the blood or liquor sanguinis is rather increased by the removal of the fibrine; so that, contrary to the rule of health, the serum has a higher specific gravity than the liquor sanguinis. He supposes that this light fibrine, or parafibrine, may be that which is formed during, and as a result of, the inflammation, and which, in severe cases, may be formed in such quantity that it more than balances the effect of the ordinary fibrine and the *bradyfibrine* in increasing the specific gravity of the blood, so that the specific gravity of the blood is increased when both it and they are removed from it. He describes the parafibrine as coagulating very slowly into a mass which has a gelatinous aspect, and consists of very slender filaments, like those which give consistence to the albumen of an egg. When the serum is pressed from such a clot it becomes fibrous, tenacious, and heavier than the serum. Such clots are found in vesications: the fluid they contain is rich in parafibrine, and may be studied for the characters of this substance.

Both these modifications of fibrine may coexist in various proportions with ordinary fibrine in inflammatory blood, and hence arise many varieties in the time of coagulation and the appearance of the buffy coat of such blood. It is this light fibrine in the blood in inflammations, which, according to Dr. Polli, is effused with the serum in inflamed parts, for its tenuity permits it to traverse the walls of the vessels more easily than any other constituent of the blood can; and, once effused, it coagulates and becomes fibrous. Its copious existence in blood indicates the highest degree of inflammation; of this the gelatiniform buffy coat is the best sign. The simply retarded coagulation, due (at least in part) to the formation of bradyfibrine, indicates a lower degree of inflammation. The mere increase of ordinary fibrine is characteristic of the first degree of the same, and the increase of the one is proportionate to the extent of the other. Lastly, the existence of light fibrine in blood explains how a buffy coat may in certain stages of inflammations be formed in blood which coagulates quickly. In such a case the liquor sanguinis is very light, and may have been made lighter by previous bleedings; the corpuscles sink rapidly and leave above a layer which may presently form the buffy coat.

Saline constituents of animal fluids. Dr. Golding Bird† questions the correctness of Enderlin's‡ deduction that no organic acid is combined with the alkaline bases of any of the secretions except the bile, because no alkaline carbonate is found in any of their ashes. He thus shows that a salt of soda

* Page 6. † Philosophical Magazine, May 1845. ‡ See last Report, p. 8.

with an organic acid may exist in a solution of phosphate of soda, and yet yield no carbonate on ignition;—nine grains of dry tribasic phosphate of soda being mixed with four grains of acetate of soda, and exposed to a full red heat in a covered crucible, the product is easily soluble in water, and has a strong alkaline reaction; but no effervescence ensues on adding dilute sulphuric acid to it.*

BLOOD.

Corpuscles. Mr. Gulliver† has published a synopsis of all his former observations on the sizes of the red corpuscles of the vertebrata, in tables, stating the measurements of these bodies in no less than 485 species. He has also shown,‡ in reference to the formation of the buffy coat, that the rapidity of sinking of the blood corpuscles is not directly proportionate to the tenuity of the fluid in which they are collected, but rather, inversely proportionate; for it depends mainly on the rapidity and completeness with which they aggregate in rolls and clusters (as described by Hewson and others). This grouping is promoted by saline solutions, which have been made thicker by mixing gummy matters with them, and is retarded or destroyed by dilute saline solutions. As soon as the corpuscles are aggregated they begin to sink rapidly; and hence, in blood that will be buffed, there is a remarkable acceleration of the sinking of the corpuscles two or three minutes after it is drawn.

Blood. Dr. G. O. Rees,§ extending his well-known observations on the changes produced in the blood corpuscles by the entrance of fluid when they are placed in fluids of less specific gravity than the liquor sanguinis, and by the exit of fluid from them when placed in fluids of higher specific gravity than their fluid contents, has shown in these changes several sources of fallacy in the quantitative analyses of the blood. He states also that, after copious perspiration induced by exercise, the corpuscles are thin and very like those from which fluid has exuded when immersed in a denser fluid. The change may be ascribed to the loss of water from the liquor sanguinis, and the consequent increase of its density, till it becomes of higher specific gravity than the fluid contents of the blood-corpuscles. On the other hand, the serum of a dog, into whose veins six ounces of water were injected in the place of six ounces of blood just previously drawn, was tinged by the colouring matter of the corpuscles, which had oozed from them into the fluid of reduced specific gravity.‖ In examining corpuscles retained at a temperature of 100° F., he has seen each corpuscle contracting into a hour-glass form, and then dividing into two, usually of unequal size, a process which he supposes to be similar to that by which the corpuscles are naturally multiplied.

Gaseous contents. Prof. Magnus¶ has repeated his experiments to determine the quantities of oxygen and nitrogen contained in the blood, and the extreme quantities of those gases which it is capable of absorbing. The ex-

* Among other works relating to animal chemistry in general, the most important are, of course, the Lectures of Liebig in the Lancet of the past year, and the continuation of Mulder's Physiol. Scheikunde; and after these,—1. An essay by Dr. Schmidt, 'Zur vergleichende Physiologie der wirbellosen Thiere,' in the Annalen der Chemie, Juni 1845; from an essay published at Brunswick. Its main purpose is to obliterate the supposed line of absolute material distinction between the animal and vegetable kingdoms, evidence being drawn from ample analyses of the elementary tissues of the invertebrata. 2. Gobley, "Sur l'existence des acides oléique, margarique, et phospho-glycérique dans le jaune d'œuf," in the Comptes Rendus, Sept. 29, and Nov. 3, 1845. I may here also refer for the compendium of every recent work on chemistry to the Annuaire de Chimie of MM. Millon and Reiset. Paris, 1846.

† Proceedings of the Zoological Society, No. cliii, Oct. 14, 1845.

‡ Dublin Medical Press, Dec. 11, 1844, and Edinb. Med. and Surg. Journ., Oct. 1845.

§ Gulstonian Lectures. Medical Gazette, March 14, e. s. 1845.

‖ Schultze has made a similar observation in blood drawn after taking large draughts of water.

¶ Philosophical Mag., Dec. 1845, Suppl. p. 563, from the Annalen der Physik u. Chemie, lxvi, p. 177.

periments consisted chiefly in repeatedly agitating fresh drawn blood with atmospheric air, and then (with necessary cautions) washing out the absorbed oxygen and nitrogen with carbonic acid. The results of numerous experiments were pretty uniform. The quantity of oxygen obtained from the blood was from 10 to 12·5 per cent. of its volume, that of nitrogen from 1·7 to 3·3 per cent. Reversing the mode of experiment, it was found that blood could absorb 1½ times its volume of carbonic acid, and that, after all its oxygen and nitrogen had been washed out by carbonic acid, it could absorb, at a maximum, 16 per cent. of its volume of oxygen, and 6·5 of nitrogen. Similar experiments were made on old horses' arterial blood, and they proved that such blood naturally contains in simple solution from 10 to 10·5 per cent. of oxygen, and from 2 to 3·3 per cent. of nitrogen. And some further experiments lead the author to believe, that of this 10 per cent. of oxygen in arterial blood, about 5 per cent. are consumed in the systemic capillaries, and 5 per cent. remain in the venous blood, to be completed to the general average of 10 per cent. by the absorption of fresh oxygen in the pulmonary capillaries

Life: capacity of organizing. Whatever doubt might still remain in the minds of some, whether Mr. Hunter maintained the truth in his doctrine that the blood could be organized, that is, that the whole substance of a portion of blood, being at rest either within or without a vessel, may develope itself into tissue and become vascular, and grow and nourish itself, must be satisfied by the observations of Dr. Zwicky* on the metamorphoses of the thrombus, or clot of blood which forms in a vessel above a part at which it is tied. For observations which, like these, stand on the boundary between physiology and pathology, no more space can be given than to state briefly the result; which is, that, to all microscopic appearance, the organization of the fibrine in an inflammatory condition and of that in a clot of blood is in all essential respects the same process, the presence of the blood corpuscles in the latter having no further influence on the process than that of somewhat retarding it. The whole process of the metamorphosis of the fibrine of the blood into fibro-cellular tissue, and of the formation of vessels in it is satisfactorily traced; and thus the only additional evidence which was needed is supplied for the establishment of another of the long-doubted Hunterian doctrines.

Modifications in disease. An account has already been given of the researches of Dr. Polli on the changes of blood in inflammation. The continued investigations of MM. Andral and Gavarret† have in numerous striking instances confirmed their account of the constant increase of fibrine in the blood in direct proportion to the increasing acuteness of the disease in all cases of inflammation, and of its regular and proportionate decrease in typhoid fever and purpura.

And these cases are confirmed by the laborious investigations of MM. Becquerel and Rodier,‡ who have prefaced their pathological results by certain facts of more present interest in physiology. They give a standard table of the comparative constitution of the healthy blood of the adult male and female, showing the constant differences between them. As to the differences of constitution of the blood at different ages, they find none that is well-marked in men, but in women it is constant that the proportion of corpuscles is less before menstruation is fairly established than it is afterwards, and that when menstruation has ceased it again diminishes. In both sexes the cholestearine increases with increasing years after the age of between forty and fifty. The corpuscles are more abundant in the robust and well-fed. During pregnancy there is a great diminution in the proportion of corpuscles, a less considerable one in that of albumen, a slight increase of fibrine and of the

* Die Metamorphose des Thrombus; von Dr. H. Zwicky. Zurich, 1845. 4to.

† Comptes Rendus, Séance du 18 Nov. 1844. ‡ Ibid. and Gazette Méd., 23 Nov. c. s.

phosphorized fatty matter, and an increase in the proportion of water. The other results obtained by these authors relate more exclusively to pathological states of the blood, but they are in that regard of very high interest.*

GENERAL ANATOMY AND PHYSIOLOGY OF THE TISSUES.

Fibro-cellular tissue and elastic tissue. Distinct filaments are described by Stadelmann† as demonstrable in the transverse sections of fibro-cellular tissue, cut and moistened after being dried. Their cut ends are like exactly circumscribed, pellucid circles, shadowed by sharp lines. Their diameters are all equal, and, as nearly as they could be measured, $\frac{1}{15000}$ of an inch. After similar preparation of portions of the elastic tissue, he finds the cut ends of its fibres round, ovate, triangular, or polygonal, circumscribed by pellucid lines, and separated by intercellular substance. They measure in the human ligamenta subflava from $\frac{1}{5174}$ to $\frac{1}{7037}$ of an inch: in the ligamentum nuchæ of the horse they are nearly twice as large.

Membranes. Much attention has been given to the arrangement of nerves in the serous membranes. The republished observations of Purkinje,‡ on the nerves of the pia mater and arachnoid are noticed elsewhere. They are confirmed and added to by those of Mr. Rainey,§ on the nerves in the arachnoid; but these have been published since the time to which the Report has reference. M. Bourgery‖ also has given a long account of what he believes to be nerves of the peritoneum, dissected by the help of weak nitric acid, and briefer notices of those of other serous membranes; but, in regard to the nerves of the peritoneum, since the cords which he dissected were not submitted to minute microscopic examination, and the microscope can discern only obscure traces of any nerves at all in that serous membrane, his description cannot yet be received as sure. So also, it must remain an open question, what amount of nerves the pleura receives, and how they are arranged.¶

Cuticles, pigment, &c. E. H. Weber** has noticed that in all cases, but especially in the nasal ciliary epithelium of warm-blooded animals, the ciliary movement is accelerated by warmth and reduced to half its usual rate by cold. In this as well as in its rythmic action the ciliary movements are like those of the heart; and there is no striking difference between them in regard of their period of continuance after apparent death, for he has seen the frog's auricle contracting for sixty hours, and after the blood in which it was placed had become quite putrid.

Dr. Moleschott†† has examined the discoloration of the pigment of the

* Papers on the development of the blood corpuscles have been read to the Royal Society by Mr. Newport and Mr. Wharton Jones, but I cannot gather an intelligible account of their results from the abstract given in the Proceedings of the Society, Nos. 60 and 61.

There is an agreeably written sketch of the present state of the pathology of the blood, by Wunderlich, with the title 'Pathologische Physiologie des Blutes, Stuttgart, 1845,' but there is nothing in it, I think, which is both new and interesting in physiology. Here also, as well as for the anatomy of the tissues, I may refer to Prof. Harting's 'Recherches micrométriques,' 4to, Utrecht, 1845, containing exact measurements of the blood corpuscles and other parts. The physiological part of the work is preceded by a careful discussion of the comparative merits of different methods of micrometry, among which, after minutely testing all, the author prefers the common plan by double sight, because it is the most convenient, always applicable, and, for very minute objects, liable to least error.

† Sectiones transversæ partium elementarium corporis humani; Turici, 8vo, 1844, pp. 10 and 12.

‡ Müller's Archiv, Hefte iii, iv, 1845.

§ Report from the Med. Chir. Society in the Lancet, &c., February 27, 1846.

‖ Comptes Rendus, 8 Sept. and Gazette Médicale, 20 Sept. 1845.

¶ See hereon Pappenheim, in the Comptes Rendus, Oct. 1845, and Purkinje l. c. p. 293.

** Archives d'Anat. génér. et de Physiologie, Janvier 1846, from a paper read at the Scientific Congress at Naples in 1845.

†† Tijdschrift voor Naturl. Geschiedenis en Physiologie, 1845, St. 2, D. 12.

skin of frogs when they breathe in pure oxygen, held over water, so that the carbonic acid they produce may be absorbed. In a few hours the blackish-green elongated marks, extending backwards and downwards from the eyes, become lighter and greener. Then for several days the change goes on more slowly, but at the end of twelve days it is very distinct in all the spots; the dark semicircular marks on the thighs can then hardly be seen.

The development of epidermis has been described by Mr. Erasmus Wilson* as consisting essentially in—1st, the production of "primitive granules" in the blastema; 2d, the collection of four, five, or six of these into "aggregated granules," which become the nucleoli of the complete cells; 3d, in the arrangement of a single tier of aggregated granules around each of these first aggregated granules, so as to form an oval or circular mass, a "nucleated granule," the nucleus of the complete cell; and 4th, the production of another tier of aggregated granules around the nucleated granule, forming a transparent border around it, around which border there is probably a cell-membrane, forming the proper epidermis cell or "nucleolo-nucleated cell." The growth of the cell he believes to be due to successive repetitions of the same process in the development and growth of aggregated granules within it, the full-grown cell containing secondary and tertiary cells and bodies in all the four stages of development above mentioned.

Cartilage. M. Valenciennes† has made an extensive examination of the structure of the cartilages in mollusca and cartilaginous fish, but the only facts which it appears important to state here are—1st, that the cartilage cells are generally arranged in such regular plans that it would be possible to determine by microscopic examination the order, or even the genus, of an animal, from the character of its cartilages; 2d, that none of the cartilage cells have, in any species, canaliculi communicating with them; and 3d, that gelatine, not chondrine, is abundant in the cartilages of cephalopods.

Bones: chemical analysis. A number of analyses of bones of various animals have been made by Dr. Stark.‡ He shows that, besides their marrow [i. e. I suppose, all the fat that can be easily scooped out of the cancellous tissue], the bones of mammalia contain from 13·5 to 29·2 per cent. of fatty matter. The solid part of the shaft of the canon bone of a sheep contained 4·3 per cent. The bones of almost all birds under one year old contain an oleo-albuminous (?) matter, which, as the bird grows older, is absorbed. No less than 232 analyses of various bones from all the vertebrate classes are given, to show the proportions of animal and earthy matters; and the result is, that in all bones the proportion is nearly the same, the average proportion for all being 66·09 of earthy matter to 33·91 of cartilage. No confirmation is afforded of the notion that the proportion of earthy matter is the larger the higher the animal is in the scale of creation, for the largest proportion (68·74) was in the true bones of cartilaginous fishes—the smallest (61·9 to 62·3) in the bones of the bear and marmozette monkey. The proportion of earthy matter appeared a little greater in the wild than in the domesticated mammalia. There was no evidence found of its increasing with age in either men, cows, or sheep. Neither did the hardness, or inflexibility, or want of transparency of bones appear to be dependent on the earthy matter being present in a very large proportion, but on the mode in which the textures of the bone were held together.

Structure. Mr. Goodsir§ states that in the cavity of each bone-corpuscle, but not extending into the canals, there is "a little mass of nucleated cells of great transparency." This he regards as the *germinal centre* of the texture or

* Proceedings of the Royal Society, No. 61, June 19, 1845; and Philosophical Mag., Feb. 1846.
† Comptes Rendus, 25 Nov. 1844. ‡ Edinb. Med. and Surg. Journ., April, 1845.
§ Anatomical and Pathological Observations. Edinburgh, 1845. 8vo, No. x, p. 64.

of the cells comprehended within the canals of the corpuscle which it occupies, through which canals, also, he supposes that the nutritive material which the mass of cells attracts into the corpuscle is conveyed for the nutrition of the hard parts between them. He also describes the tissue which lies between the blood-vessels and the walls of the Haversian canals as a layer of cellular substance, at least in the fœtus, for in the adult it is replaced by fat.* In another paper† he describes, (as a source of fallacy in those experiments in which it appeared that, when a piece of bone was removed and its periosteum was left, the periosteum reproduced the lost bone,) how that it is hardly possible to separate periosteum from bone without detaching minute portions from the surface of the bone; and that these, adhering to the periosteum, may serve as nuclei for the production of the new bone, which thus seems to be produced from the periosteum itself.

Development. The early stages in the development of bone are described by Köstlin,‡ from the examination of a thin layer of new bone (osteophyte), which lined nearly the whole skull of a woman who died three weeks after delivery. His account has, perhaps, less of special than of general interest, for it may not be true of the ordinary development of bones, but it confirms the account given by Karsten§ and some others, of the general plan of cell-development. He finds that, in a homogenous membrane formed in the earliest stage of the exudation, there are first formed scattered elementary granules. These gradually increase in number; some enlarge, some coalesce, and the larger granules thus formed separate into a moderately thick wall, a dark contents, and an eccentric nucleus. Thus they become primary cells; and these also enlarge by growth and coalescence. At first their form is nearly spherical, but they gradually become less regular, and, their contents disappearing, they become also less turgid and clearer, and their nuclei more numerous. As their contents disappear so their walls coalesce with the intercellular substance, and their nuclei become imbedded in it. The number of these nuclei increases still more and they become darker by the in-taking of earthy matter; and, moreover, there is formed from the intercellular substance around each cell a group of dark corpuscles like the nuclei, which acquire a regular arrangement in regard both to them and to one another. Their arrangement is such, that if one imagines them connected with one another and with the nuclei by dark lines, and the cell spaces darkened by the reception of earthy matter, one will have the common bone-corpuscles with the dark network of the so-called calcigerous canals.

Periosteum. Purkinje‖ has found (as Pappenheim¶ also did) a copious network of nerve filaments in periosteum. He examined particularly that of the front of the tibia, and that of the vertebral canal. In the latter, which he describes as formed by an outer layer of the dura mater, divided into two layers at the foramen magnum, he found abundant bundles of fine-fibred nerves, which appeared to communicate through the intervertebral foramina with the sympathetic.

Muscular tissue: its structure, action, and growth. Stadelmann,** in transverse sections, could never detect any appearance of an empty space or canal in the axis of the striated muscular fibre, such as Valentin and others have described.

E. Weber†† has examined the mode of contraction of the muscular fibres

* I cannot make out what is described as the mode of origin and development of the cells of this layer of substance.

† No. xi.

‡ Müller's Archiv, 1845, Heft. 1.

§ See last Report, p. 37.

‖ Müller's Archiv, Heft. iv, p. 289.

¶ Last Report, p. 10.

** Archives d'Anat. Gén. et de Physiologie, Janvier 1846, from the Report of the Scientific Meeting at Naples in 1845.

†† L. c., p. 15. The rest of his account of this tissue confirms Mr. Bowman's.

under the favorable condition of continued contraction, into which the frog's muscles are thrown when excited by the electric current from a rotating magnet. He finds that the contraction is always by simple shortening, and never by zig-zag flexures of the fibres; and that these are formed, as Mr. Bowman stated, only when the fibres relax, and by their elasticity tend to elongate. He shows also that, in contraction, the fibres do not become harder, and that it is only their increased tension when contracted which makes them feel harder, as it does also the tendons and other tissues at the same time.

Dr. Helmholtz* appears to have obtained a more direct proof than hitherto existed, that chemical decomposition accompanies the action of a muscle. With all due precaution he electrified the hind legs (say the right hind legs) of frogs till their muscular irritability was exhausted, and then compared the results of the analyses of the muscles of these and of the other hind legs of the same frogs, which, except in that they had not been electrified nor in any way excited to muscular action, had been kept in the same external conditions. The result was, that in every case the quantity of alcoholic extractive matter was found to be increased in the electrified muscles, and the quantity of water-extractive matter decreased in them. The proportionate quantities of the former in the electrified and in the non-electrified were (on an average of six experiments) as 1·33:1, and those of the latter as 0·78:1. Similar results were obtained by like experiments with an eel-pout and with a pigeon. Of the other constituents of the muscles;—the changes, if any, in the fibrine could not be determined; the differences in the quantity of albumen were inconsiderable; the fat appeared unchanged. Counter-experiments were made, which showed that electricity did not produce similar changes in muscles whose irritability had been previously exhausted, and that the changes were not due to ordinary spontaneous decomposition.

Together with many observations on the minute structure of the muscles of hymenoptera, Prof. Harting† has given an account of the comparative dimensions of the muscular fibres of the new-born child and adult, showing that the average diameter of the primitive fibres in the child is to that in the adult as 1:3·64, and the respective average intervals between the transverse striæ as 1:1·18. In the child the distance between the striæ is to the width of the fibre as 1:4·415; in the adult as 1:8·42. It is hence deducible that the ordinary growth of a muscle is due to the increase in size, not in number, of its primitive fibres; but whether this increase of size is due to enlargement, or to increased number, of the primitive filaments, could not be determined. It is evident also, that of the growth of a muscle in its length, part is effected by the increase in the length or depth of the particles (?) on which its transverse striation depends; but this is not enough to account for the whole growth of a muscle between childhood and manhood; so that part of the growth in length must be ascribed to the increased number of these particles (?).

He has also measured the muscles and their elementary parts in the healthy and the paralytic arm of an adult; from which it appears that the diminution of size accompanying the paralysis of the muscles was due to a diminution in the size, not in the number, of the primitive fibres. But with the diminution in the size of the muscles there was no corresponding diminution of the nerves: both their trunks and their ultimate fibres were of equal size in both arms.‡

* Müller's Archiv, 1845, Hefte i, ii. † Tijdschr. voor Naturl. Geschied. en Physiol. d. xii.

‡ See further on this subject in the section upon Nutrition. And on the several tissues refer to. 1. C. Bruch, Untersuchungen zur Kenntniss des körnigen Pigments; Zurich, 1844, 4to, containing a clear account of all that is known of pigments, whether healthy or morbid, with some original observations. 2. J. E. F. Knorz, De pili structura et genesi; Marburg, 1844, 8vo, of which a similar account may be reported. 3. Dr. Gregory, On the presence of Phosphate of Magnesia in Bone, and on a new method of obtaining Phosphoric Acid from Bones; in the Medical Gazette, April 11, 1845. 4. C. G. L. Bruch, Nonnulla de Rigore Mortis; Magunt, 1845, 4to, confirming by new evidence the

CIRCULATION.

Action of the heart. Dr. Mitchell,* in a case of ectopia cordis, watched the movements of the heart for an hour and fifty minutes. The pulsations were twenty-five in a minute before the separation of the umbilical cord; after it they fell to twenty, and then to seventeen. After the auricles were distended with blood they emptied themselves by a gentle flowing motion, and immediately after this the ventricles contracted. The effect of the ventricular contraction was to shorten the heart from base to apex, and to cause a considerable bulge or projection in the centre, giving rise to an evident elevation of the fingers when laid on it. The apex of the heart was *not* elevated. After the ventricular contraction the heart appeared quite flaccid and relaxed, although it was evident that the ventricles were not emptied.

Volkmann† has discussed the relation of the movements of the heart to the nervous system; and, to prove that they do not depend on the brain and spinal cord, he adduces the fact that they continue after the heart is cut out, while all the rythmical movements which do depend on the brain and cord, such as those of respiration and the lymph-hearts, cease as soon as their connexion with parts of those nervous centres is destroyed. He supposes, therefore, that the movements of the heart depend on the *immanent* power of the sympathetic nerve-fibres and ganglia in its substance. His experiments show, that if the auricle and ventricle, while pulsating rythmically and in harmony in a fresh frog's heart, be suddenly separated from each other, though they may both continue to pulsate they will not pulsate in harmony. And when the ventricle is divided by incisions carried through nearly its whole length, some of its portions will continue to pulsate spontaneously and rythmically, while others, just as large, will only move when irritated. He thinks that this shows that, in the former, central organs remained from which impulses for movement might proceed, while in the latter there were no such central organs or ganglia. He concludes, therefore, that the ganglia in the heart are so many central organs, or points from which motor impulses flow out, and that they are suited for action in concert by connecting nerve fibres, forming altogether a system so arranged as to produce, in regular series, the successive contractions of the muscles of the heart.

This theory coincides in many points with that of Kurschner, alluded to in the last report; and he holds that the auricles are the parts to which the reflex influences of the ganglia are always first directed, and that the contraction of the ventricles is the consequence of the contraction of these. In evidence of this he says, that whatever part of a heart, when cut out but still irritable, is irritated, the consequent contraction always begins in the auricle. [But it is certainly not always so: I have many times tried the experiment in cut out turtles' hearts, and have always found that the contraction begins at the part irritated and thence extends over all the rest.] Valentin,‡ also, has made experiments on the same question, but he doubts whether the rythmical contractions of the heart are dependent wholly on its nervous system, and not in part on the mechanical arrangement of its fibres; since, he says, the continuance of rythmical movements is observed only in those pieces of the heart

established view that the rigor is due to the contraction of the muscles. 5. Harting, Histologische Aanteekeningen; in the Tijdschrift already referred to. Besides the papers on the muscles, lens, and nerves, of which an account is here given, the essay contains remarks on the milk-corpuscles, the action of sublimate on the blood-corpuscles, and other questions of the anatomy of tissues. The continuations of the admirable works of Dr. Sharpey, in the new edition of Quain's Anatomy, of Dr. Todd and Mr. Bowman, in their Physiological Anatomy, and of Mulder, in his Physiol. Scheikunde, are also important contributions to the recent history of general anatomy.

* Dublin Journal of Med. Sciences, Nov. 1844. † Müller's Archiv, 1844, Heft iv, p. 424.

‡ Handb. der Physiologie, Bd. ii, p. 767.

which remain connected with portions of the tissue intermediate between the auricle and ventricle. Kölliker,* also, while he agrees with Volkmann's explanation of the rythmical action of the heart, states this as a fact which he, like Valentin, has observed in frogs' hearts. [But in the hearts of turtles, the size of which makes the fact evident enough, it is very different; I have often seen the apex of the heart continuing its rythmical contractions long after being cut off from the rest of the ventricle. Both auricles also continue similarly contracting when cut off above the auriculo-ventricular rings; but their contractions are not synchronous. Indeed, I do not remember to have ever seen any difference, in respect of the continuance of their rythmical action, whether the pieces cut from the heart were or were not connected with the tissue uniting the auricles and ventricles.† I believe, therefore, that Volkmann's theory may be held as true; but that there is no evidence at present for assigning a peculiar locality for the chief centres for harmonizing the rythmic actions of the heart.]

Arteries. An account of the structure of the blood-vessels, confirming in all respects that of Henle, and supporting the microscopic by chemical evidence, so far at least as the action of acetic acid and potash on the several coats is concerned, is given by Mulder,‡ from examinations by himself and Donders.

The nerves of the arteries are among those which Purkinje§ has submitted to renewed investigation. He finds, by the help of acetic acid, all the plexus of arteries in the rete mirabile of the ox surrounded by a close web of nerves.

In an excellent essay on the anatomy of the parts concerned in laryngotomy and tracheotomy, Dr. Gruber‖ has treated especially of the middle thyroid (A. thyroidea ima. seu Neubaueri) and the crico-thyroid arteries. The former occurs in about ten per cent. of the bodies examined, arising in general from the innominata, more rarely from the arch of the aorta or the common carotid, more rarely still from the inferior thyroid or thyroid axis, and most rarely from the internal mammary. It probably never arises from the left carotid; when it appears to do so the artery thus arising should be called a second inferior thyroid. Its diameter varies from half a line to two lines and a half; in two cases, with healthy thyroid glands, its trunk measured five lines. It is more frequently on the right than on the left side; and once only in ten cases was double. It may proceed vertically up the front of the trachea to the isthmus or either lobe of the thyroid gland, or it may pass obliquely across to the trachea, even going from its right side to the left lobe of the gland.

In regard to the crico-thyroid artery, Dr. Gruber notices especially that arrangement in which it is very large on one side, having a diameter of from three-quarters of a line to two lines. This is most often observed when there is a middle lobe to the thyroid gland, but the large artery is not always on the same side as the middle lobe. Such an artery occurs about once in every four persons, and is more frequent on the right than on the left side. Its place may be taken by the thyroid branch of the right superior thyroid artery. The large artery in most cases bends down at a right angle in front of the crico-thyroid membrane, to reach the isthmus of the thyroid gland, and gives off at its bend a small branch which crosses the membrane. More rarely the

* Die Selbständigkeit und Abhängigkeit des sympathischen Nervensystems. Zurich, 1844. p. 36.

† It is, however, I think, necessary for the propagation of contraction from one part of the heart to another, that they should be connected together by muscular tissue; probably because there are nerves in it. If two portions of a heart be held together by a very narrow isthmus of muscle, the contraction excited by irritating one will extend to the other; but this will not happen if the connecting isthmus be tendinous tissue, even though it be a piece of the auriculo-ventricular ring.

‡ Physiologische Scheikunde, St. vii, p. 664.

§ Müller's Archiv, Hefte iii, iv.

‖ Oesterreich. Medic. Jahrbucher, Mai u. Juni, 1845.

trunk of the artery crosses the crico-thyroid membrane and reaches the opposite side. It gives branches to the larynx through the crico-thyroid membrane, to the middle lobe of the thyroid gland, to the lobe of its own side, and when it crosses over, to that of the other side also, and very often to the isthmus of the gland.*

Capillaries. Observations on the development of the capillaries in tadpoles and the tails of youug tritons by Platner† indicate, as some others have done, that new capillaries are formed by outgrowths from old ones. First, capillaries are seen which have abrupt blunt closed ends; some of these have no trace of prolongation, but many exhibit a very thin, long *outrunner* which is gradually lost sight of; and in some cases it may be seen how two such thin canals as these unite and form one arch, which gradually increasing becomes a new capillary loop. At first this is filled by finely granular matter, and is too small to admit blood-corpuscles. It soon acquires distinct walls, but the nuclei found on fully developed capillaries are subsequent formations.

Veins. Henle described generally the differences of structure of veins in different parts of the body according to the degree in which the longitudinally fibrous coat is developed; that in some it is hardly discernible, in others strongly marked; but he did not indicate the veins thus differently conditioned. The result of many dissections by Dr. Norman Chevers‡ is that, as a rule subject to but few exceptions, the deep-seated veins of the trunk have their proper or middle coat (wherein I suppose he includes all between the striated coat and the external cellular coat) composed almost entirely of circular fibres; while in the external and superficial veins there is in the same situation a strong internal layer of longitudinal fibres, and a thinner layer of circular fibres next external to it.

Venous pulse. Two cases of pulsations in the veins of the back of the hand, coincident with the pulse of the arteries, are recorded by M. Martin-Solon.§ Both occurred in patients who had been largely and repeatedly bled for pleuropneumonia. M. Poiseuille, in a discussion on the essays, referred to cases recently recorded in a thesis by Condret, in which the persons presenting the venous pulse were strong young men with full arterial pulses.||

RESPIRATION.

Structure of the lungs. Some interesting points in the anatomy of the lungs are determined by the investigations of Mr. Rainey ¶ The air passages he divides

* For the practical deductions from these anatomical facts I must refer to the original paper.

† Müller's Archiv, 1844, Heft v. ‡ Medical Gazette, Aug. 8, 1845.

§ Bull. de l'Académie de Médecine, Nov. 1844, pp. 102-116.

|| All the physics of the circulation are well discussed by Bergmann in his article, "Kreislauf des Blutes," in Wagner's Handworterb. der Physiologie. There is an exposition of the theory of the wave-movement of the blood in the arteries, and of the mechanism of the pulse, by Dr. H. Frey, in Müller's Archiv, 1845, H. ii, iii. His essay is in full support of the theory as commonly received: against it there are, an anonymous very clever essay "On the Cause of the Pulse," in the Medical Gazette, July 11, 1845, and one with the same title by Dr. Thomas Williams, in the same, July 25, 1845. The opinions in these two essays differ in degree rather than in kind. It seems to me that the existence of what may be justly termed a wave of blood raised by each contraction of the ventricle, is quite certain: the main question is, how far does the first wave extend? None of the essays answer this question; and it is not possible to give an answer which shall be always true, for both the breadth and height of the wave must, in different cases, depend on very many variable conditions, of each of which we can as yet obtain only an uncertain estimate. Other works of more or less interest in the anatomy and physiology of the circulatory system are,—Parchappe, Sur la Structure . . . du Cœur; Paris, 8vo, 1844. Pliny Earle, Observations [with extensive statistics] on the pulse of the Insane; in the American Journ. of Med. Science, January 1845. Norman Chevers, On the effects of obliteration of the Carotid Arteries; in the Medical Gazette, Oct. 31, 1845. Bouisson, Sur les lésions des artères fessière et ischiatique; in the Gazette Médicale, Mars 1845, containing measurements of the exact relative positions of these vessels. Prevost et Lebert, Sur le développement des organes de la circulation . . . du poulet; in the Annales des Sc. Nat., t. i, 1845.

¶ Medico-Chirurgical Transactions, vol. xxviii, p. 581, 1845.

into the bronchial tubes and the intercellular passages continued from them, taking for the line of demarcation between them the parts at which the mucous membrane and ciliary epithelium of the bronchi terminate. These terminate abruptly in bronchi of from $\frac{1}{50}$ to $\frac{1}{30}$ of an inch in diameter; the mucous membrane retaining to this boundary its longitudinally and circularly fibrous character and its pale colour. Beyond the boundary, the tubular form of the air passages continued from the bronchi is for some distance retained, but it is more and more lost as the passages branch and approach the surface of a lobule, and as the number of air-cells which open into them continually increases. Thus, at last, each minute division of the air passages becomes quite irregular in its form, air-cells opening into every part of it, and almost constituting its walls, till itself ends, without dilatation, in an air-cell. Beyond the boundary at which the bronchial mucous membrane abruptly terminates the walls of the air-passages are formed only by a membrane similar to that which walls-in the air-cells; and they have no epithelium. Those air-cells are smallest and most vascular which are situated nearest to the centre of the lung; their size increases and their vascularity diminishes at the parts nearer the circumference—in adaptation, probably, to the larger and more ready supply of fresh air to the former parts. Those cells which open directly into the bronchial tubes and intercellular passages open into them by large circular apertures; they are themselves similarly opened into by other cells, which again are similarly opened into by others; forming thus, for each opening into a tube or passage, a series of communicating cells; each series reaching usually from the passage to the surface of the lobule. The cells which are placed in the angles of the branching of an intercellular passage probably open into both the branches, as well as into one another. The walls of the air-cells, on which the capillaries are placed, are formed by thin and transparent but fibrous membrane, which in the formation of the successive communicating cells is folded, or as if constricted, so as to form a definite sharp-edged circular opening of communication between each two, and between the first of the series and the passage into which it opens. In the reptiles the corresponding folds bordering the sacculi of their lungs inclose each a double or folded layer of capillary plexus; in the mammal's lung each such fold incloses a single layer of capillaries, so that both sides of all these capillaries are exposed to the air in the adjacent cells. And this plan of arrangement and structure of the air-cells exists in the mature mammalian fœtus just as it does in after-life.*

Nerves of the lungs. In his admirable essay on the physiology of the nerves, Volkman,† to prove the influence of the pneumogastric nerves on the bronchial tubes, relates an experiment analogous to those performed by Dr. C. J. B. Williams. He tied a glass tube drawn fine at one end into the trachea of a beheaded animal, and when the small end was turned to the flame of a candle he galvanized the pneumogastric trunk; every time he did so the flame was blown, and once it was blown out. The experiment succeeded even when the chest was opened; but the lungs having then collapsed, the efflations were slighter. The movements following the irritations were pulse-like.

Air-changes in respiration. Dr. Marchand,‡ from experiments on frogs, confirms the observation of Valentin and Brunner,§ that there is always more oxygen absorbed in respiration than is employed in the formation of the carbonic acid expired. The surplus combines with hydrogen, and forms water. When the animals were deprived of food, less carbonic and more water were

* There is a paper on the anatomy of the lungs by Dr. Eichholtz in Muller's Archiv, H. v, 1845, in which he tries to prove that the nuclei of certain cells, like hepatic cells, which he finds in the lungs, are transformed into blood-corpuscles.

† Wagner's Handworterb. der Physiologie. Art. Nervenphysiologie, p. 586.

‡ Report of the Scientific Meeting at Bremen, in the Medic. Central-Zeitung, Oct. 9, 1845.

§ See last Report.

formed. He found that frogs would not live more than three hours in hydrogen, however pure; they soon went to sleep, and gradually died.

Dr. Vierordt* has performed on himself a series of 578 experiments to determine, chiefly, the variations produced by different internal and external conditions in the results of the respiratory process. The full evidence for his deductions is given in numerous tables.† He shows, in a first section, the influence of variations in these following conditions: 1. *The time of day.* From 9 to 10 a.m. the pulse becomes less frequent; from 10 to 12 it is stationary; from 12 to 2 it increases fast and much; from 2 to 7 p.m. it regularly falls to the frequency of 9 a.m. In the same periods there are nearly corresponding changes in the number of respirations; but at 7 p.m. they are less numerous than at 9 a.m. There is a slight increase in the quantities of air and of carbonic acid expired in a minute between 9 and 10 a.m.; a slight decrease between 10 and 11: a greater decrease between 11 and 12; a very great increase between 12 and 2 p.m.; and a nearly regular and great decrease from 2 to 7 p.m., at which latter time they are much less than at 9 a.m. The increase from 12 to 2 is probably due to the influence of feeding and digestion, for the chief meal was between 12 and 1, and if it were not taken the respirations diminished before 2 p.m. The other changes also are probably due to variations in other internal processes rather than to those of external circumstances. 2. *The effects of dinner* are to produce an increase, *per minute*, in the pulse of 16·3 beats; in the respirations of 1·72 times; in the air expired of 269 cubic inches; in the carbonic acid expired of 19·37 inches. The evening meal produces similar but slighter effects in all but the frequency of the pulse, which is unaffected by it. 3. *Spirituous drinks*, as Dr. Prout showed, diminish the exhalation of CO_2 by $\frac{1}{2}$ per cent., or $\frac{1}{8}$ the whole quantity previously exhaled in a given time; and they do this especially when taken into an empty stomach. 4. *Bodily exercise*, in moderation, makes the exhalation of carbonic acid about $\frac{1}{3}$ more than it is during rest; and for about an hour after exercise there is an increase of about 118 cubic inches in the air expired per minute; and an increase of 14 per cent. in the proportion of CO_2 contained therein. 5. *Sleep.* Vierordt has not examined the products of respiration during sleep, but he fully confirms Dr. Prout's observation that there is a great, though quickly transitory, increase in the exhalation of CO_2 directly after, or even in the act of, waking. 6. *A change of external temperature* equal to 10° F. produces the following changes in the rates per minute: in the number of respirations, ·28; in the quantity of air expired, 33·4 cubic inches; in the carbonic acid expired, 2·114 cubic inches, or ·0183 per cent. These changes are by increase when the external temperature diminishes, and by diminution when it increases. The temperature in which the observations were made ranged between 38·7 F. and 75·7 F. Within this range the pulse is unaffected by the changes. 7. *A rise in the barometer* equal to half an inch increases the rate per minute, of the pulse, 1·3; of the expirations, ·74; of the quantity of air expired, 230 cubic inches; and, as Dr. Prout showed, diminishes the relative proportion of CO_2 expired by ·3 per cent. The barometric pressures in which the observations were made ranged between 29·3 and 30·2 inches. The influence of changes of atmospheric pressure was greater when the temperature at the time was high.

In a second section Dr. Vierordt discusses the influence of *variations in the respiratory movements* on the products of respiration: 1st, *In various frequencies of respiration* the proportionate quantities of CO_2 in the expired air are, with six respirations in the minute, 5·528 per cent; with 12, 4·262; with

* Physiologie des Athmens; Karlsruhe, 1845, 8vo.

† The mode of investigation is also detailed; but, as it was the same in all the experiments, it is unimportant to notice it here. Moreover, I have noticed only the facts obtained by his investigations; but the work contains, besides these, many useful deductions from the labours of others.

24, 3·355; with 48, 2·984; with 96 respirations in the minute, 2·662 per cent.* But the absolute quantities of CO_2 exhaled in a minute in the same circumstances are (since by rapid breathing the whole quantity of air that is breathed is proportionally increased) as follows: with 6 respirations per minute, 171 cubic inches; with 12, 246 cubic inches; with 24, 396 cubic inches; with 48, 696 cubic inches; with 96, 1296 cubic inches. 2d. *By increasing the volume or depth of the respiration*, the per centage proportion of CO_2 in the expired air is diminished; in the deepest respiration it is 1·97 per cent. less than in ordinary breathing. But for this proportionate diminution also there is a full compensation in the greater total volume of air which is thus breathed; for equal volumes of expired air, whether breathed slowly or quickly, deeply or not, contain equal quantities of CO_2. 3d. It appears, moreover, that if the air in one ordinary expiration contains on an average 4·48 per cent. of carbonic acid, the air expired in the first half of the whole expiration contains ·76 per cent. less than this average, and that expired in the last half contains ·96 per cent. more than the average, proving that the air in the parts of the lungs nearest the great bronchial tubes contains less CO_2 than that in the deeper and more distant parts does.† 4th. *In holding the breath*, the proportion of CO_2 in the air within the lungs increases greatly, but in a diminishing ratio; in one minute's holding it becomes 2·42 per cent., and in 100 seconds 3·08 per cent. more than it is in an ordinary expiration. In this condition also the composition of all the air within the lungs very soon becomes uniform.

Finally, Vierordt shows how wide the range of occasional variation is within which, even in health and perfect bodily rest, the several parts of the respiratory functions are discharged. In these conditions the pulse ranged, in occasional instances, between 54 and 101 per minute; the respirations between 9 and 15; the expired air between 1637 and 3676 cubic inches; the carbonic acid therein contained, between 3·358 and 6·22 per cent.; the volume of each expiration between 144·6 and 275 cubic inches.

The changes in the quantity of carbonic acid exhaled in much higher and lower temperatures than Vierordt observed them in have been investigated by M. Letellier.‡ The experiments were performed on small birds, mice, and guinea pigs, who were made to respire for periods varying from half an hour to six hours. The results are stated in tables which show generally that the quantity of carbonic acid exhaled at a temperature between 82° and 104° was about as much below the average at ordinary temperatures as the quantity exhaled at temperatures about zero were above the same average; and the greatest quantity exhaled at the lower temperatures was about twice as much as the smallest exhaled at the higher temperatures. None of the animals experimented on could live without danger in a temperature equal to that which is natural to their own bodies; and all speedily died when the temperature was increased to 5° higher. At temperatures from 80° to 92° they lived and breathed quite tranquilly.

Hannover,§ also, has instituted experiments to determine the quantities of carbonic acid exhaled in certain diseases, and finds, 1, that chlorotic females exhale an absolutely larger quantity of it than healthy ones do; 2, that the quantity is much diminished in all cases of phthisis; and, 3, that it is little or not at all changed in bronchitis.

Asphyxia. Many interesting matters connected with the physiology of respiration are well discussed and illustrated by Mr. Erichsen,‖ in his essay on

* The author hence deduces a general rule for calculating the proportions, in which, however, he is probably premature.

† See, on the anatomical adaptation of the lungs to this condition, Mr. Rainey's Observations, at p. 553.

‡ Annales de Chimie et de Physique, Avril 1845, p. 478.

§ De Quantitate . . . acidi carbonici ab homine sano et ægroto exhalati; Havniæ, 1845, 8vo.

‖ An Experimental Inquiry into the Pathology and Treatment of Asphyxia; Edinburgh, 1845, 8vo.

Asphyxia. 1. To show that, although the cessation of the respiratory movements is not the sole cause of the cessation of the circulation in asphyxia, yet their continuance enables it to be continued longer than it will after they have ceased, artificial respiratory movements with a small quantity of air (a quantity too small to retard materially the occurrence of asphyxia,) were maintained in a dog till all the chemical respiratory changes had ceased, and the heart was motionless. On examination after death, the difference between the quantities of blood in the two sides of the heart was not nearly so great as in ordinary cases of asphyxia: the left cavities contained nearly as much blood as the right; proving that the respiratory movements, unassisted by the chemical changes, had for a time facilitated the passage of blood through the lungs. 2. To show that the arrest of the blood in the small vessels in asphyxia is not the consequence of the action of venous blood in the nervous centres, three dogs were so arranged that while one of them was being asphyxiated by ligature of the trachea, arterial blood might be propelled through its carotids from the carotids of the other two. Care was also taken to prevent the passage of venous blood through the vertebral arteries of the asphyxiated dog, and to prevent congestion of its nervous centres by opening one of its jugular veins. The result was that the dog was asphyxiated in the ordinary time, and that there was the usual amount of congestion of its vessels, although arterial blood had, throughout the experiment, circulated through its nervous centres.

3. Two experiments are related in evidence that, if the action of the heart be maintained, black blood may be propelled by it through a lung long after the chemical respiratory changes have ceased. Dogs were pithed, and while artificial respiration was being maintained the right bronchus was tied. As long as the heart's action continued (and it continued much longer than in ordinary cases of asphyxia), there flowed nearly as much black blood through a right pulmonary vein as of red blood through a left one.

4. A description is given of the contraction of the small arteries, and distension of the veins of the mesentery during asphyxia, as observed with the microscope. And this contraction is maintained to be the principal or sole agent in the obstruction to the passage of the blood observed in asphyxia, the black blood being regarded as a stimulant to the contractile coats of the small systemic arteries and pulmonary veins. [But the description does not disagree with the more probable opinion that the small arteries of the mesentery contracted only because less blood was impelled into them.]

Mr. Erichsen's conclusion from these and some other previous experiments* is that the cessation of the circulation in asphyxia depends upon all three of those circumstances, to each of which, by various former writers, it has been exclusively ascribed: viz., 1, the arrest of the respiratory movements; 2, and more importantly, the weakening of the heart's action in consequence of the lessened quantity and altered quality of the blood which passes into the left ventricle, and the coronary arteries; 3, obstruction to the blood in its passage through the small vessels. [*i. e.* as he thinks, by the refusal of the minute pulmonary veins and systemic arteries to receive venous blood, but, as others believe, because the capillaries cannot transmit blood charged with carbonic acid.]

The average periods, in minutes and fractions thereof, at which the chief phenomena of asphyxia are observed in dogs are stated thus: voluntary movements cease in $1\frac{3}{4}$; involuntary in $2\frac{1}{2}$; the blood in the arteries becomes as black as that in the veins in $1\frac{3}{4}$, or, occasionally, in $1\frac{1}{4}$; ventricular contractions cease in $9\frac{1}{2}$ (at the earliest in $6\frac{1}{2}$, the latest, in adult animals, in 14); the left auricle ceases to act in 18 minutes, the right in $19\frac{1}{2}$, but once, in an adult animal, the former continued acting for 37, and the latter for 44 minutes;

* Medical Gazette, 1842.

pulsations in the femoral artery continue for 7½ minutes. In puppies four days old the ventricles contracted regularly for 117 minutes, and with irregular movements for three hours; and the auricles continued acting for three hours and twenty-five minutes.

ANIMAL HEAT.

Liebig* has shown the error of the opinion derived from Dulong and Despretz, that the quantity of heat generated by an animal which consumes by respiration a certain quantity of oxygen in a given time, is less than would be produced by the direct combustion of carbon and hydrogen in the same quantity of oxygen. He proves that this opinion is founded on incorrect premises; the combustion-heats of carbon and hydrogen having been reckoned too low by both Dulong and Despretz; and having established the authority of new and more accurate numbers to represent these combustion-heats, he shows that the amounts of heat developed by animals consuming certain proportions of oxygen, are nearly equal to those which are produced by the direct combustion of definite proportions of carbon and hydrogen in the same quantity of oxygen.

For the sake of their relation to the foregoing observations of Vierordt, I place here some of these by Gierse,† whose work should have been noticed in the Report for 1842-3. At different periods of the day, he found the temperature under his tongue as follows:—from 11 P.M. to 2 A.M. 98·26° F.; from 6 to 8 A.M, before breakfast, 98·55° F.; from 6 to 8 A.M on other days, after breakfast, 98·73°; between 9 and 11 A.M., 99°; just before dinner, 98·82°; after dinner, 99·5; between 3 and 6, P.M., 98·61°; between 6 and 10 P.M., 98·86°; from 11 P.M., to 1 A.M., while asleep, 98·15°. All the other observations by this author relate to the temperature of inflamed parts, except a few from which he believes that there is no increased heat in the vagina during menstruation or parturition. He found almost constantly that an inflamed part communicated its heat to the thermometer more rapidly than a healthy part of the same temperature did. He found also that, in animals, struggling and excitement commonly raised the temperature more than the inflammation of a part did.

Some of these results also, are confirmed by Dr. John Davy.‡ He finds the temperature highest in the morning, on rising after sleep; high, but fluctuating, till the evening; and lowest about midnight, ranging from 98·7° to 97·9°. In active exercise, within the limits of exhaustion, the temperature of the body is increased in direct proportion to the exertion; in passive exercise in the cool air it is lowered; during quietude in an atmosphere from 42° to 32°, it may be reduced from 1° to 2°. Sustained mental exertion slightly raises the temperature; a light meal scarcely alters it; a heavy one lowers it.

Sundry observations are recorded by Dr. Dowler and other American surgeons§ of considerable increase of temperature of the surface of the body after death from yellow fever and coup de soleil. Within a few minutes after death the temperature is said to rise to 102°, and then to increase steadily to 106° or 108° or even to 113°. Thus it may remain for from four to six hours, being usually higher at the thigh and abdomen than in the heart; then it gradually subsides to the temperature of the surrounding medium. The cases are said to have all occurred in the summer, in a temperature of at least 80°; but they are very imperfectly related. In the paper first referred to in the subjacent notes, another phenomenon is mentioned, which is certainly very rarely if ever observed in this country, namely, the maintenance of the power of forcible contraction in the voluntary muscles for six or seven hours after death. When all the muscles of the fore-arm are dissected bare (the arm

* Lancet, Feb. 22, 1845, and Annalen der Chemie und Pharmacie, Jan. 1845.

† Quænam sit ratio caloris organici, Dissert. inaug.; Halæ, 1842, 4to.

‡ Proceedings of the Royal Society, No. 61, June 19, 1845.

§ B. Dowler (New Orleans), in the Boston Medical Journal, Aug. 6, 1845; in the New York J. of Medicine, Sept. 1845; in the Medical Examiner, Sept. 1845.

being amputated at the shoulder) a blow with the hand will excite powerful contractions in the bared muscles; and twenty minutes or more after death a large hatchet weighing three pounds being tied in a subject's hand, it is said that the fore-arm was bent several times so as to raise the hatchet and strike the trunk as often as the flexor muscles were struck with the hand.

DIGESTION.

STRUCTURE OF THE DIGESTIVE ORGANS; *The Tongue*. Dr. Nuhn* has minutely described a pair of glands in the substance of the lower part of the apex of the tongue. Each gland lies about two lines from the median line, just below the ranine artery, on the outer side of the expanding branches of the lingual division of the trifacial nerve, under some longitudinal fibres of the anterior part of the stylo-glossus. It has the structure of a salivary gland, and measures from 7 to 10 lines in length, from 3 to 4½ in breadth, and from 1½ to 2½ in thickness. It has, at least, five ducts, which open through the mucous membrane over it by small orifices, each surrounded by a slightly elevated ring; it receives many branches from, and sometimes surrounds, the ranine artery. Dr. Nuhn has at present found the gland in man and the orang alone; he has sought it in vain in many other mammalia; he thinks therefore it may be a mucous gland whose fluid facilitates the movement of the tongue in speaking: [it looks exactly like a salivary gland].

Structure of the Palate.† Dr. Tourtual‡ has detected a new muscle of the posterior nares and palate, and found it regularly in six men as well as in dogs, cats, and sheep. It lies between the mucous membrane and the internal pterygoid plate, before the Eustachian tube, and behind the inferior meatus of the nose. Its upper edge or origin extends from below the palatine process of the turbinated bone backwards and upwards, towards the upper margin of the Eustachian tube; in this line the muscle arises from the vertical plate of the palate bone, and from the internal pterygoid plate of the sphenoid, reaching sometimes to the cartilage of the Eustachian tube; hence its fibres straight descend, and are augmented by others arising from the several parts near which they pass; and going just behind the hard palate, they enter the anterior and outer part of the soft palate, in which they expand on an aponeurosis, mingling with that of the circumflexus palati. The posterior margin of the muscle is usually confused with the anterior fibres of the levator palati, and is covered by a short fold of mucous membrane, which forms the anterior margin of the orifice of the Eustachian tube.

The discoverer proposes for this muscle the name of pterygo-palatine, or levator palati mollis anterior seu minor.§ It receives a (probably sensitive) filament from the internal palatine branch of the fifth; and has, probably, also motor fibres from the glosso-pharyngeal. The function of the pair of muscles is probably that of elevating and of slightly stretching transversely the *anterior* and lateral parts of the soft palate, for which the larger and posterior levatores palati do not provide. In this tension of the palate they probably prevent the anterior fibres of the circumflexi from drawing the anterior part of the palate backwards. They probably are important in sounding the palatine consonants, and their posterior portions may assist the greater levatores palati in narrowing the Eustachian orifices.

Structure of the Liver. An elaborate anatomy of the liver, with measure-

* Ueber eine bis jetzt noch nicht näher beschr. Drüse im innern der Zungenspitze; Mannheim, 1845, 4to. I supposed he had discovered these glands, but Schlemm (Müller's Archiv, H. v, 1845) shows that they were discovered by Blandin, and decribed in his Anatomie Topographique, p. 175; Paris, 1834.

† On the Structure of the Teeth; see, in a future part, Prof. Owen's account of their development.

‡ Müller's Archiv, 1844, Heft v, p. 452.

§ This name had better be used; for the former has been already applied to what is probably a part of this muscle, arising from the hamular process of the pterygoid plate, and mingled with fibres of the pterygo-pharyngeus. See Haller, Elem. Physiol. t. vi, p. 80, note *t*.

ments of all its measurable parts, is published by Theile.* He confirms Mr. Kiernan's account of its lobular structure; for though he admits that the investment of each lobule, which is so dense in the pig and some other animals, may be absent or very thin in the human liver, yet he urges, and very rightly, that the latter is, by the arrangement of its vessels, divided into lobules, just like those of the pig and others. For in the human liver, as in theirs, each portion thus bounded by vessels, if not by a distinct capsule, is a miniature liver, having in itself a complete bile-secreting apparatus. He confirms also the account of the hepatic cells being arranged in networks with a radiated plan; but he denies the existence of the vaginal branches and plexuses of the portal vein as described by Mr. Kiernan. These vaginal plexuses, he says, "are derived, not from the portal veins, but from the hepatic arteries, from which they are completely filled when both arteries and veins are at the same time injected; and when they appear to be injected from the portal vein, it is because the injection has passed through those vessels by which the blood of the hepatic artery is carried to the portal vein. The interlobular portal veins are therefore, he says, derived directly from the portal veins; and those which appear to be vaginal branches of the portal vein are its *internal roots*, by which it receives the blood which has served for the nutrition of the hepatic ducts and other vessels of the liver. But that which is most new in his essay is the account of the glands of the biliary ducts. The orifices of these are, as Mr. Kiernan showed, arranged in two opposite rows through the whole extent of the ducts within the liver, as far as they can be dissected; an arrangement peculiar to the human liver. Their number is less in the human than in the other livers which Theile examined, but their form is more complex. They generally consist of an elongated tortuous canal, bearing alternate small sacculi and pedicled bunches of cellules; reminding one of the Meibomian glands. These canals, moreover, branch, and their branches anastomose with one another, and with those of the adjacent glands. Such a net-like connexion of the gland-canals occurs within the Glisson's capsule, investing the larger and middle-sized hepatic ducts; but it is most remarkably developed in the transverse fissure of the liver. Here, after the hepatic duct has been well injected, red streaks, forming plexuses, can be seen, which are nothing but large examples of these glands.† The canal-shaped glands in this situation are a quarter of a line in diameter, (in other parts they are from one fifth to one tenth); their walls are beset by pouches and bundles of sacculi, some of which are one sixth of a line in diameter, and there open into them at short intervals elongated glandules, just like those found in the prolongations of Glisson's capsule, except in that they do not open directly into the hepatic ducts.

Krause‡ suspects that these glands of the hepatic ducts, as well as the vasa aberrantia of Weber, are incompletely injected bile-ducts, [or, perhaps, they should rather be considered as such ducts imperfectly developed; for if Krause's account of the acinous structure of the liver be true, no line of essential distinction could well be drawn between them and the more perfect ducts]. He holds still§ that all the bile-ducts have acini at or near their ends, having confirmed his opinion by recent numerous injections. He agrees generally with Kiernan and Weber's account of the reticular arrangement of the minutest hepatic ducts, having often seen this by injection; and he adds that, on many of those ducts he has injected, and has seen, after dissolving the injection that was in them, regular round and oblong vesicles or acini, from $\frac{1}{750}$ to $\frac{1}{350}$ of an inch in diameter; many of these acini, grouped upon their several ducts, compose each lobule, each group of acini having one duct, and therefore each lobule having probably several ducts, or roots of the hepatic

* Wagner's Handwörterbuch der Physiologie, art. Leber.

† E. H. Weber has described these as a part of the vasa aberrantia of the hepatic ducts, similar to those in the left lateral ligament and some other situations. See last Report, p. 29.

‡ Müller's Archiv, Heft v, 1846.

§ His former paper is in Müller's Archiv, 1837; and the same account is given in his Handbuch der Anatomie.

ducts, which proceeding from it enter into the lobular plexus of ducts. He says also, that the acini may often be discerned entire at the edges of very thin sections of the human liver; they appear within the lobules as regular, round, or oval, yellowish-gray and very thin-walled corpuscles or cells, invested by very fine and short fibro-cellular fibrils, and each containing from six to eight hepatic cells, with, in many cases, bile.

M. Natalis-Guillot* has also, (if I rightly apprehend his expressions), confirmed by injections the account given in the last Report of the retiform arrangement of the minutest bile-ducts. These, he says, surround, either in a net, or in dense tufts, the whole surface of each lobule, and spread over the surface of each of the "ultimate ramifications of the vena portæ," i. e. of the interlobular portal veins. The ducts themselves, he thinks, are surrounded by minute branches of the hepatic artery.

PROPERTIES OF THE DIGESTIVE FLUIDS. *Saliva.* Experiments by M. Magendie† and others on the saliva of the horse have shown a difference between that secreted by the parotid gland and the mixed saliva from all the glands. The parotid saliva obtained from a fistula into the duct is more alkaline than the rest [as Mitscherlich also showed]. It contains carbonate [?] of potass which the rest does not contain, and a much larger proportion of ptyaline and albumen, the latter constituting one fifth of the whole solid mass. The parotid saliva also has no influence on starch-paste or raw starch, at a temperature from 104° to 167°; but the mixed saliva speedily transforms the starch-paste into sugar at 104°, and at the same temperature acts slowly but evidently on raw starch and coagulated albumen. This mixed saliva, obtained from food, masticated and swallowed into the œsophagus, is not limpid and clear like that from the parotid, but yellowish-gray, easily becoming turbid. It is weakly alkaline, and contains no carbonate, but abundant alkaline chlorides. There is a great "analogy" between the different kinds of saliva poured into the mouth of man, and those just described as secreted in the horse.

Gastric Fluid. The researches of M. Blondlot,‡ from which he deduced the presence of an acid phosphate of lime in the gastric fluid, are partly confirmed and partly opposed by those of Dr. R. D. Thomson.§ Thus, like Blondlot, he has never found a volatile acid in the stomach if digesting animal food alone; and he disproves the presence of hydrochloric acid in the gastric fluid during the digestion of either kind of food. But, contrary to Blondlot, as well as to the experiments next mentioned, he says, that the acid of the stomach can be saturated with chalk; and that therefore it cannot be an acid phosphate of lime. What the acid is, his experiments leave uncertain; but in the digestion of vegetable substances, he finds a volatile acid always present in very minute quantity, and a fixed acid which, he says, resembles the lactic more nearly than any other.

MM. Bernard and Barreswil hold that the gastric acid is the lactic. Their experiments,‖ like those of M. Blondlot, show that it cannot be either the acetic or hydrochloric—the first cannot be distilled from it; and, though the second can, yet it is only at the last, when the chloride salts are decomposed by the acid really present. Moreover, a minimum of oxalic acid produces a precipitate of oxalate of lime, which it could not do if even $\frac{1}{5000}$ of free hydrochloric acid were present. But they show that M. Blondlot failed to observe effervescence on adding carbonate of lime to the gastric fluid, because of the state of dilution of the fluid: when it is concentrated by evaporation distinct effervescence ensues. This effervescence proves the existence of some acid besides the phosphoric; yet, since the addition of an excess of carbonate of lime cannot completely neutralize the acid, it is highly probable that a small

* Comptes Rendus, 18 Novembre, 1844.

† Archives Gén. de la Médecine, Nov. 1845, from the Acad. des Sciences, 20 Oct.

‡ See last Report, p. 24.

§ On the digestion of vegetable albumen, &c. Philos. Mag., May, 1845; and Lancet, May 17, 1845.

‖ Comptes Rendus, 9 Décembre, 1844.

proportion of phosphoric acid free, or in an acid salt, is present in the gastric fluid and gives it part of its acid reaction. The other acid with which the carbonate of lime effervesces is, the authors believe, the lactic; and they show that in every essential respect the conduct of the gastric fluid with various tests is similar to that of water acidulated with lactic acid, and with the addition of a little chloride of sodium.

The experiments also of M. Melsens* confirm some of these, proving that both marble and other carbonates of lime lose weight when immersed in gastric fluid, and that there must therefore be a free acid in it.

Bile. The most careful examinations of the urine and blood of a patient with intense jaundice did not enable Scherer† to detect in either of them, a trace of any constituent of bile except the colouring matter and cholestearine. In evidence of the speedy transformation which the biline would probably undergo in the blood, he mentions that in a large quantity of green fluid vomited, and containing abundant biliary colouring matter, he could not detect a trace of the biline which it must previously have contained. In the same essay he gives an accurate account of his analysis of the biliary colouring matter which he collected from the patient's urine.

The conclusion respecting the non-existence of the essential principles of the bile in the fæces is confirmed by the delicate test for bile invented by Pettenkofer.‡ To the fluid supposed to contain bile ⅔ of its volume of sulphuric acid are added by drops, that the temperature may not rise above 140° F., and then from two to five drops of a solution of sugar (one to four parts of water). Presently a reddish violet colour appears, intense in direct proportion to the quantity of bilic acid. By this test no bile (except the colouring matter) could be found in healthy fæces; but the fæces of diarrhœa and those discharged after purgatives contain complete bile. So also, by this test, bile could always be found in the urine of the pneumonic.

Dr Redtenbacher§ has found that taurine contains 26 per cent. of sulphur, and his discovery is confirmed by Dr. Gregory.‖

Pancreatic fluid. Twenty-eight grains of the pancreatic fluid of an elephant, collected eight days after death, yielded to Professor Bergmann,¶ of Bonn, 92·77 per cent of water and 7·33 per cent. of albumen, caseine, and a minute quantity of chloride of sodium and carbonate of soda.

Digestive Principles. MM. Barreswil and Bernard** also maintain that the active organic digestive principle (presently again to be mentioned) is the same in the saliva, the gastric fluid, and the pancreatic secretion; and that the special action which it appears to exhibit in these several fluids is due only to its being combined with an acid in the gastric fluid, and with an alkali in the others. They say that from whichever of these three sources this principle is obtained it will, if an acid be added to it, digest meat, gluten, and other azotised compounds, and act like artificial gastric fluid; but if it be made alkaline it will only be capable of digesting the amylaceous principles, and thus will be an artificial saliva or pancreatic fluid. So also, by making gastric fluid alkaline, it will act like saliva or pancreatic fluid; and by making either of the latter acid it will act like gastric fluid.

Process of Digestion. The principal researches of the year to be placed under this head have had reference to the digestion of the saccharine and amylaceous principles of food. The most considerable are those of MM. Bouchardat and Sandras,†† whose results are as follows:

* Comptes Rendus, 9 Decembre, p. 1289.
† Annalen der Chemie und Pharmacie, Marz, 1845; and Lancet, May 24, 1845.
‡ Annalen der Chemie u. Pharmacie, No. lii, p. 90.
§ Annuaire de Chimie, p. 724, from the Comptes Rendus, t. xx, 1845.
‖ Outlines of Chemistry, Part ii, p. 566.
¶ Oesterr. Med. Wochenschr., Dec. 14, 1844, from the Med. Corresp. Blatt Rhein. u. Westphal. Aerzte, No. 17, 1844. Much more concerning the nature of the pancreatic fluid will be found in the following paragraphs.
** Gaz. Méd., 12 Jul. 1845, from the Report of the Acad. des Sciences, 7 Jul.; Comptes Rendus, 25 Jul.
†† Report from the Académie des Sciences, 20 Jan. 1845, in the Gazette Médicale, 25 Jan. 1845.

When cane-sugar is given to dogs it is found, either unchanged or converted into *sucre interverti* or lactic acid, in the whole length of the digestive canal. After it has been given for several days it may be found in the urine, and in the bile, blood, and chyle. When cane-sugar is introduced directly into the blood it passes into the urine; but when the same quantity of *sucre interverti* or lactic acid, was so introduced none was detected in the urine. In order that cane-sugar may be destroyed in the blood (i. e. ultimately reduced to carbonic acid and water), it must first be transformed into *sucre interverti*, or lactic acid in the digestive canal.

Raw starch is very imperfectly digested by men and carnivora. The greater part of it may be found unaltered in the excrements. A greater effect is produced on it by the digestive organs of herbivorous rodents. It undergoes no change in their stomachs; but in the contents of their small intestines (which are always alkaline, except sometimes at the pyloric part of the duodenum) there are found, together with entire starch granules, others that are cracked, eroded, or almost wholly destroyed; and, also, dextrine with traces of grape-sugar. The cæcum contains an acid paste, in which there are some entire starch granules, dextrine, grape-sugar, and lactic acid. The same materials are found in the rectum. The dextrine, grape sugar, and lactic acid may also be detected in the blood and the bile, but not in the urine of these animals; and the blood of their vena portæ contained more water and more of these three substances than their arterial blood did—making it sure that they are absorbed by the blood-vessels, not by the lacteals.

Graminivorous birds digest raw starch more completely than mammalia do. It undergoes no change in their crops; and in their gizzards, though there are traces of dextrine and grape-sugar, yet nearly all the starch-granules appear unchanged. In the small intestines the starch-granules are gradually more and more destroyed, and ultimately they undergo the same changes as in the intestines of the herbivorous rodents, but more completely. In both, the authors consider the changes to be due to the high temperature, the alkaline reaction, and the presence of a secreted principle which acts like diastase, only with less energy: all which conditions exist in greater force in the birds than in the mammalia.

When the starch-granules have been burst by cooking they are digestible by men and carnivorous animals. Their solution commences in the stomach, and is slowly continued through the intestinal canal. The products are dextrine, grape-sugar, and lactic acid; which are found mixed with some starch remaining unchanged.

If more than a certain proportion (one gramme, at the most, for an adult dog) of a feculent or saccharine principle be mingled at once with the blood, sugar is always eliminated by the kidneys. Hence the slow introduction into the blood of the products derived from the digestion of these principles appears to be an essential condition for their due disposal; and this condition is secured by two chief means, namely, the slowness of their solution, and the chief manner of their absorption. They are formed into soluble compounds in the intestines; and these being absorbed by the blood-vessels, are carried to the liver, where, if combustible matters are in excess in the blood, the greater part of them are secreted and discharged with the bile into the intestines, whence some of them may be absorbed with the other soluble constituents of the bile. "Thus then is established a limited circulation of the combustible matter, which, by this admirable artifice, is only gradually carried into the general circulation."

These conclusions concerning the transformations which starch undergoes in digestion are, to some extent, confirmed by Dr. R. D. Thomson,* whose experiments show that dextrine and soluble starch exist in the stomachs of pigs fed on farinaceous food during and for some time after digestion, and

* Philos. Mag., May 1845; and Lancet, May 17, 1845.

that sugar exists in the blood of the same animals in the proportion of from 2·57 to 8·05 grains in 1000 grains of the serum.

In a subsequent memoir* MM. Bouchardat and Sandras state that the principle which, as above-mentioned, appears to act like diastase in the transformations of starch is secreted chiefly by the pancreas. They find the pancreatic fluid of birds transparent, viscid, slightly alkaline, and capable of liquefying starch-paste, and of transforming it into dextrine and grape-sugar. Portions of pancreas cleared of blood and large vessels possess the same power in a very high degree; and no other organ besides the pancreas, and in a slighter degree the salivary glands, possesses such a power.† It is, moreover, wholly destroyed both in the pancreatic fluid and in the pancreas itself by such influences as destroy the like property in diastase, such as a temperature of 212°, tannin, mineral acids, metallic salts, &c.

The same influence which these authors ascribe to the pancreatic secretion is ascribed by M. Mialhe‡ to the saliva, from which he gives directions for obtaining the digestive principle, *animal* or *salivary diastase*,--by filtering it and then treating it with five or six times its weight of absolute alcohol. The diastase being insoluble in alcohol is thus precipitated in white flocculi. He describes the aqueous solution of this substance as insipid and neutral, not precipitable by subacetate of lead, and when left to itself undergoing a transformation into butyric or some similar acid. With raw starch this salivary diastase requires several days for the production of dextrine and sugar of starch; but with starch-powder the change is quickly effected; and with starch-paste it is very speedily completed if aided by a temperature of about 160°.§

The experiments of M. Lassaigne‖ confirm those of MM. Bouchardat and Sandras as to the properties of the pancreas and its fluid; and, at least in great measure,¶ those of M. Mialhe on the properties of the saliva. He shows that at the natural temperature of the body saliva has no effect on whole starch, and that mastication does not change the form in which it naturally exists in cereal grains; that horse's saliva does not act on starch even when its grains are broken; but that human saliva, though it does not affect raw and whole starch at a temperature of 100°, can even at a temperature of from 64° to 68° convert powdered starch partly into dextrine and partly into sugar of starch; the envelopes of the granules preserving at the same time their property of becoming violet when touched with iodine.

Influence of the Bile in digestion. Dr. Platner** has made experiments to find how the bile contributes to digestion. He has confirmed, what Simon and others showed, that the fæces contain none of the bile except its colouring matter [and some of its fat?]; and what Purkinje showed, that bile will put a stop to or prevent the artificial digestion of coagulated albumen. On mixing pure artificial digestive fluid, neutralized by carbonate of soda, with bile, no change took place; but on adding hydrochloric acid to the mixture it became very turbid. The same happened when bile was mixed with digestive fluid not neutralized; but hydrochloric acid added to bile alone produced no precipitate. The precipitate consisted of bilic acid united with some organic body, perhaps pepsin, explaining probably the fact quoted above from Purkinje. When bile was added to a solution of albumen in acetic acid, a precipitate

* Archives Gén. de Médecine, Mai 1845; Report from the Acad. des Sciences, 14 Avril.

† See in connexion with this subject a paper by M. Bouchardat, "Sur la fermentation saccharine ou glucosique," in the Annales de Chimie et de Physique, Mai 1845, t. 89, p. 61.

‡ Ibid., 5 Avril 1845; Report for the 31st of March.

§ According to Dr. R. D. Thompson (l. c.) the transformation of starch into dextrine is effected to some extent by boiling it for half an hour in pure distilled water.

‖ Arch. Gén. de Médecine, Mai 1845, from the Report of the Acad. des Sciences, 7 Avril.

¶ Ibid. Juillet 1845, from the Report of the Acad. des Sciences, 2 Juin. In the earlier paper his results were opposed to those of M. Mialhe.

** Muller's Archiv, 1845, Heft iv. He has since published a special work, "Ueber die Natur und den Nutzen der Galle," Heidelberg, 1845; but I have not yet received it. Another work relating to the physiology of the bile is H. Meckel, De genesi Adipis; Halis. 1845, 8vo.

was formed which was insoluble in all acids, but soluble in alkalies. When bile and albumen were mixed and acetic acid added, a precipitate like coagulated fibrine was formed; and a similar precipitate was formed by the agency of even carbonic acid; showing that although the bilate of soda (i. e. the pure principle of bile) retains its composition under the action of either acids or alkalies alone, yet it is decomposed easily by combinations of acids with organic substances. When bile was added to a solution of albumen or gelatine obtained by artificial digestion, precipitates were formed which were soluble in acetic acid and consisted of bilic acid united with the organic substance. Sugar and gum in like circumstances appeared to unite with the fatty matters of the bile.

Fæces. The usual microscopic constituents of human fæces are thus enumerated by Dr. Gobee.* 1. A large quantity of vegetable cellular tissue, with or without epidermis and hairs. 2. Vegetable hairs. 3. Vegetable spiral vessels. 4. Elongated quadrangular plates of light yellow colour in great abundance, of uncertain nature; they are not affected by acetic acid, and are insoluble in cold ether, but iodine displays transverse striæ on them. [Probably they are portions of muscular fibre. I have found such, tinged pale yellow by the bile, in the fluid discharged through an artificial anus.] 5. Large quantities of crystals of phosphate of ammonia and magnesia. 6. Fat-globules or cells in various quantity. 7. A great quantity of granules. 8. Few epithelium- and mucus-cells. 9. Much of the brown-colouring matter of the bile.

ABSORPTION.

Structure of the Lymphatics. The subject of one of Mr. Goodsir's excellent essays† is the structure of the lymphatic glands. At the points of connexion between the extra- and intra-glandular lymphatic vessels, the coats of the former (whether afferent or efferent) separate. The outer coat is continued into the external capsule of the gland, from which processes pass inwards, binding together the substance of the gland, and supporting the vessels within it. The middle, or fibrous coat, is usually nearly lost, as the vessels pass towards the centre of the gland. But the internal coat becomes thicker and more opaque in the intra-glandular lymphatics; and when, in any of these thickened, dilated, and oft anastomosing vessels, it is broken up, it appears composed of two substances; namely, first, a thin transparent membrane, in which ovoidal bodies, containing one or more minute vesicles, are imbedded, as "germinal spots,"‡ at regular distances; and secondly, thick layers of close-packed spherical nucleated particles about 1-5000th of an inch in diameter, which make the vessel appear opaque, and leave only a narrow and irregular canal along its axis, the walls of which canal appear formed by them, not by any membrane lining them. The capillaries in the lymphatic glands ramify in contact with (not in the substance of) the external layer of their coats; as they do on the ultimate ducts of the true secreting glands; and they form as fine a network.

The general result of these observations is plainly favorable to the opinion of an intimate analogy between the lymphatic and the true secreting glands; of which some account was given in the last Report, and more will presently be said in speaking of the glands without ducts.

Process of Absorption. Some observations by E. H. Weber§ are said to prove that the chyle is first absorbed into the epithelium-cells covering the villi, which, at a certain period, are found full of chyle-globules, and from which it is transferred into the proper cells of the villi, to be by them conveyed to the lacteal vessels. And among the cells of the villi, it is said that

* Kliniek; Tijdschrift voor wetensch. Geneeskunde; voor G. C. Gobée, 1844, St. iv.

† Anat. and Pathol. Observations, No. viii, p. 44.

‡ The expression has reference to the author's general theory of nutrition: the bodies he describes appear to me identical in aspect with the nuclei or cytoblasts of many secreting gland-ducts.

§ Archives d'Anat. Gén. et de Physiol., Jan. 1846.

two peculiarly large ones are often seen in man during digestion, which touch each other, and of which one contains an opaque-white liquid, and the other a clear fatty matter.

Experiments on absorption have been performed by Mr. Fenwick,* and their results are very like those obtained by Herbst. 1. He relates two experiments to show that indigo will pass into the lacteals. 2. He relates many to prove that the lacteals do not absorb strychnia, or milk, or other food, from any part of the digestive canal in which the blood is not circulating. 3. He shows, as Herbst does, the passage of liquor sanguinis and blood into the lacteals when the adjacent blood-vessels are much congested. 4. In other experiments, oily matters (?) and prussiate of potash injected into the pleura of a rabbit were shortly after found in the lacteals. 5. Others again show that the action of the lacteals and lymphatics is independent of nervous influence. 6. And others confirm the fact already known, that they continue to propel their contents after apparent death. 7. From his experiments, from analogy, and from many ingenious, but I think insufficient, arguments, Mr. F. concludes that these vessels obtain their fluid neither by absorption, nor by secretion from the blood-vessels adjacent to them, but by parts of the contents of the blood capillaries, according to the degree of congestion, being directly and mechanically effused into them.

Propulsion of Lymph. An attempt has been made by Dr. Bidder† to determine the average quantity of lymph and chyle which flow through the thoracic duct in a given time. The measurements were made by collecting what flowed from the thoracic duct immediately after death. In five cats, the fluid continued to flow from one to six minutes, and, judging by the quantity collected in this time, the average quantity which would have flowed in an hour, was 373 grains (the extremes being 276 and 480 grains); and the average proportion between the weight of the cat and the weight of the chyle and lymph, which at the same rate, would have flowed in twenty-four hours, was as 5·34 to 1 (the extreme proportions being as 6·8 : 1, and as 5·1 : 1). In two dogs the average rate of efflux (similarly calculated) was 3858 grains in the hour; and the average weight of the dogs was, to that of the chyle and lymph which would have flowed in twenty-four hours, as 6·66 :1. Now, the average weight of blood in cats is, to the weight of their bodies, as 1:5·7; and of dogs, as 1:4·5; hence the quantity of fluid daily traversing the thoracic duct of a cat, is about equal to the whole quantity of blood in it; and the quantity of the same in a dog is equal to two-thirds of its blood.

Lymphatic hearts. Volkmann‡ has proved that the rhythmical movements of the lymphatic hearts of frogs depend on the direct influence of portions of the spinal cord. They cease on the instant of destroying the cord, though those of the blood-heart continue for many hours. But repeated experiments showed that the contractions of the anterior hearts would continue long while they retained a nervous connexion with the cord about the third vertebra; and those of the posterior hearts as long, if their connexion with the cord at the eighth vertebra was uninjured. The movements thus continued in the lymphatic hearts though the whole of the cord, except these portions, were destroyed; and on the instant of destroying either of these portions, though all the rest of the cord were intact, the movements of the corresponding hearts ceased. Removal of the brain had no influence. The movements continued also after the division of the posterior spinal roots (they were therefore not reflex), but they ceased directly on the division of the anterior roots.

* Lancet, Jan. 11, 18, 25, Feb. 1, 1845. † Muller's Archiv, 1845, Heft i.

‡ Ibid. 1844, Heft iv, and Wagner's Handworterbuch, art. Nervenphysiologie, p. 489. Valentin, in the body of his Physiologie (vol. ii, p. 767), denied the truth of these experiments, but in a later appendix confirms them. For other papers on the lymphatics, see Oesterlen, as presently quoted; H. Nasse, an elaborate article on the lymph in Wagner's Handworterbuch; and papers on some unusual arrangements of the lymphatics, by Svitzer and von Patruban, in Muller's Archiv, H. ii, 1845.

GLANDS WITHOUT DUCTS.

In the last Report I deferred the notice of Dr. Oesterlen's observations* on the vascular glands that I might include with it that of the then unpublished essays of Mr. Simon.† I am thus able to set out more briefly the results of the two most important works ever yet published on these organs, and this with advantage even to Dr. Oesterlen's work, for his facts gain importance and clearness by the corroboration and bright illustration which they receive from the truly admirable researches of Mr. Simon.

Thymus Gland. According to Mr. Simon, the earliest condition of the thymus gland is that of a simple tube of transparent homogeneous membrane, with granular and dotted contents. It presents at regular intervals elongated thickenings of its wall, which are probably the attenuated nuclei of a series of primordial cells, by the fusion of which the tube may be first formed. The tube has no connexion with the respiratory mucous membrane. In the next stage the tube (which remains in the axis of each half of the gland as its central cavity,) bulges at certain points of its length, forming diverticula or follicles, which communicate with and have the same structure and contents as itself. These usually assume hemispherical or pedunculated forms; and, in the next stage, themselves branch or form secondary and tertiary bulgings; and this is generally effected without elongation of the isthmus or pedicle by which the primary follicles were connected to the main canal, so that the secondary ones appear sessile. The progress of the development of the gland consists in repetitions of this process—the growth of follicles extends successively to all parts of the main tube; in each new crop of follicles are repeated the same acts of development and branching; and the whole substance of the gland enlarges by interstitial growth.

The gland continues growing through the whole "age of early growth;" and the period at which it attains its greatest size cannot be more nearly determined than the age of completed growth of the whole body can. Mr. Simon's observations on this point agree with those of Haugsted.

This account of the development, affords some notion of the mature structure of the thymus. According to both Simon and Oesterlen, whose observations now begin to coincide, it consists of a collection of polygonal mutually flattened membranous cells, from half a line to nearly two lines in diameter, the terminal vesicles or follicles of the gland. These are ranged in masses round a common axis; each mass forming a sort of cone, whose apex is directed towards the axis. The vesicles or cells are not completely close and separate, they are closed in about three fourths of their periphery; by the remaining part, each is attached to the general trunk of the glandular substance and opens into some diverticulum of its common cavity.

The walls of the vesicles are formed by the same kind of homogeneous membrane as the primitive tube; each having on its exterior a capillary network; groups of them are connected by investing areolar tissue, in which is mingled a small proportion of delicate elastic fibrils.

The vesicles are filled by a fluid and a multitude of corpuscles. The corpuscles, according to both observers, have the structure and relations of nuclei. They are generally circular, yet often deviate widely and variously from this form, and are flat and disc-like. Their average diameter is $\frac{1}{3850}$ of an inch; they are characteristically dotted, having from two to five very small dark spots, either scattered, or collected into a single corpuscle in their centres. In animals past the most active period of the thymus, there may be found cells in which these dotted corpuscles appear as nuclei, and which are, or become, perfect fat-cells.

* In his Beitrage zur Physiologie; Jena, 1843, 8vo, pp. 1-95.

† A Physiological Essay on the Thymus Gland; London, 1845, 4to; and, The Comparative Anatomy of the Thyroid Gland; in the Philosoph. Transactions, 1844, part 2.

The fluid of the thymus-cells contains (in the active period) proteine-compounds and traces of fatty matter; and the gland itself, on ultimate analysis, yields nearly the same proportion of essential elements as blood and flesh do.

The most important facts afforded by the comparative anatomy of the thymus, laboriously investigated by Mr. Simon are, 1st, that it "belongs, without exception, to all animals breathing with lungs, and to no others." 2. That in the hybernating rodent animals, the large persistent thymus and its prolongations into large masses which lie in the posterior mediastinum, are wholly composed of fat, the nuclei or cytoblasts which its cells contained having, at the approach of the hybernating period, genuine fat-cells formed round them. 3. That the thymus of birds, hitherto unknown, is a semitransparent ampullated tube, following the line of the superficial cervical vessels, and very early ceasing to discharge its functions. 4. That in reptilia, to which also a thymus was generally denied, its existence is constant. In the hybernating serpents also there is always appended to the persistent thymus, an accessory organ or fat-body. In the batrachia and some others, the thymus is at a very early period converted into a mass of fat, which is persistent. The larvæ of batrachia while breathing with gills have no thymus; its development begins as soon as their pulmonary respiration is established. Among the icthyoid reptiles with persistent gills, it is found in the menopoma, amphiuma, axolotl, and menobranchus, but not in the siren and proteus. 6. In fishes, a thymus is not found.

Thyroid Gland. According to Mr. Simon, the thyroid gland consists of a dense aggregation of completely closed vesicles, each formed by a layer of delicate homogeneous membrane (*limitary membrane*), invested by a close capillary network. These are the analogues of the cells or follicles of the thymus, but they are completely closed, and do not communicate with any central cavity. They are filled by fluid and cytoblasts, which are the analogues of those in the cells of the thymus: and are held together by fibro-cellular tissue. The cytoblasts are not materially different from those of the thymus; in the young animal they lie close together on the inner surface of the containing cell, like, Oesterlen says, "a tesselated epithelium;" but from this position they detach themselves and float freely in the fluid of the cavity. That they have the relation of nuclei is proved, according to Mr. Simon, by their being not unfrequently found as the nuclei of cells, about $\frac{1}{1100}$ of an inch in diameter. He shows also that a thyroid, or an organ representing it, exists in all vertebrate animals, appearing to have relation to the development of their nervous centres, always maintaining an intimate relation to the vascular supply of the brain, existing in certain fishes as a mere diverticulum to the cerebral circulation, and in the animals above them having a super-addition of glandular structure.

Spleen. Oesterlen and Mr. Simon agree that the Malpighian bodies of the spleen are not vesicles, but aggregations of cytoblasts (analogous to those of the vascular glands already described) which are herein collected in small bodies; but these have no inclosing cell wall; each lies within a kind of capsule of capillary vessels, receiving themselves no vessels into their interior. They are held together by an amorphous transparent substance with obscure fibres; among which Oesterlen believes he once detected lymphatic vessels. He has not always found the Malpighian bodies in any of the animals in which he has examined them. Most of the corpuscles or cytoblasts of the spleen, whether scattered through its red substance, or collected in the Malpighian bodies, appear like those of the thymus, but they vary in their contents; others are larger and are merely close-pressed aggregates of granules; others are partially surrounded by darker granules; and others, in different species, differ from all these, approaching the characters of blood-corpuscles.

Renal Capsules. Oesterlen has described the structure of these more minutely than that of any other vascular gland. He finds inconstancy in the distinction, proportions, and colours of the cortical and medullary substances. His account of the blood-vessels accords with Müller's. He finds no central

cavity (except that of the great central vein); but, occasionally, hollow, elongated spaces of conical shape, which have no lining membrane, and are empty, or contain a thick grayish-white fluid. The appearance of radiating striæ in the cortical substance of these organs is due to groups of the small corpuscles or cytoblasts of which these, like the other vascular glands, are chiefly composed; and of which, in these, many are grouped together with fat-cells and molecules in the form of nearly parallel cylinders or elongated cones; each group being, as Mr. Simon has discovered, inclosed in a tube of very delicate membrane. The medullary substance of the renal capsules, and that which intervenes between the tubes full of corpuscles consist, according to Oesterlen, of cytoblasts, uniformly scattered, and with these are mingled minute molecules, and small collections of fat-cells or particles.

The various microscopic objects found in the renal capsules are, according to both Oesterlen and Simon—(*a*) minute oil or fat particles, either scattered or grouped in round, oval and retort-shaped flattened masses; (*b*) similar groups of fat particles collected round the proper cytoblasts of the organs, but devoid of separate cell-walls; (*c*) the cytoblasts, pale, roundish or oval, disc-like, or more commonly, concavo-convex: some have but the appearance of a nucleolus; in others it is distinct, central, or scattered, or marginal; (*d*) similar cytoblasts surrounded by oval cells; (*e*) four other forms are enumerated by Oesterlen, which are probably only varieties of the preceding.

The preceding statements justify the conclusion drawn by both the authors that these four organs are so similar, that they may be classed as members of one order. To say nothing of their likeness in obvious characters, the essential constituents of all, their peculiar corpuscles, are similar in all, though admitting of distinction in their best marked states. In all, these corpuscles have the relation of cytoblasts or nuclei. In all, some of them are collected in groups of definite, though various form; which groups are in all except the spleen, inclosed in vesicles or tubes formed by delicate membrane, (primary, limitary, or basement membrane), the exterior of which is covered by capillary blood-vessels.

Oesterlen, who cites, besides these analogies, many others of less importance, includes, in the same class with these four organs, the lymphatic glands, the pineal and pituitary glands, the choroidal gland in the eye of fish, and the greenish gland on each side of the gastric sac in crabs, and suspects that many structures in invertebrata will be brought into the same class. In the lymphatic glands he finds, besides those of the lymph, a variety of corpuscles like those in the renal capsules. They may be generally distinguished into two kinds; viz., fat cells and molecules, as in the renal capsules, and the cytoblasts [which, doubtless, are the same as those composing the layer of granular matter described by Mr. Goodsir]. These cytoblasts are various, but the majority cannot be distinguished in any way from the cytoblasts of the thymus. Moreover, Oesterlen says that he has found, in an inguinal gland of a fœtal calf, gland-vesicles like those of the thymus, parotid, &c., formed by transparent membrane, with traces of capillary network on it, and full of cytoblasts, with which fat-molecules and fat-cells are mingled.

Oesterlen describes a very similar structure in the pituitary gland: and he says the pineal gland is similarly composed.

But to know the number of organs which may be included in this class or family, is less important than to determine to what other class of organs they are in nearest relation. It is clear that (as many have believed, but none have proved), they are, in all essential characters, *glands*, and that the name of either vascular glands, or glands without ducts, is appropriate to them. Their chief analogies to the true glands are seen in (*a*) the constant possession of cytoblasts, or nuclei, analogous to the nuclei of the true gland-cells; for even in the latter it is probable that the nuclei, rather than the cells, are the most active and essential apparatus; (*b*) the general existence of the structureless (limitary) membrane inclosing the cytoblasts, as the membrana propria of the true glands incloses the secreting cells (the spleen in which it is absent having perhaps its analogue in the liver among the true glands); (*c*) the arrangement

of the capillary vessels on the exterior of the vesicles or other collections of cytoblasts in the glands without, just as in those with, ducts. Thus, the glands without ducts possess all the apparatus which, in the true glands, is provided for secretion. They differ from them in that the formation of cells around their cytoblasts is exceptional, and that their secretion is poured into closed cavities, not into open canals.

Assuming then, that the common occupation of these organs consists in withdrawing from the blood some material which they may (probably after some elaboration) discharge into it again,—the next question is,—What is more particularly the function of each of these glands? In answer only one thing is proved; Mr. Simon's observations prove that in the hybernating animals the thymus forms in itself a store of fat, to be consumed in the maintenance of the temperature during hybernation; and this is more than ever yet was proved of any of these glands. His and former observations also render it highly probable that in other than the hybernant animals, the thymus, during all its temporary existence, is occupied in sequestrating some material from the blood to be restored to it again, in the same or (more probably) some other form. He believes that this material is always such as may be consumed in the service of respiration, "the thymus gland fulfilling its use as a sinking fund in the service of respiration." [But the evidence appears to me insufficient for this conclusion, or even opposed to it; for when in hybernation the gland performs this function, and performs it in the highest degree, it is temporarily adapted for it, not by temporary development, but, as all the analogies of the formation of fat in other cases show, by temporary degeneration. The formation of an extraordinary quantity of fat in any part expresses a defective nutrition of that part; and when the thymus of hybernants accumulates fat at the approach of their winter sleep, it is probably rather because some process in general nutrition to which it before ministered is ceasing, than because it is now about to discharge in an extraordinary degree its ordinary function. I should regard the fatty degeneration or fatty atrophy of the thymus at the approach of each winter-sleep as an annual recurrence of a process analogous to that atrophy by diminution or total removal of substance which takes place once for all in the animals in which the thymus is not persistent. In each case the atrophy is an indication that the necessity for the ordinary acts of the thymus has ceased; but in the hybernants it is for new circumstances made to minister to a new purpose, till, at the cessation of the winter-sleep, and the recommencement of new growth, it begins again to be truly developed, and to form the more highly organic azotized compounds which it may restore to the blood for the nutrition of the fresh-growing tissues.]

For the thyroid gland, Mr. Simon believes (chiefly on the evidence of its comparative anatomy), that it supplies, in its simplest state, a vascular diverticulum to the stream of the cerebral circulation, and that in its higher development, its secretion bears some essential relation to the nutrition of the brain, such that, for instance, while the brain is at rest it may be separating from the blood the same materials as the brain in action takes from the blood.

In like manner he holds the spleen to be as a diverticulum to the systematic circulation when the vessels are filled after taking food; and, by the secretion of the Malpighian corpuscles, an organ in which nutritive matter may be stored up till the system needs it. And, lastly, he thinks the renal capsules may have with the generative system some such relation in alternating secretion as he supposes the thyroid gland to have with the brain.

In the place of these several theories, Oesterlen, in a long discussion, enunciates but one, and that not a new one; namely, that the acts of the glands without ducts are the taking of fluid from the blood, from which as a cytoblastema their cytoblasts are formed; and that these, after their completed development, liquefy and restore to the blood a material more fitted for nutrition than that which it gave for them. [And indefinite and incomplete as this theory is, I must confess it appears to me to express all that can as yet be considered very probable in the general physiology of the glands without ducts.]

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